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by

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Abstract

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This dissertation studies topical questions in empirical macroeconomics. What is the long-term aggregate effect of public investments targeting distressed regions? What was the role of demand factors in driving the increase in inflation after the COVID-19 pandemic? What is the impact of balanced budget requirements on local economic activity? From a methodological standpoint, I employ rigorous causal inference techniques borrowed from applied microeconomics to discipline macroeconomic models and derive consistent aggregate results.

In the first chapter, I study the long-term, aggregate effects of regional development programs on industrial production. Between 1950 and 1992, Italy implemented one of the largest regional development programs in history to foster industrialization in its Southern regions. Exploiting three distinct identification strategies, I estimate that the big push substantially increased local manufacturing activity, with gains persisting up to 2011. At the same time, the program shifted production across regions, limiting labor reallocation from the lagging South to the industrialized Center-North. To account for crowding-out effects, I develop a multi-region growth model with public capital and factor mobility, allowing for increasing returns to scale through regional agglomeration economies. Calibrating the model to match my reduced-form estimates, I find that, despite large crowding-out effects, the program induced gains in national industrial production that outweighed its costs. These results document that big push programs can promote cost-effective structural change in distressed regions, but general equilibrium effects substantially mitigate their impact on aggregate output.

In the second chapter, co-authored with Giulia Gitti, we estimate the slope of the Phillips curve before, during, and after COVID. To do so, we exploit panel variation in inflation and unemployment dynamics across US metropolitan statistical areas (MSAs), using a shift-share instrument to isolate demand-driven fluctuations in local unemployment rates. We specify a two-region New-Keynesian model to derive the slope of the aggregate Phillips curve from our MSA-level estimates. We find that the slope of the Phillips curve dropped to zero during the

pandemic and more than tripled, relative to the pre-COVID era, from March 2021 onward, reaching its highest level since the mid-1970s. These estimates allow us to quantify the extent to which US post-pandemic inflation is propelled by demand factors. Demand-driven economic recovery explains around 1.4 out of the 5.6 percentage-point increase in all-items inflation observed from March 2021 to September 2022. Had the slope of the Phillips curve not steepened after COVID, the demand contribution to the rise in inflation would have been small and statistically insignificant.

In the third chapter, co-authored with Francesco Filippucci and Simone Valle, we estimate short-term income multipliers stemming from budget balance requirements (BBRs). Fiscal consolidation programs often entail BBRs imposed by central governments on local governments. However, little is known about the effects of BBRs on economic activity, as most quasi-experimental estimates of local fiscal multipliers stem from windfall expansionary shocks. This paper studies the 2013 extension of a BBR to Italian municipalities below 5,000 residents. Tighter rules pushed local governments to increase their net budget surplus by 1% of local income. Treated municipalities cut capital expenditures, rather than decreasing current expenditures or raising taxes. The estimated multiplier is not statistically different from zero and is significantly lower than 1.5, the prevailing estimate in the literature.

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Contents

Contents **iv**

List of Figures **vi**

List of Tables **viii**

1 How Big Is the Big Push? The Macroeconomic Effects of a Large-Scale Regional Development Program **1**

1.1 Introduction 1
1.2 Historical Background 6
1.3 The Local Effects of the Regional Big Push 8
1.4 Model 18
1.5 CasMez's Impact on the Regional and National Economies 24
1.6 Conclusion 30

2 Inflation Since COVID: Demand or Supply **62**

2.1 Introduction 62
2.2 Vertical Supply Chains and the Phillips Curve 66
2.3 Data and Descriptive Evidence 70
2.4 Empirical Strategy 71
2.5 Results and Aggregate Implications 74
2.6 Robustness Checks 77
2.7 Conclusion 79

3 Balanced Budget Requirements and Local Austerity Multipliers **89**

3.1 Introduction 89
3.2 Institutional Setting 91
3.3 Data and Identification 93
3.4 Results 96
3.5 Conclusion 100

A Appendix of How Big Is the Big Push? The Macroeconomic Effects of a Large-Scale Regional Development Program 107

A.1 Raw Data . . . 107
A.2 Additional Reduced-Form Results . . . 108
A.3 Alternative Allocation: Place-Blind Program . . . 111
A.4 Appendix A: Figures and Tables . . . 113

B Appendix of Inflation Since COVID: Demand or Supply 126

B.1 Model . . . 126
B.2 Interpolation . . . 131
B.3 Empirical Strategy: Model-Based Impulse Response Functions . . . 132
B.4 Appendix B: Figures and Tables . . . 133

C Appendix of Balanced Budget Requirements and Local Austerity Multipliers 145

C.1 Appendix C: Figures and Tables . . . 146

Bibliography 164

List of Figures

1.1 Center-North vs. South per-capita GDP Ratio . . . 32
1.2 South → Center-North Net Outmigration Rates, by Decade . . . 33
1.3 CasMez's Jurisdiction . . . 34
1.4 Time Series of CasMez's Investments, by Type . . . 35
1.5 Time Series of CasMez's Infrastructure Investments, by Type . . . 36
1.6 Early vs. Late Industrial Development Areas (IDAs) . . . 37
1.7 Early IDAs vs. Late IDAs - First Stage . . . 38
1.8 Early IDAs vs. Late IDAs - Reduced Form . . . 39
1.9 IDA Municipalities vs. Matched Non-IDA Municipalities . . . 40
1.10 1-to-1 Match - First Stage . . . 41
1.11 1-to-1 Match - Reduced Form . . . 42
1.12 Municipalities North vs. South of CasMez's Jurisdiction Border . . . 43
1.13 Static Long Difference-in-Discontinuities (1991) . . . 44
1.14 Dynamic Long Difference-in-Discontinuities - First Stage . . . 45
1.15 Dynamic Long Difference-in-Discontinuities - Reduced Form . . . 46
1.16 Industrial Production: Data vs. Counterfactual (No CasMez) . . . 47
1.17 PDV of Industrial Production Gains and CasMez's Spending . . . 48
1.18 MFG Employment: Data vs. Counterfactual (No CasMez) . . . 49
1.19 Center-North vs. South Manufacturing Labor Productivity Ratio . . . 50

2.1 The Phillips Correlation Across US Cities . . . 81
2.2 Unemployment and Inflation Across US Cities . . . 82
2.3 Estimates of ψ from column (4) of Table 2.2 . . . 83
2.4 12-month Inflation Rate, Change relative to March 2021 . . . 84

3.1 Share of Total EU27 Government Expenditures Covered by BBRs . . . 102
3.2 Dynamic Effect of DSP Extension on Net Budget Surplus and Per-Capita Income 103

A.1 Flumendosa Dam (1952-1958) . . . 113
A.2 Picture of Flumendosa Dam (Sardinia, 1959) . . . 114
A.3 Raw Data: Grants to Alfa Romeo in Pomigliano d'Arco (1967) . . . 115
A.4 Picture of Alfa Romeo Avio Establishment (Pomigliano d'Arco, 1969) . . . 116
A.5 Picture of Stellantis Establishment (Pomigliano d'Arco, 2022) . . . 116

A.6 Early IDAs vs. Late IDAs - First Stage Composition 117
A.7 1-to-1 Match - Decade-Specific Investments Composition 118
A.8 Dynamic Long Difference-in-Discontinuities - First Stage Composition 119
A.9 1-to-1 Match - First Stage 120
A.10 1-to-1 Match - Reduced Form 121
A.11 Reduced-Form Coefficients - Share of College-Educated Population 122
A.12 MFG Output: Place-Blind Program vs. No CasMez 123
A.13 PDV of Industrial Production Gains and Place-Blind Program's Spending . . . 124

B.1 Real Personal Consumption Expenditures (Jan 2018-Sep 2022) 133
B.2 Inflation Rate, Unemployment Rate, and Efficient Unemployment Rate (Jan 2018-Sep 2022) 134
B.3 Real Consumption Expenditures and Inflation, Goods vs. Services (Jan 2018-Sep 2022) 135
B.4 Original vs. Interpolated CPI series, Chicago and Los Angeles (Feb 2020-Sep 2022) 136
B.5 Inflation Interpolation Error Distribution and Correlation with z^x_{it} 137
B.6 Impulse Response Functions to Intermediate Sector Productivity Shock 138

C.1 Stability in Match Between Balance-Sheet Variables in the Pre-2015 Model (CCOU) and Post-2015 Model (CCOX) 146
C.2 Treated and Control Municipalities 147
C.3 Dynamic Effects of DSP on Net Budget Surplus and Local Income (Including Sardinia and Sicily) 148
C.4 Dynamic Effects of DSP on Municipal Borrowing 149
C.5 Dynamic Effects of DSP on Capital Expenditures 150
C.6 Ring around the municipality of Crescentino 151
C.7 Dynamic Effects of DSP on Neighborhood Surplus and Spillovers on Local Income 152

List of Tables

1.1 CasMez Endowment Over Time . . . 51
1.2 IDA Approvals Over Time . . . 52
1.3 1-to-1 Matching Balance Table . . . 54
1.4 Effect of €1,000 Investments Per Capita - 2SLS Estimates . . . 55
1.5 Cost Per Job and Manufacturing Job Multiplier . . . 56
1.6 Effect of €1,000 Investments Per Capita - 2SLS Estimates . . . 57
1.7 Effect of €1,000 Investments Per Capita - 2SLS Estimates . . . 58
1.8 IV Estimates of Agglomeration Elasticities . . . 59
1.9 Structural Parameters and Measured Quantities . . . 60
1.10 Contributions to Crowding-Out Effects . . . 61

2.1 First Stage Coefficients . . . 85
2.2 Estimates of ψ from Equation (2.5) . . . 86
2.3 IV Estimates of ψ from Equation (2.5) for different post-COVID periods . . . 87
2.4 IV Estimates of ψ from Equation (2.5) for broad CPI categories . . . 88

3.1 Effect of DSP on Surplus and Local Income . . . 104
3.2 Composition of the Municipal Budget Shock Induced by DSP Extension . . . 105
3.3 Effect on Neighborhood Surplus and Spillovers on Local Income . . . 106

A.1 IV Estimates of Cross-Sectoral Agglomeration Elasticities . . . 125

B.1 Descriptive Statistics . . . 139
B.2 IV Estimates of ψ from Equation (2.5) with different inflation measures . . . 140
B.3 IV Estimates of ψ from Equation (2.5) with v_{it} (V/U) measuring labor market tightness . . . 141
B.4 IV Estimates of ψ from Equation (2.5) for different pre-COVID periods . . . 142
B.5 IV Estimates of ψ from Equation (2.5) with proxy of local inflation expectations 143
B.6 IV Estimates of ψ from Equation (2.5) with more stringent MSA-time FE . . . 144

C.1 Evolution of DSP Rules for Italian Municipalities . . . 153
C.2 Descriptive Statistics (Full Sample) . . . 154
C.3 Descriptive Statistics (Selected Sample) . . . 155
C.4 Effect of DSP on Surplus and Local Income (Different Bandwidth) . . . 156

C.5 Effect of DSP on Surplus and Local Income (Restricted Sample) 157
C.6 Effect of DSP on Surplus and Local Income (Including Sardinia and Sicily) . . . 158
C.7 Composition of the Municipal Budget Shock Induced by DSP Extension (Restricted Panel) 159
C.8 Composition of the Change in Expenditures Induced by DSP Extension 160
C.9 Effect of DSP on Neighborhood Surplus and Spillover on Local Income (Upper Panel: Municipalities 5,000-20,000 residents; Lower Panel: 5,000-10,000 residents) 161
C.10 Effect of DSP on Neighborhood Surplus and Spillover on Local Income (Municipalities 5,000-15,000 residents) 162
C.11 Effect of the DSP Extension on Neighborhood Surplus and Spillovers on Local Income (Including Sardinia and Sicily) 163

Chapter 1

How Big Is the Big Push? The Macroeconomic Effects of a Large-Scale Regional Development Program

1.1 Introduction

Many countries exhibit remarkable differences in GDP per capita across regions. In 2022, real GDP per capita in the state of New York, the most productive U.S. state, was 2.2 times higher than in Mississippi, the least productive one. Regional disparities are even more pronounced in Italy, the context of this study. In 2022, GDP per capita in Trentino-Alto Adige was 2.5 times higher than in Calabria. Governments often address such disparities through large-scale regional development programs targeting disadvantaged areas. These programs usually combine infrastructure spending and firm grants with a twofold objective. First, they aim to encourage convergence by narrowing regional productivity gaps. Second, they intend to foster national long-term economic growth by channeling investments in relatively underdeveloped areas, where returns to public capital might be higher, as hypothesized by the *big push* theory (Rosenstein-Rodan 1943; Murphy et al. 1989).

However, the effects of these policies are, in principle, ambiguous. First, encouraging economic activity in low-productivity places may be inefficient if place-specific factors prevent them from generating agglomeration economies. Moreover, these policies may induce distortions that may dampen their potentially positive impacts. For instance, crowding out of production factors from the more productive regions may mitigate the gains accruing to distressed regions. Notably, the empirical evidence assessing this ambiguity is scarce because the treatment assignment is non-random – thus making it difficult to isolate the effects of the program from other unobservable drivers of place-level outcomes – and the estimation of long-run effects – particularly relevant to assess the desirability of these policies – is challenging, due to the scant availability of high-quality data spanning several decades.

I overcome these challenges by studying the long-term local and national effects of one of the most extensive regional big push programs in history: the Italian *Cassa per il Mezzogiorno* (CasMez). From 1950 to 1992, CasMez devoted an extraordinary amount of resources to foster the industrialization of Southern Italian regions, whose economies considerably diverged from the Center-North ones since unification in 1861. Specifically, CasMez provided Southern Italian regions with infrastructures (e.g., systems for water and electricity provision, roads, ports, etc.) and offered firms incentives to locate in the South.

Two reasons make CasMez a particularly suited context to examine the general equilibrium consequences of large-scale regional development programs. First of all, the size of the program is unprecedented. According to administrative sources, a total of 450 billion US$ (2010) was devoted to this massive industrialization effort, corresponding to an average of 1% of national GDP per year for more than 40 years (Felice and Lepore, 2017) and about 6 times the GDP of Southern Italian regions in 1950. In absolute terms, the program was 17 times larger than the Tennessee Valley Authority, the most extensive regional development program ever implemented in the U.S. (Kline and Moretti, 2014), 12 times larger than what Germany spent to foster convergence of its Eastern regions after unification (Siegloch et al., 2021), and 3.5 times larger than the Marshall Plan, whose target was the whole Western European territory (Bianchi and Giorcelli, 2023). Second, the diverging economic conditions of the targeted and the non-targeted areas at the time were pushing millions of individuals to move from the lagging South to the industrialized Center-North. Therefore, it is reasonable to hypothesize that such a large-scale, geographically targeted industrialization effort had non-negligible spillover effects on the rest of the country.

My analysis proceeds in three steps. First, I combine administrative data from historical archives covering the universe of CasMez-financed projects and decennial census data geo-localized at the municipality level to provide reduced-form evidence of the impact of CasMez's investments on municipal economies up to 2011. I propose three distinct identification strategies. The first two strategies consist of difference-in-differences designs leveraging variation in the allocation of funds across municipalities within CasMez's jurisdiction stemming from the establishment of *Industrial Development Areas* (IDAs).[1] Specifically, the first strategy compares municipalities belonging to early IDAs (i.e., formed between 1960 and 1965) with municipalities belonging to late IDAs (i.e., formed between 1966 and 1974), while the second compares each municipality belonging to an IDA with another Southern municipality not belonging to an IDA, matched according to baseline characteristics and pre-treatment trends. The third strategy exploits differences across municipalities induced by their location, just North or South of CasMez's jurisdiction border (Albanese et al., 2023). For all

[1] IDAs consist of agglomerates of municipalities (i.e., *Aree di Sviluppo Industriale* or *Nuclei di Industrializzazione*) that become eligible for extra manufacturing-oriented investments upon formation. 48 IDAs, corresponding to the major urban areas of the South, were formed over the 1960-1974 period.

empirical strategies, identification requires the parallel trends assumption to hold. I assess its validity by testing the significance of pre-treatment coefficients.

Consistently across the three empirical strategies, I find that CasMez's funds substantially increased manufacturing employment, total employment, population, and employment rates in the targeted municipalities, with gains persisting up to 20 years after the end of the program. Nevertheless, I estimate a high cost per job created at the municipal level, about 6 to 12 times the 2010 Southern GDP per capita, depending on the empirical strategy. Moreover, substantial effects on the resident population suggest the presence of sizeable crowding-out effects on non-targeted municipalities.

To account for crowding-out effects, I extend the reduced-form analysis to a more aggregated geographical level, exploiting province-level variation in investments induced by the first of my three identification strategies. Using administrative data on province-to-province migration flows, I document that province-level population gains stem from lower out-migration rates (both to the rest of the South and the Center-North). This implies that the program shifted national production across provinces, thus limiting the ongoing mass migratory waves from the South to the Center-North. Therefore, a general equilibrium model is needed to account for both the direct effects on the targeted areas and the indirect effects on the rest of the country induced by factor mobility.

To assess the aggregate effects of the regional big push, I develop a one-sector multi-region growth model that builds on typical features of both the growth (Solow 1956; Swan 1956) and economic geography (Roback 1982; Kleinman et al. 2023) literature. In my model, regional manufacturing production depends on regional productivity, labor, capital, and a fixed factor. I allow for increasing returns to scale by modeling regional productivity as a function of "public capital" (i.e., the cumulative investments in infrastructures and firm grants) and employment density. This second channel captures the idea that the effects of temporary public investments might be highly persistent due to agglomeration economies (Marshall 1890; Ciccone and Hall 1993; Glaeser and Gottlieb 2008; Kline and Moretti 2014). Notably, the model accounts for cross-regional crowding-out effects due to factor reallocation, as labor and private capital are assumed to be mobile across regions (Blanchard and Katz, 1992).

I use steady-state approximations of the model to derive closed-form expressions for the local and aggregate effects on industrial production of a region-specific change in the public capital stock. With perfectly mobile private capital, the impact of a regional big push on national manufacturing output is larger the lower the initial endowment of public capital in the targeted region due to decreasing returns to regional public capital. Moreover, the effect is larger the more elastic the aggregate labor supply to the manufacturing sector, the lower the cross-regional labor mobility, and the lower the difference in output per worker and agglomeration forces between the non-targeted and the targeted regions. This follows from the intuition that reallocating the marginal worker from a high-productivity to a low-productivity region decreases aggregate output per worker. The model's structural parameters are derived by combining standard calibration techniques with the causally identified parameters estimated in my reduced-form analysis.

The model-based analysis reveals that CasMez's investments increased national manufacturing output by an average of 2.7% per year over the 1951-2011 period. This average masks substantial regional heterogeneity. Relative to the counterfactual, industrial production in the South increased by an average of 35.7% per year, while it decreased by an average of 2.6% per year in the rest of the country. Consistently, the ratio between the net present value of industrial production gains in the South induced by CasMez's investments and CasMez's spending over the 1951-2011 period (i.e., the *long-run regional multiplier*) is 2.2, while the same statistic for the whole country (i.e., the *long-run aggregate multiplier*) is 1.3. Therefore, the program is cost-effective in the long run, although the aggregate industrial production gains are 41% lower than the regional ones.

According to my calibration exercise, the regional big push explains a small fraction of the reduction in the Center-North vs. South manufacturing labor productivity gap observed between 1951 and 2011. In fact, the ratio between Center-North vs. South manufacturing output per worker, which decreased from 1.5 to 1.19 in the 1951-2011 period, would have decreased to 1.24 in the absence of CasMez's activity. This result implies that more than 80% of the South vs. Center-North convergence in manufacturing labor productivity would have occurred even without the program, arguably through more pronounced factor reallocation and diminishing returns. Intuitively, fewer investments devoted to the industrialization of the South would have triggered larger migration flows to the Center-North. In turn, larger migration flows would have exacerbated the crowding of the fixed factor of production in the Center-North, causing regional labor productivity to fall. The reverse dynamic would have occurred in the South, with sizeable mitigating effects on the Center-North vs. South manufacturing labor productivity gap.

Two counterfactual exercises allow me to quantify the contribution of regional differentials in fundamentals to the size of crowding-out effects and distinguish between *cost-effectiveness* and *optimality* of the program. In the first exercise, removing regional differentials in manufacturing employment rate, output per worker, and agglomeration forces causes the long-run aggregate multiplier to rise to 1.9. This result implies that regional development programs are substantially less effective in spurring national long-term growth precisely in the presence of those large regional differentials that often motivate them. In the second exercise, I simulate a government spending program of the same size as CasMez but not directed to any specific region of the country (i.e., a *place-blind* program). Even under conservative assumptions about the structural parameters governing the direct effect of investments on productivity in the Center-North, I find that such a program would have had larger long-term effects on national industrial production.

Overall, both the reduced-form and structural analyses suggest that the big push generated substantial long-term gains for distressed areas in the South that persisted even two decades after the end of the program. These positive effects are partially dampened by negative spillovers on the highly productive regions of the Center-North. Taken together, these results document that big push programs can promote cost-effective structural change in distressed regions, but general equilibrium effects substantially mitigate their impact on aggregate output and regional convergence.

This paper builds on and contributes to four strands of the literature. First, I contribute to the literature on big push programs and economic growth by studying one of the largest government-financed industrialization efforts of the past century. In doing so, I account for general equilibrium effects induced by cross-regional reallocation of economic activity and heterogeneous agglomeration economies. The big push literature dates back to Rosenstein-Rodan (1943) and Hirschman (1958). In the 1990s and 2000s, the main view was that industrial policy would harm developing economies through increased resource misallocation (Krueger 1990; Rodrik 2006). Liu (2019) challenges this view by providing theoretical basis for the positive impact of industrial policies in 1970s South Korea and modern-day China. Juhász et al. (2023) provide the most updated and comprehensive review of recent papers on industrial policy. Among them, studies focusing on the long-term effects of different industrial policies in South Korea (Kim et al. 2021; Choi and Levchenko 2021; Lane 2022) and the U.S. (Kantor and Whalley, 2023) tend to find positive partial and general equilibrium effects.[2] My study emphasizes that increased cross-regional factor misallocation mitigates the aggregate output gains from industrial policies, especially in contexts characterized by marked regional labor productivity differentials, such as post-WWII Italy.

Second, my work contributes to the broad literature on place-based policies by estimating the long-term general equilibrium effects of combined infrastructure spending and firm grants. My study most closely relates to Kline and Moretti (2014), who estimate the aggregate welfare gains induced by the Tennessee Valley Authority (TVA) in the U.S. In contemporaneous work, Atalay et al. (2023) quantify the general equilibrium effects of a place-based industrial policy implemented in Turkey in 2012 using the framework developed by Caliendo et al. (2019). Both their work and mine argue that regional development programs are only modestly successful in reducing spatial labor productivity differentials because of their impact on factor allocation. More broadly, the paper relates to numerous studies analyzing the impact of place-based policies on local economic activity. Neumark and Simpson (2015) provide a comprehensive summary of these studies. Among more recent papers, my study closely relates to Criscuolo et al. (2019), Slattery and Zidar (2020), Siegloch et al. (2021), and Bianchi and Giorcelli (2023).

[2]Kim et al. (2021) represent an exception in this respect, as they find worsened resource allocation within industries-regions in South Korea with null effects at the industry-region level.

Third, I contribute to the long-standing literature on the Italian regional divide (Clough and Livi 1956, Eckaus 1961, Iuzzolino et al. 2011, Felice 2019), the problem of Southern Italy's development (Chenery, 1962), and CasMez's activity (Felice and Lepore, 2017). In a recent paper, Fernández-Villaverde et al. (2023) propose a wedge decomposition of the Center-North vs. South per-capita income gap, arguing that it is mostly explained by differences in intermediate input sector productivity and government transfers. My work complements their analysis by quantifying the extent to which a sizeable and prolonged government transfer designed to increase productivity in the intermediate input sector contributed to reducing the Center-North vs. South labor productivity gap. More broadly, this paper relates to Borgomeo (2018), Buscemi and Romani (2022), and Albanese et al. (2023), who study relevant political economy aspects of the program.[3]

Last, my study contributes to the literature on the aggregate effects of resource misallocation by empirically assessing the extent to which regional differentials in manufacturing employment rate, output per worker, and agglomeration forces amplify the crowding-out effects of regional big push policies induced by factor mobility. Seminal work in this literature includes Restuccia and Rogerson (2008), Hsieh and Klenow (2009), and Midrigan and Xu (2014). Among many other contributions, my work builds on insights provided by Gaubert (2018), Rotemberg (2019), and Hsieh and Moretti (2019).

The paper is structured as follows. Section 1.2 provides a historical background of the Center-North vs. South divide in Italy and describes the institutional context. Section 1.3 discusses the identification strategies and presents the reduced-form results at the municipality and province levels. In Section 1.4, I present the model, while Section 1.5 uses it to calculate the aggregate effects of the program. Section 1.6 concludes.

1.2 Historical Background

This section provides a summary of the historical evolution of the Center-North vs. South divide in Italy and a brief description of CasMez's activity throughout its 42 years of existence.

[3]Borgomeo (2018) and Buscemi and Romani (2022) document that political interests affected resource allocation, finding limited long-run effects induced by this subset of interventions. Exploiting the spatial discontinuity induced by CasMez's border, Albanese et al. (2023) show that higher exposure to government transfers persistently increases demand for redistributive policies.

The Center-North vs. South Divide

The Italian economy has been characterized by a pronounced divide between the Center-North and the South of the country ever since unification in 1861.[4] Figure 1.1 displays the ratio between the Center-North and the South GDP per capita over the 1871-2011 period (Vecchi et al., 2011), both in nominal terms (blue line) and adjusting for regional purchasing power (red line). Starting in the late 1800s, the two regions began diverging markedly, as the economy of the Center-North was industrializing fast, while the Southern economy was primarily trapped in agriculture. Many factors contributed to this rising divergence. The Center-North was geographically closer and better connected to the rapidly expanding European markets. Moreover, the Center-North was characterized by relatively more pro-market institutions encouraging private entrepreneurial initiatives. Between 1891 and 1951, the ratio between the Center-North and the South GDP per capita increased from 1.2 to 2.

In the aftermath of WWII, Italy underwent the so-called *economic miracle*, i.e., about 20 years of significant and sustained development, encouraged by the 1948-1952 U.S. reconstruction aid and consolidated by fast capital accumulation in the following decades. The 1951-1971 period is the only one in Italian history in which the Southern economy converged vis-à-vis the rest of the country. Two important factors contributed to this achievement. First, sizeable per-capita income differentials triggered mass migratory waves from the South to the Center-North. In this period, about 2 million citizens (i.e., more than 10% of the 1951 Southern population) moved their residency from the lagging South to the Northern industrial hubs, providing a relatively cheap workforce to the fast-growing manufacturing and construction sectors.[5] Second, the government undertook an unprecedented effort to bring industrialization to the South through the institution of CasMez.

The convergence process suddenly stopped at the beginning of the 1970s in the face of the international oil crisis. From 1971 to 2011, the ratio between the Center-North and the South GDP per capita has remained relatively constant at 1.6-1.7. During this period, migration flows from the South to the Center-North and capital accumulation started slowing down, thus curbing convergence. A large per-capita income gap persists today, explained mainly by differences in employment rate rather than output per worker.

CasMez and the Extraordinary Intervention

In the aftermath of WWII, the development of Southern regions was an issue of primary importance for Italian policymakers. In 1950, Prime Minister Alcide De Gasperi established

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[4] The Center-North of the country includes the following regions: Valle d'Aosta, Piedmont, Lombardy, Liguria, Veneto, Trentino Alto Adige, Friuli Venezia Giulia, Emilia Romagna, Toscana, Umbria, Marche, and Lazio. The South includes the following regions: Abruzzo, Molise, Campania, Apulia, Basilicata, Calabria, Sicilia, and Sardegna.

[5] Figure 1.2 displays the decade-level South to Center-North net outmigration rate.

CasMez to promote self-sustained economic development in the South. Figure 1.3 shows the territory covered by CasMez's jurisdiction. Over the whole 1950-1992 period, CasMez spent the equivalent of about 6 times the 1950 GDP of Southern regions, with the largest share of expenditures devoted to land improvements, public infrastructures, and firm grants.[6] Figure **??** shows the time series of CasMez's investments in public infrastructures and firm grants, the focus of this paper. The value of these investments accounts for about 62% of CasMez's total endowment over the 1951-1992 period.

During the first decade of its activity, CasMez's expenditures were concentrated in basic public infrastructures and land improvements. Over the whole period of its activity, CasMez financed and executed significant investments in water and electricity provision, roads, waste management, ports, and the prevention of natural calamities.[7] Figure 1.5 shows the amount of resources devoted to each type of public infrastructure. Starting from 1957, the main focus of CasMez's activity shifted from land improvements toward firm grants in an attempt to foster industrialization in the South.[8] Grants could cover installation costs, which included expenses for opening new establishments in the South, expanding existing ones, or purchasing machinery. CasMez's management was technical and independent during this period, and the decision-making process was centralized.

From the 1970s, with the establishment of regional governments, allocating funds and assessing projects increasingly became a prerogative of local bureaucrats. The amount of resources devoted to firm grants, as opposed to public infrastructures, increased dramatically during this period, causing the costs of the regional development program to rise substantially (Buscemi and Romani, 2022). CasMez was suppressed in 1984 and substituted in 1986 by a new entity, named *Agenzia per la Promozione e lo Sviluppo del Mezzogiorno* (AgenSud), with similar goals and endowments. The extraordinary intervention was gradually phased out and officially terminated in 1992.

1.3 The Local Effects of the Regional Big Push

Data Collection

I assemble two panel datasets, at the municipality and province level, combining three data sources. First, to measure local exposure to the regional development program, I collect data on the universe of infrastructure projects and firm grants financed by CasMez from

[6]This corresponds to an average of about 1% of national GDP for more than 40 years (Felice and Lepore, 2017).

[7]A smaller amount of resources was devoted to other infrastructures, including railways and airports, tourism, training programs, schools, hospitals, and sports facilities.

[8]See Law 634/1957 and Law 555/1959.

digitized historical archives, named *Archivi dello Sviluppo Economico Territoriale* (ASET). Importantly, the data provide information regarding each project's timing and cost, specifying the amount financed by CasMez. Appendix A.1 provides a detailed description of the raw data for both infrastructure projects and firm grants.

I geo-localize all infrastructure projects and firm grants at the municipality level to measure the total funds invested in each municipality for every year of CasMez's activity. The source typically provides the location of the project. If missing, I parse the provided description to assign at least one location to each project. Sometimes, multiple locations are affected by the realization of a project. In those cases, I divide the amount of resources assigned to the project among the municipalities involved, according to their population. The year assigned to each project corresponds to the year the project was approved.

I measure municipality-level demographic and labor market outcomes using data from the decennial population Censuses. The main outcome variables include manufacturing, agriculture, total employment, resident population, and employment rate. Other variables in this dataset, such as the share of the illiterate population and the share of manufacturing employment, are useful to control for baseline municipal characteristics that could correlate with the amount of funds received or with the outcome variables.

Finally, to estimate the impact of CasMez's investments on internal migration patterns, I use an updated version of the data in Bonifazi and Heins (2000), measuring yearly province-to-province migration flows. The data cover the 1955-2011 period and are constructed using entries to, and cancellations from, the population registries for changes of residence. From the province-to-province migration matrix, I compute the net migration flows for each province from and to the South and the Center-North.

Early vs. Late Industrial Development Areas

In the late 1950s, the focus of CasMez's investments shifted from land improvements to manufacturing-oriented infrastructures and firm grants to foster industrialization. To this end, Law 634/1957 established that CasMez could cover up to 20% of the expenses incurred by firms for the installation of new establishments within CasMez's jurisdiction and up to 10% of the expenses for purchasing machinery. Moreover, in an attempt to trigger agglomeration economies, the government sought to identify areas within CasMez's jurisdiction that were particularly suited for industrial development. Therefore, the status of *Industrial Development Area* (i.e., IDA) was introduced, defining a consortium of municipalities with the power to propose, execute, and manage additional infrastructure projects to encourage local industrial production. A governmental committee established the necessary criteria to constitute an IDA, and CasMez was authorized to cover up to 50% of the proposed infrastructure projects' costs.

Between 1960 and 1974, 48 IDAs, made of 879 municipalities, were approved, corresponding to most of the major Southern metropolitan areas. Table 1.2 lists all the IDAs, the Presidential Decrees that established their formation, and the year of approval. After the formal approval, IDAs were required to draft a local strategic plan (i.e., *Piano Regolatore*) that typically became fully operational after a minimum of 2 to a maximum of 5 years. My first identification strategy exploits variation in the allocation of funds across municipalities induced by the timing of IDAs' formation. Specifically, I restrict the sample to municipalities belonging to IDAs and compare the dynamics of CasMez's investments and labor market outcomes in municipalities belonging to early-approved IDAs versus municipalities belonging to late-approved IDAs, before and after 1961 (i.e., the closest Census year to 1960, when the first IDA was approved). Figure 1.6 shows the resulting treated and control municipalities located within CasMez's jurisdiction.

The advantage of this approach is that it effectively controls for unobservable characteristics constant across municipalities that determine selection as an IDA. The strategy relies on the intuition that municipalities belonging to an early-approved IDA received more funds throughout CasMez's activity. This occurred for two reasons. First, they became eligible for CasMez's co-financing of infrastructure projects earlier than their counterparts. Second, as a result of these early infrastructure investments, they were more likely to attract firms eligible for subsidies within CasMez's jurisdiction even in the following decades, when control municipalities also became eligible for CasMez's co-financing of infrastructure projects.

More formally, I estimate two-stage least squares (2SLS) coefficients from a dynamic fuzzy difference-in-differences design. The first-stage and reduced-form equations take the following form:

$$Y_{it} = \alpha_i + \delta_{rt} + \sum_{t \neq 1961}^{2011} \beta_t D_{it} + \mathbf{X}'_{i1951}\Gamma_t + \varepsilon_{it}, \tag{1.1}$$

where Y_{it} denotes the outcome variable in municipality i and period t, α_i denotes municipality fixed effects, δ_{rt} controls for region-specific trends, D_{it} is a dummy variable denoting municipalities belonging to IDAs approved between 1960 and 1965 interacted with time dumies, and $\mathbf{X}'_{i1951}$ denotes a vector of 1951 municipal characteristics. These include log population, the share of manufacturing employment, the density of manufacturing employment, and the share of illiterate population, and are interacted with time dummies, Γ_t, to capture heterogeneous trends induced by differences in baseline size, industry mix, agglomeration potential, and education levels across municipalities. Identification requires conditional parallel trends for both the first-stage and reduced-form outcome variables in the pre-treatment period (i.e., between 1951 and 1961). Observations are weighted by 1961 population, and standard errors are clustered at the municipality level.

Figure 1.7 shows the dynamic coefficients of the first-stage regressions. In Panel (a), the outcome variable is decade-specific investments per capita – where investments comprise public infrastructure spending and firm grants divided by the 1961 municipal population – while, in Panel (b), the outcome variable is cumulative investments per capita. There is no evidence of differential pre-trends in investments between the treatment and the control group. As expected, municipalities belonging to early IDAs received more investments per capita in the 1962-1971 and 1972-1981 decades, while the share of municipalities belonging to IDAs was higher for the treatment than for the control group, but not in the 1982-1991 decade, about ten years later the approval of the last IDA. More specifically, the instrument induces an increase of about 6,000 Euros (2010) per capita in cumulative investments over the whole 1950-1992 period.

In response to the investments, manufacturing employment in treated municipalities increased by about 30% relative to control municipalities between 1961 and 1981, as shown by Figure 1.8, Panel (a). Manufacturing employment gains were highly persistent and remained unchanged between 1981 and 2011, almost 20 years after the end of the program in 1992. This persistence following the suppression of CasMez implies that the combination of infrastructure spending and firm grants induced higher manufacturing density and self-sustaining productivity gains in the targeted areas. One way in which the literature rationalizes this finding is through the presence of agglomeration economies (Ciccone and Hall, 1993). Specifically, demand externalities (Rosenstein-Rodan 1943, Murphy et al. 1989), knowledge spillovers (Moretti, 2004), and thick markets (Marshall, 1890), may explain a long-run increase in productivity following a temporary investment.

Manufacturing employment gains and positive spillovers to other sectors drive an increase in total employment of about 20% in treated vs. control municipalities, displayed in Figure 1.8, Panel (c). Importantly, the municipal population also increased as a result of additional investments, though to a lesser extent than total employment. Consequently, Panel (e) shows that employment rates increased by 4 percentage points in treated municipalities relative to control ones. This effect is large, persists up to 2011, and follows the same dynamic of the impact on manufacturing employment. In principle, higher municipal employment rates could result from either reduced slack in the local labor market or movers' higher propensity to work relative to stayers.

Industrial Development Areas vs. Matched Control

My second identification strategy exploits variation in the allocation of funds across municipalities within CasMez jurisdiction induced by the status of Industrial Development Area (IDA). Specifically, I match each municipality belonging to the 48 IDAs with one Southern municipality not belonging to any IDA using a set of 14 baseline characteristics and pre-treatment trends, including municipality-level measures of size, education, agglomeration potential, and industry mix. Table 1.3 shows the list of variables used for the matching procedure and the balance of characteristics between the treatment and the matched control group, while Figure 1.9 shows the map of the two groups.

Relative to the first identification strategy, this approach controls for selection into IDA status through a broader range of observable characteristics. Moreover, I leverage a distinct source of variation, as the treatment group only partially overlaps with the one used for the first empirical strategy, while the control group is different. Formally, I estimate two-stage least squares (2SLS) coefficients from a dynamic fuzzy difference-in-differences design before and after 1961 (i.e., the closest Census year to 1960, when the first IDA was approved). In practice, I restrict the sample to municipalities belonging to IDAs and their 1-to-1 matched counterparts and estimate the following specification:

$$Y_{it} = \alpha_i + \delta_t + \sum_{t \neq 1961}^{2011} \beta_t D_{it} + \varepsilon_{it}, \tag{1.2}$$

where α_i denotes municipality fixed effects, δ_t denotes time fixed effects, and D_{it} is a dummy variable taking value 1 for municipalities belonging to IDAs interacted with time dummies. As in Equation (1.1), Y_{it} denotes per-capita investments in the first-stage specification and employment and demographic outcomes in the reduced-form specification.

Intuitively, municipalities belonging to IDAs should receive more funds than their non-IDA counterparts because of their special status. Figure 1.10 shows the dynamic of decade-specific and cumulative per-capita investments for the treatment and the control group over the 1951-2011 period. Starting from the 1960s, municipalities belonging to IDAs received considerably more funds than their non-IDA counterparts up to the end of the program. Cumulatively, treated municipalities received around €10,000 (2010 Euros) per capita more than control municipalities. Identification requires that no municipality-level time-varying characteristic not included among the ones used to match treated and control municipalities affects both the probability of obtaining the IDA status and the outcomes. Reassuringly, I detect no difference in investments in the 1951-1961 period, suggesting that CasMez's activity was not targeting eventually treated municipalities before the establishment of IDAs.

This alternative approach confirms that CasMez's investments substantially impacted municipal economic activity. Figure 1.11, Panel (a), shows that manufacturing employment increased markedly in the treatment group relative to the control group in the 1961-1991 period, with a gap expanding even after CasMez's suppression. By 2011, manufacturing employment was about 35% higher in municipalities belonging to IDAs. This result aligns with the evidence provided by the first identification strategy, confirming the presence of agglomeration economies that make industrial production and employment gains persistent and self-sustaining.

A within-municipality reallocation of workers from agriculture did not accompany the increase in manufacturing employment. Panel (b) shows that the trajectory of agriculture employment is almost identical between the two groups. Under the impulse of industrial production induced by CasMez's investments, total employment was about 25% higher in treated municipalities by 2011. Municipalities belonging to IDAs experienced similar, though quantitatively smaller, gains in population, which translated into a 1 percentage-point increase in the municipal employment rate in 2011. As in the previous case, gains in municipal employment rates could stem from either reduced slack in the local labor market or movers' higher propensity to work relative to stayers.

Discontinuity at the Border

The last empirical strategy exploits plausibly exogenous variation in public infrastructure investments and grants across municipalities located just South vs. North of the sharp CasMez's jurisdiction border. Municipalities outside CasMez's jurisdiction were not eligible for public infrastructure investments or firm grants. However, geographical proximity to the border may control for numerous unobservable confounders that could correlate both with the probability of being included in CasMez's jurisdiction and the outcomes of interest.

Figure 1.12 shows the border of CasMez's jurisdiction and the municipalities included within a radius of 100 kilometers South and North of the border, which I use to define treated and control municipalities.[9] Some segments of CasMez's border coincided with administrative borders (e.g., regions, provinces, etc.) active at the time or with other historical borders separating the North and the South of Italy during the Nazi occupation or before unification. Importantly, unobserved variation across municipalities determined by alternative historical borders could affect the outcomes of interest through channels different from CasMez's investments (Albanese et al., 2023). For this reason, I estimate the impact of CasMez's investments on the *growth rate* of the outcomes rather than on their *levels*. First differencing with respect to 1951 levels, allows me to control for all those time-invariant municipality-specific characteristics that might correlate with eligibility for the treatment and the outcomes.

Formally, I estimate 2SLS coefficients from a long difference-in-discontinuities design (Grembi et al., 2016) relative to the baseline period, 1951, at the border of CasMez's jurisdiction. The first-stage and reduced-form specifications take the following form:

$$Y_{it} - Y_{i1951} = \sum_{t=1961}^{2011} \left[\delta_t + \beta_t D_{it} + \sum_{j=1}^{3} \eta_{jt} R_i^j + \sum_{j=1}^{3} \gamma_{jt} R_i^j D_i + \mathbf{X}'_{i1951} \Gamma_t \right] + \varepsilon_{it} \tag{1.3}$$

[9]My benchmark specification excludes Rome from the control sample, but results do not change when Rome is included.

where $(Y_{it} - Y_{i1951})$ denotes the long difference of the outcome variable relative to 1951, δ_t captures time fixed effects, D_{it} is a dummy variable taking value 1 if municipality i is located South of the border interacted with time dummies, and β_t are the dynamic coefficients of interest. R_i denotes the running variable (i.e., distance from the border). I control flexibly for the impact of R_i on $(Y_{it} - Y_{i1951})$, using a third-degree polynomial function, whose coefficients are allowed to change for observations located North or South of the border. Finally, I control for a vector of baseline municipal characteristics, $\mathbf{X}_{i1951}$, interacted with time dummies, Γ_t, to absorb heterogeneous trends induced by differences in 1951 size, industry mix, agglomeration potential, and education levels across municipalities that might correlate with the treatment and the outcomes.

To grasp the intuition behind the estimation procedure of the β_t coefficients in each period t, I provide a graphical representation of the static long difference-in-discontinuities in 1991 (i.e., the closest Census year to 1992, when CasMez was suppressed) in Figure 1.13. Panel (a) shows that cumulative CasMez's investments per capita in 1991 jump at CasMez's border from around €20,000 (2010 Euro) to almost zero, providing evidence of a strong first stage. Panel (b) shows that employment between 1951 and 1991 increased by about 50% more in municipalities located just South of the border relative to those located just North.

Figures 1.14 and 1.15 display the coefficient β_t for all outcome variables and years to capture the dynamic effects of CasMez's investments. Municipalities located South of the border received more funds over the 1951-1991 period, with a difference in cumulative investments of about €20,000 (2010 Euro). As a result, even 20 years after the end of the program, manufacturing employment increased 40% more in treated municipalities, as shown by Figure 1.15, Panel (a). Between 1951 and 2011, total employment and population increased 20% more South of the border. Panels (c) and (d) show that employment increased faster in the 1970s and 1980s, with population levels adjusting over time. The resulting employment rate dynamic, displayed in Panel (e), is characterized by substantial gains in the 1970s and 1980s, mitigated by population inflows in the following decades.

Summary of Municipal-Level Results

Municipality-level evidence on the impact of CasMez's investments points to substantial long-term partial equilibrium effects on labor-market outcomes. In particular, the combination of public infrastructure spending and firm grants has a particularly positive impact on manufacturing employment, with gains persisting even after the end of the program and suggesting the presence of robust agglomeration economies. These gains are accompanied by total employment and population gains. Municipal employment rates are positively affected by the program overall, although the strength and dynamics of these effects differ across specifications.

Table 1.4 summarizes the results across the three specifications. Following the 2SLS literature (Angrist et al., 1996), the ratio between the first-stage and the reduced-form coefficients estimates the impact of €1,000 (2010 Euro) per capita of cumulative CasMez's investments on the outcome variable of interest. Specifically, columns (1), (2), and (3) report the coefficients obtained from the static versions of Equations (1.1), (1.2), and (1.3), respectively, where the $\sum_{t\neq 1961}^{2011} \beta_t D_i$ terms are replaced by $(T_t \times D_i)$ and T_t denotes a dummy variable for the post-1961 period.

The first specification delivers the highest manufacturing employment, total employment, and employment rate responses to CasMez's investments. The coefficients displayed in column (1) imply that, over the 1961-2011 period, €1,000 (2010 Euro) worth of additional cumulative investments per capita caused municipal manufacturing employment, total employment, and employment rate to increase, on average, by 5.1%, 2.9%, and 0.72 percentage points, respectively. The impact on manufacturing employment decreases for the second and the third specifications. Columns (2) and (3) show that manufacturing employment increased by 3.1% and 2.4%, respectively, in response to €1,000 (2010 Euro) worth of additional cumulative investments per capita. Interestingly, the estimated coefficient is decreasing in the size of the first stage, suggesting that the magnitude of the effects is decreasing in the amount of cumulative investments per capita.

Simple back-of-the-envelope calculations recover two statistics of interest, namely the number of new jobs stemming from one new manufacturing job and the cost per new job created, for each of the three specifications, which I report in the top panel of Table 1.5. One additional manufacturing job created thanks to CasMez's investments increases employment in other sectors within the same municipality by 0.6-1.2 units, depending on the specifications. Since municipal employment in agriculture is not affected by CasMez's investments (Table 1.4), these new jobs are concentrated in the services sector. These estimates are somewhat lower than the 1.6 reported by Moretti (2010), arguably reflecting the relatively small size of Italian municipalities. The cost per new job created at the municipal level implied by my reduced-form analyses ranges between 6.5 and 12.1 times the 2010 GDP per capita in the South, depending on the specification.[10] Importantly, my analysis shows that municipal employment gains are persistent. Therefore, despite being high, these costs per new job created are still lower than the discounted flows of income gains they generate within the municipality.

Spillover Effects Within and Across Provinces

Three distinct identification strategies document the positive partial equilibrium effects of the big push on municipal manufacturing employment, total employment, population, and

[10] These estimates are consistent with the firm-level estimates of cost per new job created provided by Cingano et al. (2022), who study the impact of a subsequent firm subsidy program in Italy (i.e., Law 488/1992).

employment rates. However, assuming factors are to some extent mobile across municipalities, these gains may come, at least in part, at the expense of other areas. In this section, I extend the reduced-form analysis carried out at the municipal level to a more aggregated geographical level (i.e., provinces) with two objectives.[11] First, I obtain causally identified reduced-form parameters that account for *within-province* spillover effects. Second, I estimate the impact of CasMez's investments on cross-province migration flows, which in turn helps me quantify the *cross-province* spillover effects. In the subsequent sections of the paper, I use these estimates to recover the structural parameters of the model and perform simulations.

The intuition behind the spillover analysis follows Criscuolo et al. (2019) and is the following. If the impact of CasMez's investments on the outcomes of interest is lower at the province level than at the municipal level, then adverse spillover effects on neighboring municipalities (i.e., crowding-out effects) prevail over positive ones (i.e., crowding-in effects) within a province. Otherwise, positive spillovers outweigh the negative ones. In practice, this method tests for the presence of within-province cross-municipal spillover effects and delivers reduced-form estimates og the impact of CasMez's investments that absorb such spillovers.

To identify quasi-exogenous variation in investments across provinces in the South, I rely on province-level variation stemming from my first empirical strategy (i.e., early-approved vs. late-approved IDAs). Specifically, I instrument province-level cumulative investments per capita by an interaction of three variables. First, a dummy variable taking value 1 for provinces comprising early IDAs formed between 1960 and 1965. Second, a dummy variable taking value 1 for all periods after 1961, the closest Census year to 1960, when the first IDA was formed. Finally, I multiply this interaction by the baseline share of the provincial population residing in municipalities belonging to the IDA. Intuitively, provinces with a high population share residing in early-approved IDAs in 1951 should be exposed to more investments when the industrialization effort was implemented.

More formally, I estimate 2SLS coefficients from a static difference-in-differences design where the treatment dosage is increasing in the share of the province-level population residing in an IDA at baseline. The first-stage and reduced-form equations take the following form:

$$Y_{pt} = \alpha_p + \delta_{rt} + \beta(P_p \times D_p \times T_t) + \mathbf{X}'_{p1951}\Gamma_t + \varepsilon_{pt}, \tag{1.4}$$

where Y_{pt} denotes the outcome variable (i.e., decade-specific or cumulative CasMez's investments per capita for the first stage and labor market outcomes for the reduced form) in province p and period t, α_p denotes province fixed effects, δ_{rt} denotes region-specific trends, P_p is the share of the province-level population residing in a municipality that belongs to an IDA, D_p is a dummy variable for provinces comprising an early-approved IDA, and T_t is a

[11] CasMez's jurisdiction counts 38 provinces, made of 62 municipalities each, on average.

dummy variable for the post-1961 period. As in the municipality-level specification, $\mathbf{X}'_{p1951}$ is a vector of baseline characteristics, including log population, the share of manufacturing employment, the manufacturing employment density, and the share of illiterate population, measured at the province level. These control variables interact with time dummies, Γ_t, to control for heterogeneous trends induced by differences in size, industry mix, agglomeration potential, and education levels across provinces. The identifying assumption requires that the fixed effects and control variables included in the specification absorb any variation across municipalities that correlates both with the instrument and the outcome variables.

Table 1.6 reports the 2SLS coefficients estimated at the municipality and province levels, revealing three critical findings. First, the percent effect of €1,000 (2010 Euro) cumulative investments per capita on manufacturing employment is lower at the province level than at the municipality level. This implies that some gains in manufacturing employment experienced at the municipal level crowd out manufacturing jobs in other municipalities within the same province. Second, CasMez's investments have a positive but not significant impact on province-level employment rates. This result suggests that higher employment rates at the municipality level might not result from reduced slack in the local labor market but from movers' higher propensity to work. Finally, total employment and population respond substantially to investments at the provincial level, revealing that crowding-out effects operate across province borders and significantly affect the rest of the country. Table 1.5, bottom panel, reports that one additional manufacturing job in the manufacturing sector induces an increase of 1.4 units of employment in other sectors, namely services, within the same province. This result is in line with estimates for the US reported by Moretti (2004). The cost per new job created at the provincial level is instead very similar to the one estimated at the municipal level.

In the context of post-WWII Italy, characterized by mass migratory waves from the lagging South to the industrialized Center-North, it is reasonable to hypothesize that a large part of province-level population gains induced by the regional big push program could stem from improved net migration flows. To test this hypothesis, I use data on province-to-province migration flows (Bonifazi and Heins, 2000) and estimate the contributions of changes in net migration flows to the rest of the South and to the Center-North to province-level population gains. Table 1.7, column (2), reports that 1.7% out of the 2.3% province-level increase in population induced by a €1,000 (2010 Euro) worth of additional cumulative investments per capita, about 3/4 of the effect, is due to improved domestic net migration flows. Columns (3) and (4) document that 0.9% and 0.8% of the 1.7% gain are due to improved net migration flows with the rest of the South and the Center-North, respectively.

Importantly, I further estimate that the improved net migration flows both with the rest of the South and the Center-North are explained by reduced out-migration rather than increased in-migration. Focusing on the impact on net migration flows to the Center-North, I document that CasMez's investments limited labor reallocation, particularly to the North-

Western provinces of Milan (0.3%) and Turin (0.1%), the two most prominent industrial hubs of the country at the time and home of numerous firms that benefited from CasMez's grants to open new establishments in the South. To the extent that workers move across regions in response to changes in regional labor demand, these results document a shift in production across provinces within the South and from the Center-North to the South of the country. Therefore, to properly assess the national impact of the big push, I develop a general equilibrium model that accounts for these crowding-out effects.

1.4 Model

This section presents a multi-region growth model with public capital, factor mobility, and agglomeration economies. The model's objective is to quantify the long-run effects of a regional development program on national manufacturing output and regional labor productivity gaps, allowing for self-sustaining productivity gains and accounting for crowding-out effects induced by factor reallocation. The framework combines typical features of the growth (Solow 1956; Swan 1956) and economic geography (Roback 1982; Kleinman et al. 2023) literature.

Setup

The model features one sector (i.e., manufacturing) and N regions of endogenous size. In each region, there are three types of agents (i.e., workers, landlords, and a representative firm). Workers are homogeneous, supply one unit of labor inelastically to the representative firms, and choose where to live in every period. They derive their utility from consumption and amenities and are hand-to-mouth consumers. Landlords rent capital to the representative firms but do not supply labor. They maximize their lifetime utility by deciding how much to consume and save/invest in each period. Landlords are assumed to be immobile across regions but can invest in all regions at zero cost (i.e., capital is fully mobile, and the cost of capital is common across regions). The stock of private capital depreciates at a constant rate. The model allows for aggregate accumulation of capital. Its cross-regional allocation is instead determined by the period-by-period evolution of its regional demand curves.

The representative firm in each region produces a homogeneous good tradable at zero cost across regions using labor, capital, and a fixed factor. Productivity depends on a region-specific time-invariant component (e.g., geography, institutions), a time-specific region-invariant component (e.g., aggregate technological progress), regional public capital, and regional employment density (i.e., agglomeration economies). Public funds constituting the big push are exogenously allocated across regions and increase local productivity with diminishing returns. Agglomeration economies might stem from demand externalities, technology spillovers, and thick markets (Marshall 1890; Ciccone and Hall 1993; Glaeser and Gottlieb 2008; Kline and

Moretti 2014), or even local state capacity. The model does not take a particular stance on the sources of agglomeration economies. Instead, it postulates a reduced-form positive relationship between local productivity and employment density to capture them.

Given this framework, a regional big push (i.e., an injection of public capital in one region) increases productivity in the targeted region. This, in turn, increases the local demand for private capital and labor. Private capital accumulation and agglomeration economies amplify the impact of the big push on local production, driving its persistent effect on the regional economy. However, higher local demand for capital and labor triggers crowding-out effects on the non-targeted regions. To approximate the long-run effects of a regional development program, I characterize the impact of changes in regional public capital on aggregate steady-state output, which depends on the direct effects of the big push on the targeted region and the crowding-out effects on the non-targeted regions.

Production

The model features one sector and N regions. A region indexed by i produces a homogeneous good, tradable at zero costs across regions, in any period t according to the following technology:

$$y_{it} = z_{it} k_{it}^{\alpha} F_i^{\beta} \ell_{it}^{1-\alpha-\beta}$$

where y_{it} denotes output, z_{it} denotes regional productivity, k_{it} denotes capital (i.e., buildings, structures, equipment), F_i denotes the fixed factor, and ℓ_{it} denotes labor.

To capture the impact of public investments in infrastructures and firm grants on regional productivity, as well as the persistence of this effect induced by agglomeration economies, I define $\ln(z_{it})$ as follows:

$$\ln(z_{it}) = z_i + \theta_t + \eta \ln(k_{it}^P) + \gamma_i \ln\left(\frac{\ell_{it-1}}{A_i}\right) + \varepsilon_{it},$$

where z_i captures region-specific time-invariant factors affecting productivity, θ_t denotes period-specific productivity shocks common across regions, k_{it}^P denotes public capital in region i and period t, and $\left(\frac{\ell_{it-1}}{A_i}\right)$ denotes employment density.[12] Employment density captures agglomeration economies and is assumed to affect productivity with a period lag. This ensures that the model delivers deterministic predictions in every period and prevents regions from achieving extremely different levels of manufacturing activity by chance in any given period (Krugman, 1991).

The parameter γ_i, the elasticity of productivity to employment density (i.e., the *agglomeration elasticity*), varies across regions to allow for the possibility that returns to scale are

[12] The term A_i denotes region i's area.

higher in regions targeted by the big push, or vice versa. The regularity condition $\beta > \gamma_i$ ensures that there is no equilibrium in which all workers are located in one region only. The intuition behind this condition is that the decreasing returns to the fixed factor act as a form of congestion force for regional production that counteracts agglomeration economies. Specifically, higher ℓ_{it} causes labor productivity in region i to fall because of the crowding of the fixed factor more than to increase thanks to agglomeration economies.

Combining the labor and capital demand equations stemming from profit maximization, I obtain the following expression for regional labor demand:

$$\ell_{it} = \left(\frac{1-\alpha-\beta}{w_{it}}\right)^{\frac{1-\alpha}{\beta}} \left(\frac{\alpha}{r_t}\right)^{\frac{\alpha}{\beta}} z_{it}^{\frac{1}{\beta}} F_i. \tag{1.5}$$

where w_{it} denotes the wage rate in region i and period t and r_t denotes the cost of capital in period t, common across regions. Intuitively, labor demanded in region i and period t is decreasing in the wage rate w_{it} and the cost of capital r_t and increasing in region-specific productivity z_{it} and the fixed factor F_i.

Labor Supply and Equilibrium Employment

Workers' utility in region i is defined as follows:

$$u_{it}^w = a_{it} c_{it}^w$$

where a_{it} denotes amenities and c_{it}^w denotes worker's consumption. Workers supply one unit of labor inelastically and are hand-to-mouth, i.e., they exhaust their budget in each period. Therefore, their indirect utility function can be expressed as follows: $u_{it}^w = a_{it} w_{it}$. Assuming that region-specific amenities are constant (i.e., $a_{it} = a_i$) and workers migrate to equalize utility across locations in any period t (i.e., $u_{it}^w = u_{jt}^w = \bar{u}_t^w$ for all regions i and j), I derive the following expression for the regional labor supply:

$$w_{it} = \frac{\bar{u}_t^w}{a_i} \tag{1.6}$$

Combining Equations (1.5) and (1.6), I obtain an expression for equilibrium employment:

$$\ell_{it} = \left[\frac{(1-\alpha-\beta)a_i}{\bar{u}_t^w}\right]^{\frac{1-\alpha}{\beta}} \left(\frac{\alpha}{r_t}\right)^{\frac{\alpha}{\beta}} z_{it}^{\frac{1}{\beta}} F_i. \tag{1.7}$$

Regional equilibrium employment is increasing in regional amenities, productivity, and fixed factor endowment and decreasing in the cost of capital and the indirect utility of all workers (i.e., wages and amenities in the other regions of the economy). The aggregate labor supply to the manufacturing sector takes the following form:

$$\sum_{i}^{N} \ell_{it} = L_t = L(\bar{u}_t^w).$$

This assumption implies that the aggregate labor supply to the manufacturing sector is not inelastic, and therefore increases in regional manufacturing productivity result in higher aggregate manufacturing employment.

Capital Accumulation

Landlords are geographically immobile and rent capital to representative firms. Capital consists of buildings and structures that are geographically immobile once installed and are assumed to depreciate at the constant rate δ. Landlords' intertemporal utility takes the following form:

$$v_{it}^k = E_t \sum_{s=0}^{\infty} \phi^{t+s} \frac{(c_{it+s}^k)^{1-\psi}}{1-\frac{1}{\psi}}$$

where c_{it}^k denotes landlords' consumption, ϕ the discount factor, and ψ the intertemporal elasticity of substitution. As landlords are assumed to be geographically immobile, amenities are omitted from their intertemporal utility. The intertemporal budget constraint requires that rental flows from the existing stock of capital equal the sum of landlords' consumption and the value of investments, net of depreciation, i.e., $r_t k_{it} = c_{it}^k + k_{it+1} - (1-\delta)k_{it}$.[13] Importantly, the term k_{it} denotes the stock of capital in the hands of landlords located in region i at period t and the cost of capital r_t is not region-specific, as landlords allocate capital to equalize returns across regions. After defining $R_t = r_t + 1 - \delta$, the gross return on capital, the landlords' problem takes the following form:

$$\max_{c_{it+s}^k, k_{it+s+1}} \frac{(c_{it}^k)^{1-\psi}}{1-\frac{1}{\psi}} + \phi E_t v(k_{it+1}, t+1)$$

subject to

$$c_{it}^k + k_{it+1} = R_t k_{it}$$

I follow Kleinman et al. (2023) to show that:

$$c_{it} = \xi_t R_t k_{it}$$

$$k_{it+1} = (1-\xi_t) R_t k_{it} \tag{1.8}$$

[13] I am implicitly assuming that the price of one unit of consumption c_{it}^k is the same as one unit of capital k_{it}.

where ξ_t is defined recursively as follows:

$$\xi_t^{-1} = 1 + \phi^{\psi}(E_t[R_{t+1}^{\frac{\psi-1}{\psi}} \xi_{t+1}^{-\frac{1}{\psi}}])^{\psi} \tag{1.9}$$

This result implies that landlords have a linear saving rate $(1-\xi_t)$ out of current period wealth $R_t k_{it}$. In general, landlords' saving rate $(1-\xi_t)$ is endogenous, forward-looking, and depends on the expectation of the sequence of future returns on capital R_{t+s}, the discount rate ϕ, and the intertemporal elasticity of substitution ψ. In the particular case of log-utility ($\psi = 1$), landlords have a constant saving rate ϕ, as in the Solow (1956) and Swan (1956) models.

Since I use steady-state approximations of the model to derive the long-term effects of the regional development program, I derive closed-form expressions for the steady-state saving rate and cost of capital. Combining Equations (1.8) and (1.9) with the definition of the gross return on capital R_t, I derive that the steady-state saving rate equals the discount rate (i.e., $1-\xi = \phi$) and the following expression for the steady-state cost of capital:

$$r = \frac{1-\phi(1-\delta)}{\phi}.$$

These derivations imply that the steady-state saving rate $(1-\xi)$ and cost of capital r are constant and depend solely on the discount rate, ϕ, and the depreciation rate, δ.

Regional Big Push and Aggregate Output

Combining Equation (1.5) with the expression for the labor share of income and taking logs, I derive the following expression for $\ln(y_{it})$:

$$\ln(y_{it}) = \frac{\alpha}{1-\alpha}\ln(\alpha) + \frac{1}{1-\alpha}\ln(z_{it}) - \frac{\alpha}{1-\alpha}\ln(r_t) + \frac{\beta}{1-\alpha}\ln(F_i) + \frac{1-\alpha-\beta}{1-\alpha}\ln(\ell_{it}).$$

Given the definition of regional productivity, I derive the following expression for region i's steady-state output:

$$\ln(y_i) = C_{1i} + \frac{\eta}{1-\alpha}\ln(k_i^P) + \frac{1-\alpha-\beta+\gamma_i}{1-\alpha}\ln(\ell_i),$$

where C_{1i} is a region-specific exogenous term.[14] Region i's steady-state output is increasing in regional public capital and employment. To evaluate the impact of a change in k_i^P, public capital in region i, on aggregate manufacturing output, Y, I use steady-state approximations of the model. I start by deriving an expression for the impact of a change in k_i^P on region i's steady-state output:

$$\frac{\mathrm{d}y_i}{\mathrm{d}k_i^P} = \frac{1}{1-\alpha}\left[\frac{\eta}{k_i^P}y_i + (1-\alpha-\beta-\gamma_i)\frac{y_i}{\ell_i}\frac{\mathrm{d}\ell_i}{\mathrm{d}k_i^P}\right]. \tag{1.10}$$

[14] See the Appendix for the derivation and the full expression.

The impact of a change in k_i^P on aggregate steady-state output is the sum of Equation (1.10) across all regions in the economy. If output per worker and the agglomeration elasticities are assumed to be constant within two macro-regions (i.e., $y_{iS}/\ell_{iS} = y_{jS}/\ell_{jS}$, $\gamma_{iS} = \gamma_{jS}$, and $\gamma_{iN} = \gamma_{jN}$ for all regions $i \neq j$ within the Center-North and the South of the economy) and the big push is implemented only in one macro-region (i.e., the South), then the expression for the impact of a change in k_i^P on aggregate output Y reduces to the following straightforward expression:

$$\underbrace{\frac{\mathrm{d}Y}{\mathrm{d}k_S^P}}_{\text{Aggregate effect}} = \underbrace{\frac{\eta}{1-\alpha}\frac{y_S}{k_S^P} + \frac{1}{1-\alpha}\frac{\mathrm{d}\ell_S}{\mathrm{d}k_S^P}\left[\frac{y_S}{\ell_S}(1-\alpha-\beta+\gamma_S)\right]}_{\text{Direct effect on targeted region}} + \underbrace{\frac{1}{1-\alpha}\frac{\mathrm{d}\ell_N}{\mathrm{d}k_S^P}\left[\frac{y_N}{\ell_N}(1-\alpha-\beta+\gamma_N)\right]}_{\text{Crowding-out effect on non-targeted region}}. \tag{1.11}$$

This expression decomposes the total impact of a change in k_S^P on aggregate output Y into a direct effect on the targeted region and a crowding-out effect on the non-targeted region.

The direct effect on the targeted region can be further decomposed into a first-order productivity effect and a second-order crowding-in effect. The first-order productivity effect is increasing in the parameter $\eta/(1-\alpha)$ and the inverse of the regional public capital-to-output ratio, i.e., (y_S/k_S^P). The parameter η denotes the elasticity of regional productivity to regional public capital, the parameter $(1-\alpha)$ governs the amplification of the impact of public capital on productivity due to regional private capital accumulation, and the inverse of the public capital-to-output ratio captures diminishing returns to public capital. The second-order crowding-in effect is increasing in the regional employment gains induced by the big push program, $\mathrm{d}\ell_S/\mathrm{d}k_S^P$, the baseline regional output per worker, (y_S/ℓ_S), and the regional agglomeration elasticity, γ_S. The regional employment gains, $(\mathrm{d}\ell_S/\mathrm{d}k_S^P)$, capture the number of individuals working in the Southern manufacturing sector as a result of CasMez's investments that would have otherwise migrated or not worked in the manufacturing sector.

Finally, the crowding-out effect on the non-targeted region is increasing in the regional employment losses induced by the big push program, $(\mathrm{d}\ell_N/\mathrm{d}k_S^P)$, the baseline regional output per worker (y_N/ℓ_N), and the regional agglomeration elasticity, γ_N. The regional employment losses, $(\mathrm{d}\ell_N/\mathrm{d}k_S^P)$, capture the number of manufacturing jobs lost in the Center-North, which correspond to the number of individuals staying in the South as a result of CasMez's investments that would have otherwise migrated to the Center-North and worked in the manufacturing sector.

1.5 CasMez's Impact on the Regional and National Economies

This section quantifies the impact of CasMez's investments on regional and aggregate industrial production and manufacturing employment, as well as on the Center-North vs. South labor productivity gap in manufacturing. I recover the structural parameters of the model by combining calibration techniques with my province-level reduced-form estimates. Then, I discuss the results of the model-based analysis and perform two counterfactual exercises. With the first exercise, I evaluate the role of regional differentials in the manufacturing employment rate, output per worker, and agglomeration elasticities in explaining the program's national impact. With the second exercise, I assess the program's effects under an alternative allocation of resources.

Estimating Agglomeration Elasticities in the Center-North and the South

The first step to quantify the impact of CasMez's investments on national industrial production is to estimate the region-specific agglomeration elasticities, γ_N and γ_S. To do that, I follow the methodology developed by Kline and Moretti (2014). I start by considering Equation (1.7). Taking logs, I derive the following expression for the equilibrium level of employment in region i and period t:

$$\ln(\ell_{it}) = \kappa_i + \delta_t + \frac{\eta}{\beta}\ln(k_{it}^P) + \frac{\gamma_i}{\beta}\ln\left(\frac{\ell_{it-1}}{A_i}\right) + \omega_{it},$$

where κ_i denotes a region-specific constant term, δ_t denotes a time-specific constant term, and $\omega_{it} = (1/\beta)\varepsilon_{it}$. I estimate this structural equation using a two-way fixed effects regression of period-t manufacturing employment on the lag of manufacturing employment density and recover the parameter (γ/β) separately for the Center-North and the South. Conveniently, κ_i is absorbed by unit fixed effects, and δ_t is absorbed by time fixed effects. Formally, I estimate the following specification separately for the Center-North and the South:

$$\ln(\ell_{it}) = \kappa_i + \delta_{rt} + \frac{\gamma}{\beta}\ln\left(\frac{\ell_{it-1}}{A_i}\right) + \mathbf{X}'_{i1951}\Gamma_t + \nu_{it}, \tag{1.12}$$

where ℓ_{it} is manufacturing employment, κ_i denotes unit fixed effects, δ_{rt} controls for region-specific trends, (γ/β) is the coefficient of interest, allowed to be heterogeneous between the two macro-regions N and S. (ℓ_{it-1}/A_i) measures lagged manufacturing employment density, $\mathbf{X}'_{i1951}$ is a vector of baseline characteristics interacted with time dummies, Γ_t, to control for heterogeneous trends induced by differences in size, industry mix, agglomeration potential, education levels, and IDA status across units, and ν_{it} is the error term.

A threat to identification is that the error term $\nu_{it} = (\eta/\beta)\ln(k_{it}^{P}) + (1/\beta)\varepsilon_{it}$ is likely correlated with lagged manufacturing employment density and the outcome of interest. For instance, contemporaneous CasMez's investments are more likely to be channeled in areas characterized by high manufacturing density and affect current manufacturing employment at the same time. In addition, if local productivity is serially correlated (e.g., follows an AR(1) process), the impact of past agglomeration on current employment might be either understated or overstated, depending on the sign of the autocorrelation coefficient.

Some of these concerns should be attenuated by the inclusion of $\mathbf{X}'_{i1951}\Gamma_t$. To the extent that the evolution of CasMez's investments and productivity over time is fully captured by baseline characteristics, including the IDA status, even a simple OLS regression recovers the parameter of interest. However, it could still be the case that local productivity shocks are serially correlated. This would imply that municipalities with higher manufacturing density are characterized by heterogeneous productivity trends that affect manufacturing employment independently of agglomeration economies. To address this concern, I instrument the one-decade lag of manufacturing employment density with its two-decade lag. My instrument is uncorrelated with present and one-decade lag productivity shocks by construction. Therefore, the identifying assumption is that, after conditioning on all the control variables included in Equation (1.12), productivity shocks are independent over a 20-year horizon. This assumption is pretty conservative in light of theories that describe local growth as the result of random productivity processes (Eeckhout, 2004).

Table 1.8 reports the 2SLS estimated coefficients for the Center-North and the South, as well as the difference between the two. Assuming a common β between the two regions, the agglomeration elasticity is about 24% higher in the Center-North than in the South. In contrast to the previous literature, this result empirically documents that the agglomeration elasticities are not constant across regions and, in my context, increasing in regional manufacturing density. A higher agglomeration elasticity in the non-targeted region than the targeted one lowers the aggregate output gains from the regional big push, as it amplifies crowding-out effects relative to crowding-in effects.

Calibration

To quantify the regional and aggregate impact of the regional development program on industrial production, migration flows, and labor productivity differentials over the 1951-2011 period, I use steady-state approximations of the model. Recall Equation (1.11):

$$\frac{\mathrm{d}Y}{\mathrm{d}k_S^P} = \frac{\eta}{k_S^P}\frac{y_S}{1-\alpha} + \frac{1}{1-\alpha}\frac{\mathrm{d}\ell_S}{\mathrm{d}k_S^P}\left[\frac{y_S}{\ell_S}(1-\alpha-\beta+\gamma_S)\right] + \frac{1}{1-\alpha}\frac{\mathrm{d}\ell_N}{\mathrm{d}k_S^P}\left[\frac{y_N}{\ell_N}(1-\alpha-\beta+\gamma_N)\right]$$

To perform this exercise, it is necessary to calibrate the parameters η, α, β, γ_S, and γ_N and measure the quantities $\mathrm{d}k_S^P$, k_S^P, $(\mathrm{d}\ell_S/\mathrm{d}k_S^P)$, $(\mathrm{d}\ell_N/\mathrm{d}k_S^P)$, y_S, y_N, ℓ_S, and ℓ_N. Table 1.9 lists these parameters and quantities and reports the value used for the model-based analysis, the methodology followed to compute them, and the source.

I start by calibrating the capital share of income $\alpha = 0.3$ (Griliches, 1967) and the regional labor supply elasticity $\frac{1-\alpha}{\beta} = 1.5$ (Kline and Moretti, 2014). These values imply that $\beta = 0.47$. Now, I use the estimates of $(\widehat{\gamma/\beta})$ reported in Table 1.8 to recover $\gamma_S = 0.15$ and $\gamma_N = 0.19$. Reassuringly, these estimates are in line with values reported in the literature on agglomeration elasticities. The quantities $\mathrm{d}k_S^P$, y_S, (y_S/ℓ_S), and (y_N/ℓ_N) can be directly measured from original data sources. Specifically, I measure $\mathrm{d}k_S^P$, the time series of CasMez's investments, by aggregating the municipality-level data, and y_S, (y_S/ℓ_S), and (y_N/ℓ_N) from SVIMEZ (2011). This volume reports the time series of industrial production and the number of manufacturing workers starting from 1950 for both the Center-North and the South. I convert all monetary values to 2010 Euros to make them comparable across years.

The parameter η still needs to be recovered, and k_S^P, the time series of Southern public capital stock, cannot be reliably measured. Therefore, η and the k_S^P cannot be separately identified. However, the ratio of the two, (η/k_S^P), is sufficient to perform the model-based analysis and can be obtained by combining estimation and calibration techniques. Specifically, consider the equation for the steady-state regional manufacturing employment:

$$\ln(\ell_i) = C_{2i} + \frac{\eta}{\beta - \gamma_i}\ln(k_i^P),$$

where C_{2i} is a region-specific exogenous term. From the elasticity of steady-state regional manufacturing employment to public capital, I derive a closed-form expression for the parameter of interest (η/k_i^P) as a function of the semi-elasticity of regional manufacturing employment to CasMez's investments:

$$\frac{\partial \ln(\ell_i)}{\partial \ln(k_i^P)} = \frac{\mathrm{d}\ell_i}{\ell_i}\frac{k_i^P}{\mathrm{d}k_i^P} = \frac{\eta}{\beta - \gamma_i} \implies \frac{\eta}{k_i^P} = \frac{\mathrm{d}\ell_i}{\ell_i}\frac{1}{\mathrm{d}k_i^P}(\beta - \gamma_i).$$

Notice that Table 1.6 reports the reduced-form estimate for $\frac{\mathrm{d}\ell_i}{\ell_i}\frac{1}{\mathrm{d}k_i^P} = 0.037$, while Table 1.8 reports the estimate for the agglomeration elasticity $\gamma_S = 0.15$. Combining these two results with the calibration of $\beta = 0.47$, it follows that $(\eta/k_S^P) = 0.037 \times (0.47 - 0.15) = 0.012$.

This derivation is consistent with the intuition that the response of regional employment to public investments is informative about the regional gains in industrial production induced by the program. However, the crowding of the fixed factor and regional agglomeration economies also contribute to the long-run impact of investments on output, and their contribution is accounted for by the crowding-in and crowding-out terms in Equation (1.11). Therefore, to recover (η/k_i^P), the semi-elasticity of regional manufacturing employment to public investments needs to be re-scaled by $(\beta - \gamma_i)$.

Finally, combining estimates in Tables 1.6 and 1.7, I quantify $(\mathrm{d}\ell_S/\mathrm{d}k_S^P)$ and $(\mathrm{d}\ell_N/\mathrm{d}k_S^P)$. Recall that the quantity $(\mathrm{d}\ell_S/\mathrm{d}k_S^P)$ denotes the difference between the total number of manufacturing jobs created thanks to CasMez's investments and the number of manufacturing jobs crowded out within the South, while the quantity $(\mathrm{d}\ell_N/\mathrm{d}k_S^P)$ captures the number of manufacturing jobs crowded out from the Center-North. To obtain the total increase in Southern manufacturing jobs due to CasMez's investments, I calculate the manufacturing job gains implied by the province-level reduced-form parameter 0.037 reported in Table 1.6 and the time series of cumulative CasMez's investments.

To compute manufacturing employment crowded out within the South and in the Center-North, I first calculate the population losses implied by the province-level reduced-form parameters reported in Table 1.7. Specifically, I multiply the parameter 0.008 (0.009) by the baseline Southern population and the time series of CasMez's investments to recover the number of individuals who did not migrate to the Center-North (to other areas of the South) as a result of the program. Then, I assume that those who did not migrate as a result of the cross-regional shift in manufacturing production would have been employed in manufacturing with the same probability of an individual living in their counterfactual destination region. This assumption is verified if the ratio between regional manufacturing employment and population (i.e., the manufacturing employment rate) in the Center-North and in the South did not change in response to CasMez's investments. In practice, I multiply regional population losses due to CasMez's investments by the regional manufacturing employment rate. An implication of this assumption is that one less migrant from the South to the Center-North increases crowding-out effects more than one less migrant to other areas within the South, as the Center-North manufacturing employment rate, output per worker, and agglomeration elasticity are higher than the Southern ones.

Regional and Aggregate Effects

According to the model-based analysis displayed in Figure 1.16, 2011 industrial production in the South was 55% higher than what it would have been without CasMez's activity (i.e., €60.6 billion vs. €38.9 billion). The streams of gains accruing to the South over the 1951-2011 period in terms of increased industrial production translate into an average increase of 35.3% per year relative to the counterfactual. However, crowding-out effects are sizeable. In 2011, industrial production in the Center-North was 4.1% lower than in the counterfactual (i.e., €286.6 billion vs. €295.9 billion). Over the whole 1951-2011 period, the Center-North lost an average of 2.6% per year of industrial production due to CasMez's activity. Summing the direct effects on the targeted region and the crowding-out effects on the non-targeted region, I conclude that industrial production in Italy was 3.7% higher than what it would have been in the absence of CasMez's investments (€347.2 billion vs. €334.9 billion) and it increased by an average of 2.7% per year over the whole 1951-2011 period thanks to Cas-

Mez's activity.

To evaluate the cost-effectiveness of the program, I implement the following methodology. First, I compute the yearly national gains in terms of industrial production. Then, I discount them and the costs associated with CasMez's investments to 1951, applying a real annual discount rate of 3%. Figure 1.17 displays the discounted gains in terms of industrial production for the South (blue bar), the Center-North (red bar), and Italy as a whole (green bar), as well as the discounted CasMez's expenditures (gray bar). I divide the discounted aggregate industrial production gains by the discounted CasMez's expenditures and define the resulting statistic as the *long-run aggregate multiplier* of CasMez's investments. This summarizes the impact of the big push on national industrial production.

I find that the long-run aggregate multiplier of CasMez's investments was 1.3, implying that the gains in terms of aggregate industrial production accrued up to 2011 outweighed the program's costs. However, when performing the same calculation focusing only on the South, I compute a long-run regional multiplier of 2.2. Therefore, the regional big push program had substantially positive long-term effects on the targeted region, as well as sizeable crowding-out effects on the non-targeted region, which lowered by 41% the manufacturing output gains implied by the partial equilibrium analysis. One important caveat of this analysis is that it accounts only for the operating costs of the program, ignoring the potentially conspicuous costs of funds and the overhead costs related to CasMez's activity.

The model-based analysis also allows me to examine the impact of the program on population and manufacturing employment dynamics in the South and in the Center-North. The simulation indicates that, without the program, about 1.6 million additional individuals would have migrated from the South to the Center-North of the country ($>$ 40% relative to the counterfactual), of which about 309,000 would have worked in the manufacturing sector. Figure 1.18 displays the evolution of manufacturing employment in the South, the Center-North, and the whole country with and without CasMez's activity. Interestingly, manufacturing employment levels would now be even lower than the 1951 ones in the South and substantially higher in the Center-North in the absence of CasMez's investments. These regional dynamics reflect the decreased outmigration from the South to the Center-North following CasMez's activity. Overall, the Italian manufacturing sector added around 137,000 jobs relative to the counterfactual.

The dynamics of manufacturing output and employment in the South relative to the Center-North determine the impact of CasMez's investments on the manufacturing labor productivity gap between the two regions. Figure 1.19 shows the time series of the ratio between the Center-North and the South manufacturing output per worker from 1951 to 2011 with and without CasMez's investments. Perhaps surprisingly, more than 80% of the observed convergence in labor productivity would have occurred even in the absence of the program. The intuition behind this result is that, without CasMez's investments, the migration flows from the South to the Center-North would have been considerably larger. In turn, larger migration flows would have exacerbated the crowding of the fixed factor of production in the Center-North, causing regional labor productivity to fall. The reverse dynamic would have occurred in the South, with sizeable mitigating effects on the Center-North vs. South manufacturing labor productivity gap. Atalay et al. (2023) make a similar argument in contemporaneous work studying the impact of a place-based industrial policy in Turkey. In contexts characterized by high factor mobility, regional development programs affect the relative size of the regional manufacturing sectors rather than their relative labor productivity.

Two Counterfactuals

The goal of this subsection is twofold. First, I intend to quantify the contribution of regional differentials in the manufacturing employment rate, output per worker, and agglomeration elasticities in amplifying the crowding-out effects induced by CasMez's investments. Second, I perform a counterfactual exercise to clearly illustrate the distinction between cost-effectiveness and optimality of CasMez's investments. Specifically, I simulate the impact of a program of the same size as CasMez without any regional target (i.e., a *place-blind* program) and show that the implied long-run effects on national industrial production would have been larger.

For the first exercise, I simulate the model removing South vs. Center-North differentials in the manufacturing employment rate, the manufacturing output per worker, and the agglomeration elasticities once at a time, recording the response of the long-run aggregate multiplier to quantify the contribution to each differential to the crowding-out effects. Intuitively, the larger the response of the long-run aggregate multiplier to the removal of one specific regional differential, the larger the contribution of that regional differential to the crowding-out effects. Table 1.10 shows the results of 8 simulations, indicating which parameter or quantity of the Center-North was set at the level of the South for each simulation. The fourth column shows how the long-run aggregate multiplier changes when any given regional differential is removed, with the first row representing the baseline scenario.

The long-run aggregate multiplier is exceptionally responsive to removing regional differentials in the manufacturing employment rate, increasing from its baseline value of 1.3 to 1.8. This results from the crowding-out effects being particularly amplified by the differential incidence of the manufacturing sector in the Center-North relative to the South. Instead, the multiplier is only marginally responsive to the removal of regional differentials in manufacturing output per worker and agglomeration elasticity. When removing all the differentials, the long-run aggregate multiplier increases to 1.9. Therefore, regional differentials in fundamentals reduce the long-run aggregate multiplier by 37% (i.e., from 1.9 to 1.3) and explain 70% of the crowding-out effects induced by CasMez's investments. A natural implication of these findings is that in a context with less marked differentials in regional fundamentals, the regional big push program would have increased aggregate manufacturing output much more substantially.

For the second exercise, I used the structural model to simulate the impact of a program of the same size as CasMez but place-blind (i.e., not targeting the South nor the Center-North specifically). Performing this exercise requires an assumption regarding the direct impact of public investments on the productivity of the Center-North (i.e., η/k_N^P). Decreasing returns of regional productivity to public capital imply that an additional Euro spent in the Center-North, if already endowed with a stock of functioning infrastructures, should increase regional productivity less than in the South. Since I do not observe the evolution of the public capital stock in the Center-North and the South, I assume that the stock of public capital per capita in the Center-North was half the one in the South (i.e., $k_N^P = 2 \times k_S^P$) for the whole 1951-2011 period. Then, $\eta/k_N^P = 1/2 \times \eta/k_S^P$.

The model simulation indicates the long-run aggregate multiplier of such a program would have been 1.7. Comparing the aggregate multiplier with the corresponding 1.3 estimated for CasMez, I conclude that, even under extremely conservative assumptions regarding the relative stock of public capital in the two macro-regions, channeling the same amount of resources equally across regions would have resulted in larger aggregate gains in terms of industrial production. These results emphasize that regional big push programs can be cost-effective but they are unlikely to be optimal.

1.6 Conclusion

Regional disparities in many countries often motivate large-scale regional development programs to foster economic activity in distressed areas. However, the effects of these policies are *ex-ante* ambiguous. Their desirability depends on their costs, the presence of long-term self-sustained productivity gains induced by public investments, and the size of crowding-out effects on the more productive areas of the country.

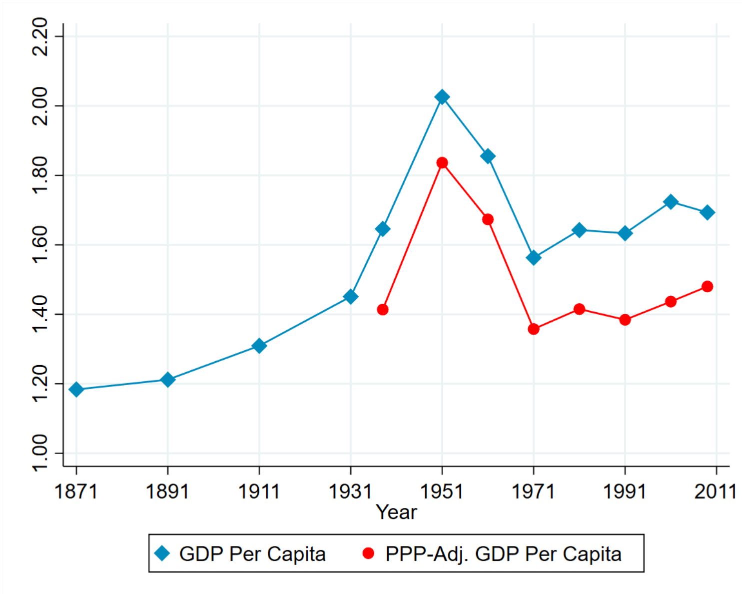
In this paper, I study the regional and aggregate long-term effects of one of the largest regional development programs in history, which devoted around $450 billion (2010 USD) between 1950 and 1992 to fostering the industrialization of the Italian South. To do so, I combine reduced-form evidence consistent across three distinct identification strategies with model-based analysis to account for cross-regional crowding out effects induced by factor mobility. I find that the program substantially boosted manufacturing activity in the South, with productivity gains persisting up to 20 years after the end of the program. I interpret this result as evidence of agglomeration economies in the manufacturing sector.

At the same time, the program diverted industrial production from the highly productive Center-North, thus limiting the ongoing mass migratory waves from the South. In the context of my model, distorting the spatial allocation of capital and labor toward less productive regions induces crowding-out effects. Calibration exercises reveal that these effects were sizeable and reduced the national industrial production gains induced by the program by about 41%. Nevertheless, these gains are positive and larger than its costs. Interestingly, most of the South vs. Center-North convergence in manufacturing output per worker observed between 1951 and 2011 would have occurred even in the absence of the program. This follows the intuition that larger migration flows from the South to the Center-North, rather than productivity-enhancing investments and capital flows, would have spurred convergence in the counterfactual scenario.

In conclusion, I document that regional big push programs can promote structural change in distressed regions, considerably increase the relative size of their economies, and be cost-effective in the long run. In contexts characterized by high factor mobility and large regional productivity differentials, general equilibrium effects substantially mitigate the programs' impact on aggregate output and regional convergence. Importantly, cost-effectiveness does not imply optimality. A counterfactual exercise reveals that if the same amount of resources were invested equally across regions, the impact of the program on national industrial production would have been higher.

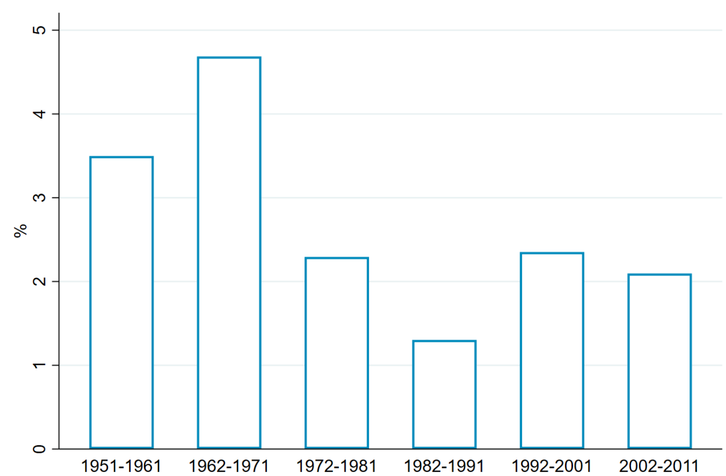
Chapter 1: Figures and Tables

Figure 1.1: Center-North vs. South per-capita GDP Ratio



Notes. The figure shows the time series of the Center-North vs. South per-capita GDP ratio for the 1871-2011 period. A ratio of 1 implies no gap. The light blue line displays the ratio not adjusted for regional PPP, while the red line displays the ratio adjusted by regional purchasing power. Data source: Vecchi et al. (2011).

Figure 1.2: South → Center-North Net Outmigration Rates, by Decade

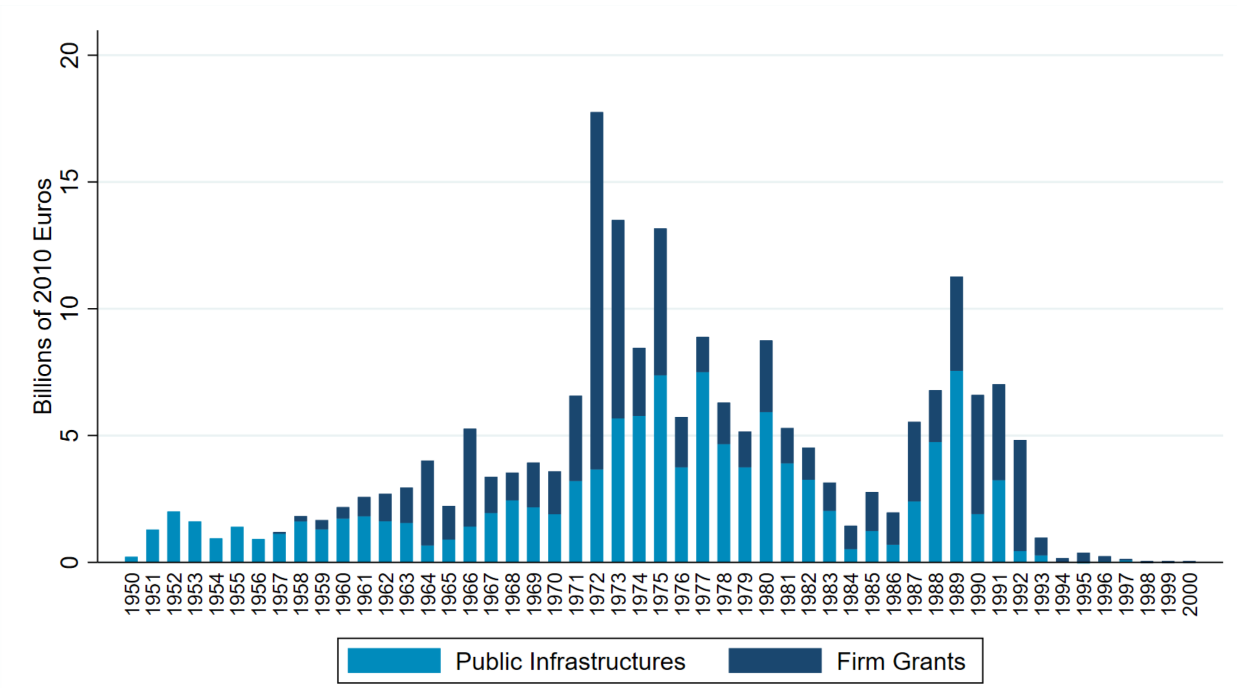


Notes. The figure shows South to Center-North net outmigration rates, by decade. Net outmigration is computed as the difference between the number of individuals moving from the South to the Center-North and the number of individuals moving from the Center-North to the South. It is converted into a rate by dividing this difference by the total Southern population in 1951. The number computed for the 1951-1961 period is obtained by extrapolating to the 1951-1954 period the average annual outmigration rate computed for the period 1955-1961, for which the data are available. Province-to-province data on migration flows come from Bonifazi and Heins (2000), while population data come from the 1951 population Census.

Figure 1.3: CasMez's Jurisdiction

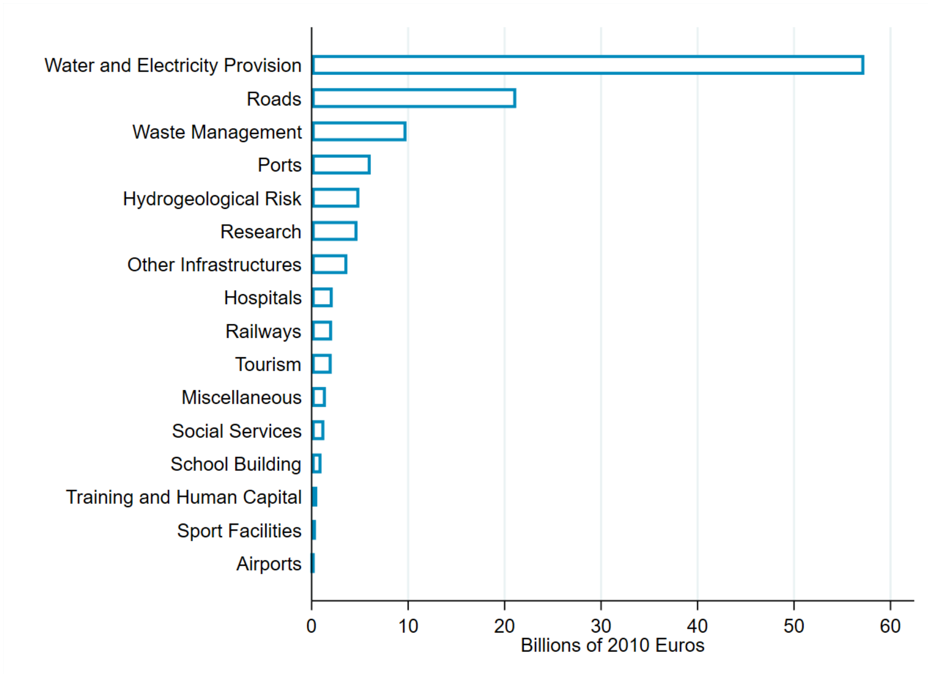
Notes.. This figure shows a map of Italy divided into municipal territories. The dark blue areas denote municipalities belonging to CasMez's jurisdiction. CasMez's jurisdiction includes the regions of Abruzzo, Molise, Campania, Apulia, Basilicata, Calabria, Sicily, and Sardinia, the provinces of Latina and Frosinone and parts of the provinces of Rieti and Roma in Lazio, parts of the provinces of Ascoli Piceno in Marche, and the municipalities belonging to the Elba, Giglio, and Capraia Islands in Tuscany.

Figure 1.4: Time Series of CasMez's Investments, by Type



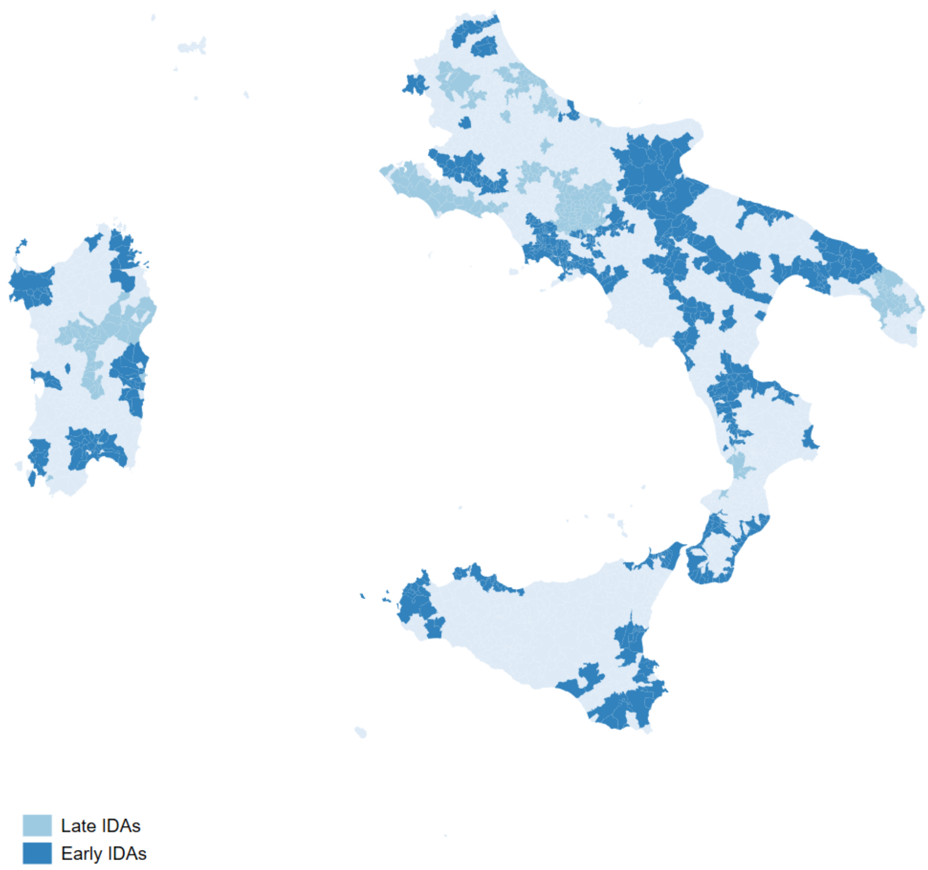
Notes. The figure shows the time series of CasMez's investments, decomposed between "Public Infrastructures" and "Firm Grants/Loans". The time series covers the whole period of CasMez's activity (i.e., 1950-1992) and the years after, up to 2000. For each year, the bar indicates the billions of 2010 USD spent by CasMez. The dark blue portion of the bar measures firm grants and loans, while the light blue portion of the bar measures investments in public infrastructures. Firm grants started in 1957, after the approval of Law 634/1957 which shifted the prerogatives of CasMez toward industrialization. Investments drastically decreased after 1992, the end of the program. Data on CasMez's investments come from the *Archivi dello Sviluppo Economico Territoriale* (ASET). Website: https://aset.acs.beniculturali.it/aset-web/.

Figure 1.5: Time Series of CasMez's Infrastructure Investments, by Type



Notes. The figure shows how CasMez's spending in "Public Infrastructures" is allocated across different types of projects over the whole 1950-1992 period. Each light blue bar indicates the billions of 2010 Euros approved by CasMez for each public infrastructure category. Data on CasMez's investments come from the *Archivi dello Sviluppo Economico Territoriale* (ASET). Website: https://aset.acs.beniculturali.it/aset-web/.

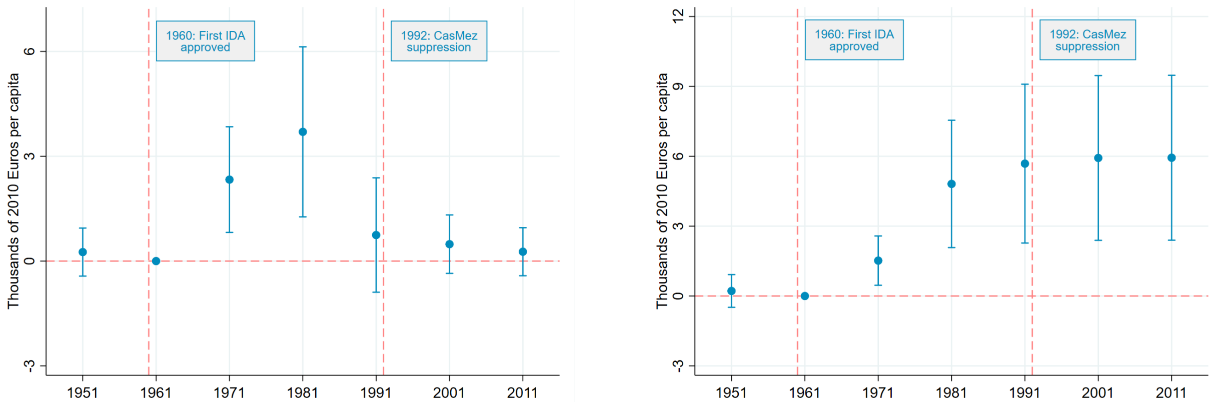
Figure 1.6: Early vs. Late Industrial Development Areas (IDAs)



Notes. The figure shows a map of CasMez's jurisdiction. The dark blue areas indicate municipalities belonging to early-approved IDAs (i.e., formed between 1960 and 1965), while the light blue areas indicate municipalities belonging to late-approved IDAs (i.e., formed between 1966 and 1974).

Figure 1.7: Early IDAs vs. Late IDAs - First Stage

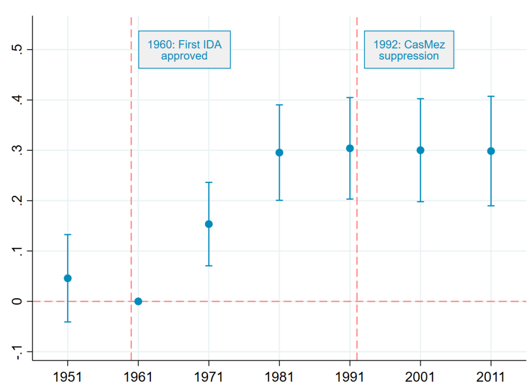
(a) Decade-Specific Investments (b) Cumulative Investments



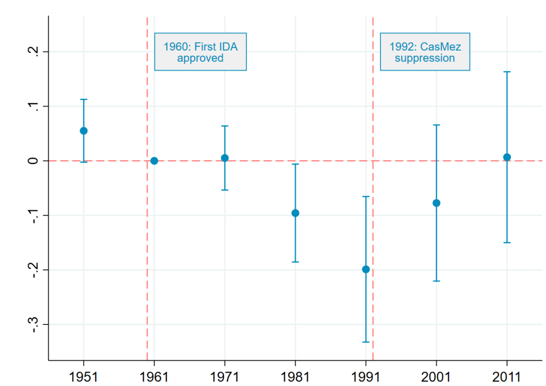
Notes. The figure shows the coefficients $\hat{\beta}_t$ estimated from Equation (1.1). Recall that the unit of observation is a municipality and this dynamic difference-in-differences design compares municipalities belonging to early-approved IDAs (1960-1965) with municipalities belonging to late-approved IDAs (1966-1974). The outcome variable in Panel (a) is decade-specific per-capita investments. while the outcome variable in Panel (b) is cumulative per-capita investments. Investments comprise public infrastructure spending and firm grants and they are converted in per-capita terms by dividing for 1961 municipal population. The period assigned to each investment is the year in which the project was approved by CasMez. Observations are weighted by 1961 population and standard errors are clustered at the municipality level.

Figure 1.8: Early IDAs vs. Late IDAs - Reduced Form

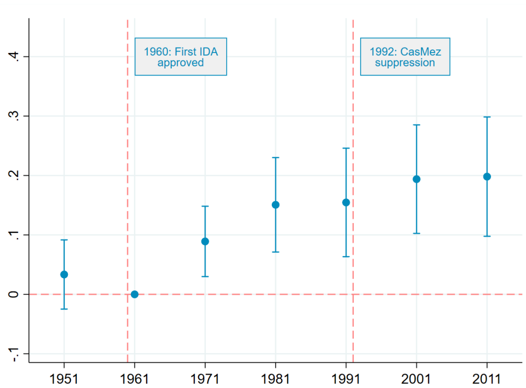
(a) Log Manufacturing Employment



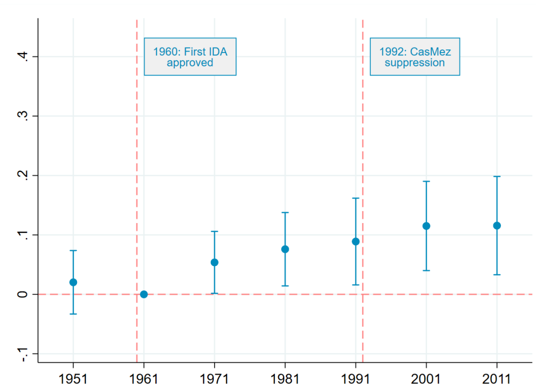
(b) Log Agriculture Employment



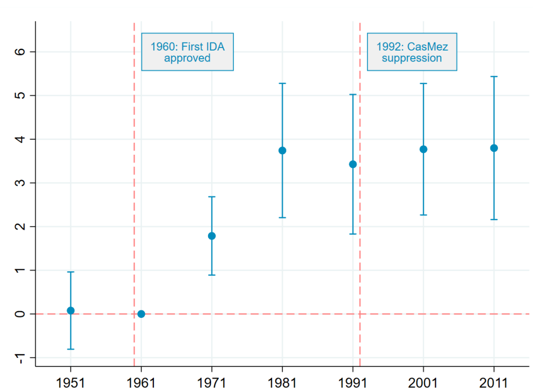
(c) Log Employment



(d) Log Population

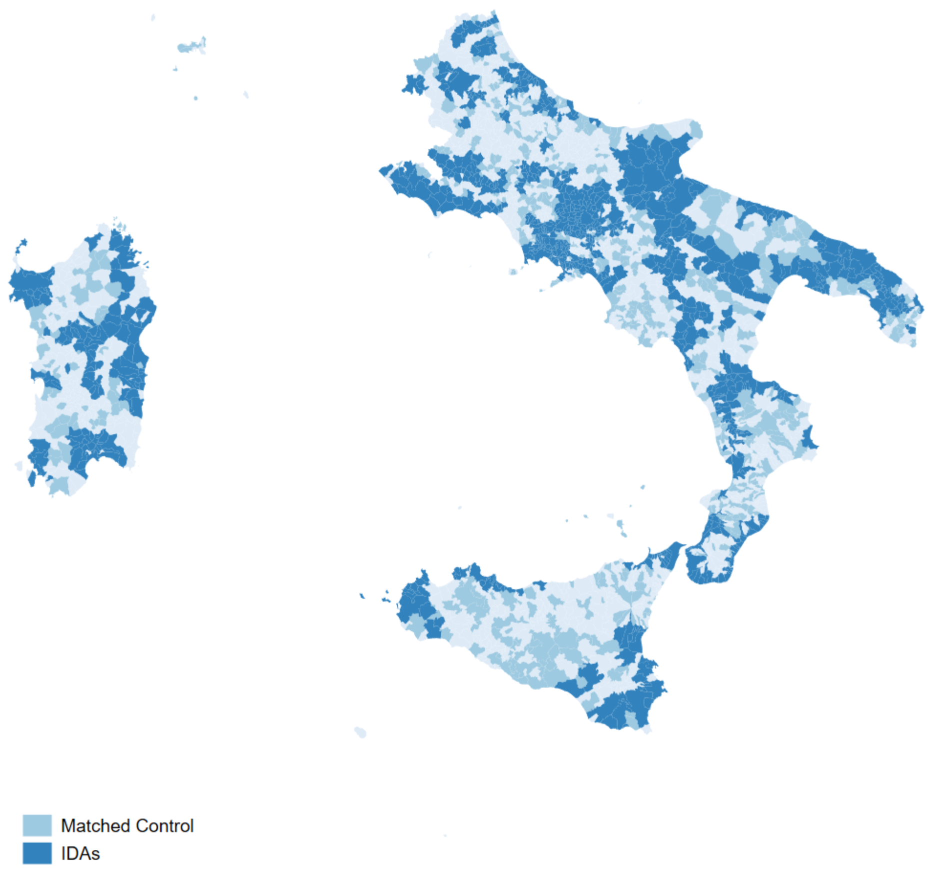


(e) Employment Rate



Notes. The figure shows the coefficients $\hat{\beta}_t$ estimated from Equation (1.1) for five different outcome variables. Panel (a): log manufacturing employment; Panel (b): log agriculture employment; Panel (c): log total employment; Panel (d): log population; Panel (e): employment rate. Recall that the unit of observation is a municipality and this dynamic difference-in-differences design compares municipalities belonging to early-approved IDAs (1960-1965) with municipalities belonging to late-approved IDAs (1966-1974). Observations are weighted by 1961 population and standard errors are clustered at the municipality level.

Figure 1.9: IDA Municipalities vs. Matched Non-IDA Municipalities

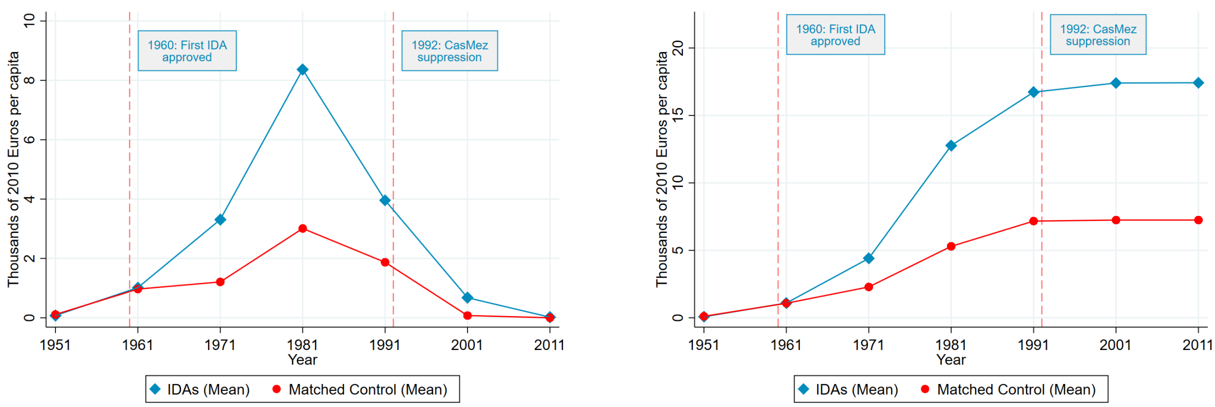


Notes. The figure shows a map of CasMez's jurisdiction. The dark blue areas indicate municipalities belonging to IDAs, while light blue areas indicate 1-to-1 matched control municipalities not belonging to any IDA. Treated and control municipalities are matched on a set of 1951 characteristics and 1951-1961 trends.

Figure 1.10: 1-to-1 Match - First Stage

(a) Decade-Specific Investments

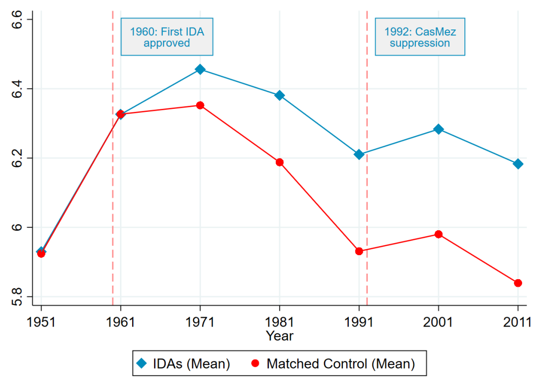
(b) Cumulative Investments



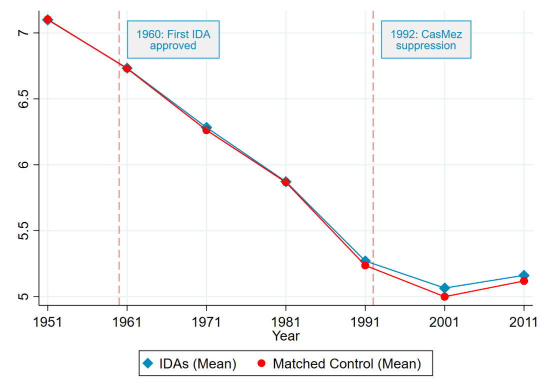
Notes. The figure shows the coefficients $\hat{\beta}_t$ estimated from Equation (1.2). Recall that the unit of observation is a municipality and this dynamic difference-in-differences design compares each municipality belonging to IDAs with one municipality not belonging to IDAs, matched on 1951 characteristics and 1951-1961 trends. The outcome variable in Panel (a) is decade-specific per-capita investments, while the outcome variable in Panel (b) is cumulative per-capita investments. Investments comprise public infrastructure spending and firm grants and they are converted in per-capita terms by dividing for 1961 municipal population. The period assigned to each investment is the year in which the project was approved by CasMez.

Figure 1.11: 1-to-1 Match - Reduced Form

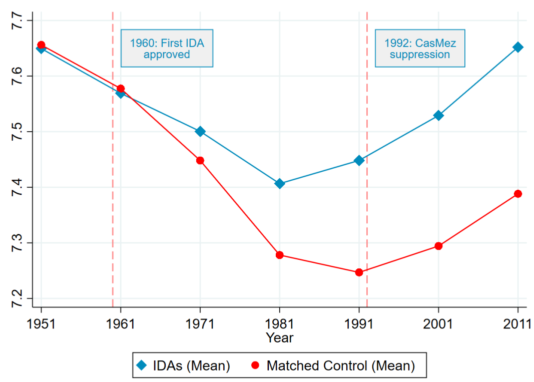
(a) Log Manufacturing Employment



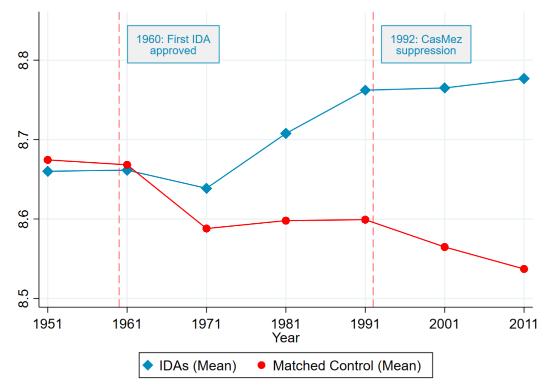
(b) Log Agriculture Employment



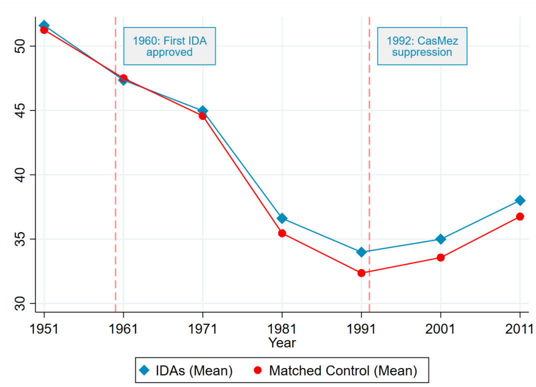
(c) Log Employment



(d) Log Population



(e) Employment Rate



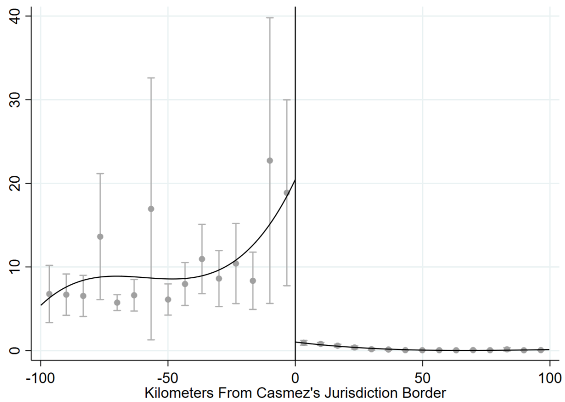
Notes. The figure shows the coefficients $\hat{\beta}_t$ estimated from Equation (1.2) for five different outcome variables. Panel (a): log manufacturing employment; Panel (b): log agriculture employment; Panel (c): log employment; Panel (d): log population; Panel (e): employment rate. Recall that the unit of observation is a municipality and this dynamic difference-in-differences design compares each municipality belonging to IDAs with one municipality not belonging to IDAs, matched on 1951 characteristics and 1951-1961 trends.

Figure 1.12: Municipalities North vs. South of CasMez's Jurisdiction Border

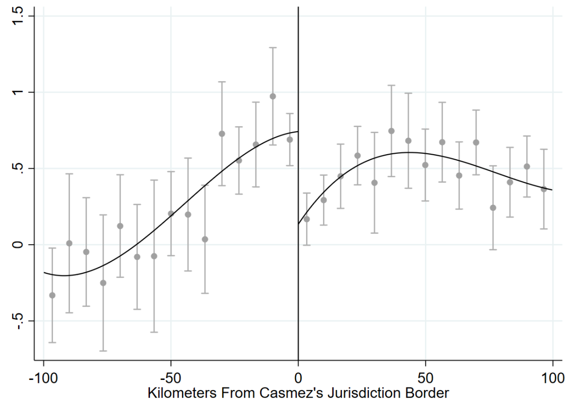
Notes. The figure shows a map of the municipalities 100 km North vs. South of CasMez's jurisdiction border. The light blue areas indicate municipalities located North of the border, while the dark blue areas indicate municipalities located South of the border.

Figure 1.13: Static Long Difference-in-Discontinuities (1991)

(a) Cumulative Investments



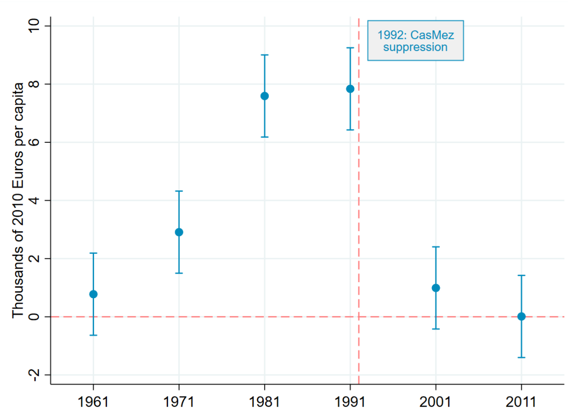
(b) Δ Log Manufacturing Employment (1991-1951)



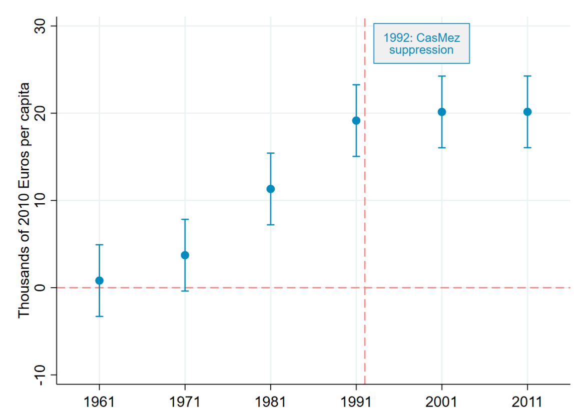
Notes. The figure shows the coefficients $\hat{\beta}_{1991}$ estimated from Equation (1.3). Recall that the unit of observation is a municipality, and this static long difference-in-discontinuities design compares municipalities just South vs. North of CasMez's jurisdiction border. The two continuous lines fit polynomial functions of degree 3 of distance from the border, separately for the South vs. North sample. The outcome variable in Panel (a) is cumulative investments per capita, while in Panel (b) is the percent change in log manufacturing employment from 1951. Cumulative investments per capita comprise public infrastructures and firm grants divided by the 1961 municipal population.

Figure 1.14: Dynamic Long Difference-in-Discontinuities - First Stage

(a) Decade-Specific Investments



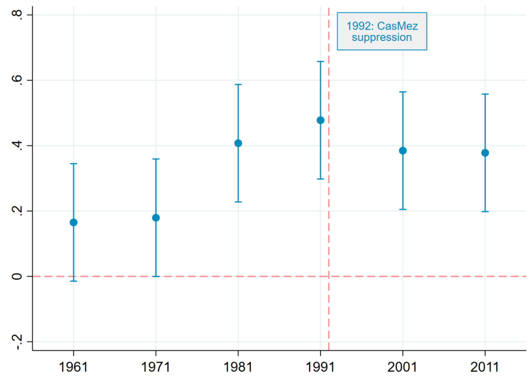
(b) Cumulative Investments



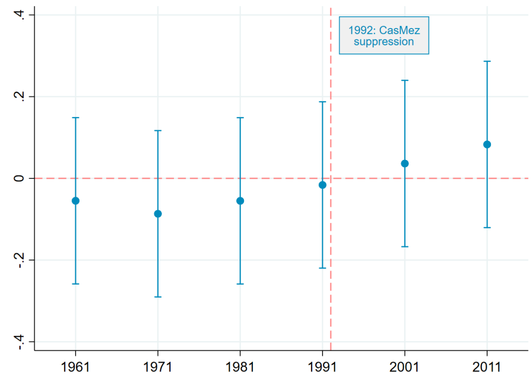
Notes. The figure shows the coefficients $\hat{\beta}_t$ estimated from Equation (1.3). Recall that the unit of observation is a municipality, and this dynamic long difference-in-discontinuities design compares municipalities just South vs. North of CasMez's jurisdiction border. The outcome variable in Panel (a) is decade-specific investments per capita, while in Panel (b) is cumulative investments per capita. Measures of investments per capita comprise public infrastructures and firm grants divided by the 1961 municipal population.

Figure 1.15: Dynamic Long Difference-in-Discontinuities - Reduced Form

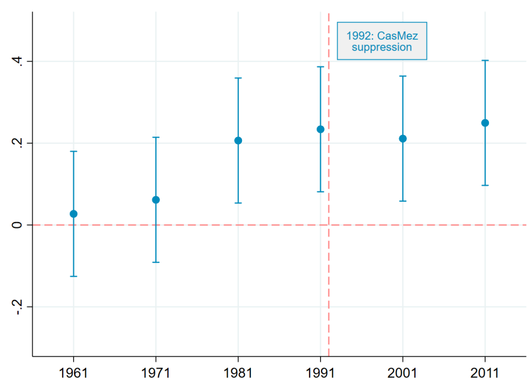
(a) Log Manufacturing Employment



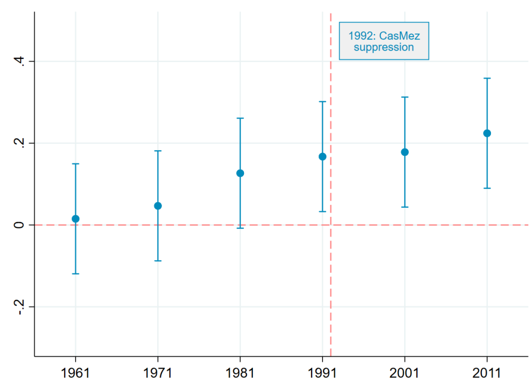
(b) Log Agriculture Employment



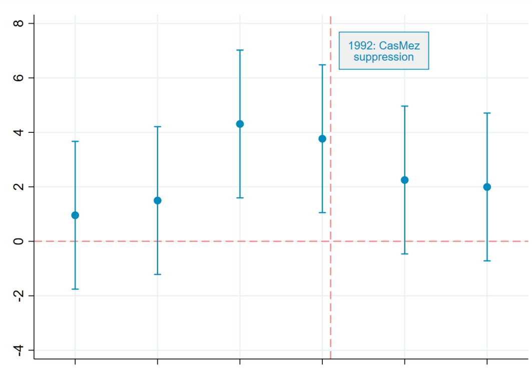
(c) Log Employment



(d) Log Population



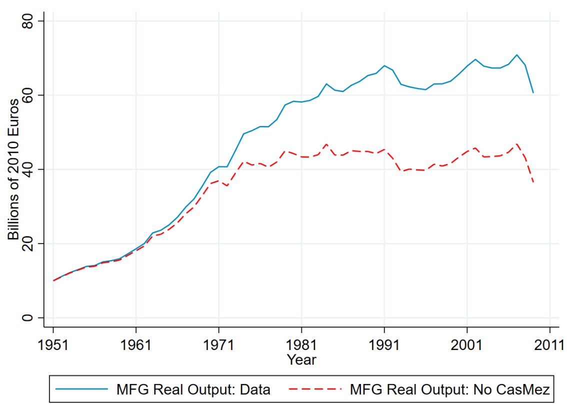
(e) Employment Rate



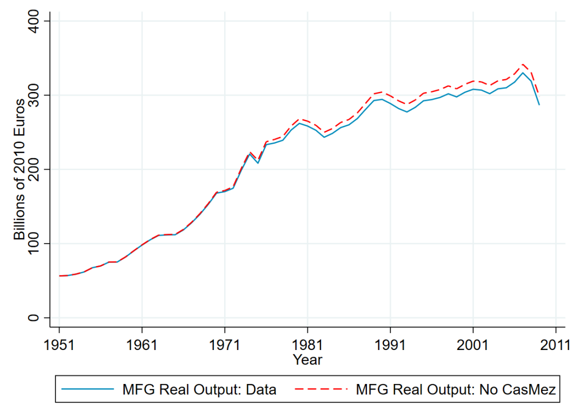
Notes. The figure shows the coefficients $\hat{\beta}_t$ estimated from Equation (1.3). Recall that the unit of observation is a municipality, and this dynamic long difference-in-discontinuities design compares municipalities just South vs. North of CasMez's jurisdiction border. Results are reported for five different outcome variables. Panel (a): log manufacturing employment; Panel (b): log agriculture employment; Panel (c): log employment; Panel (d): log population; Panel (e): employment rate.

Figure 1.16: Industrial Production: Data vs. Counterfactual (No CasMez)

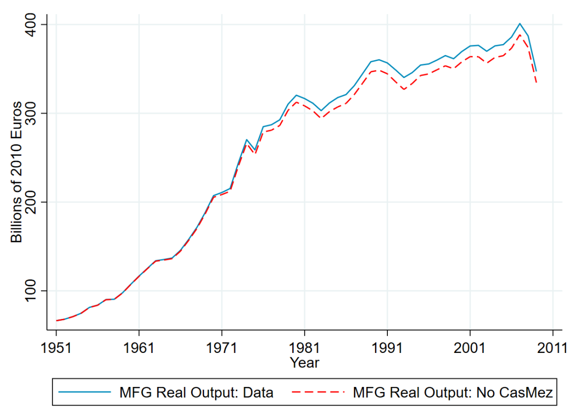
(a) South



(b) Center-North

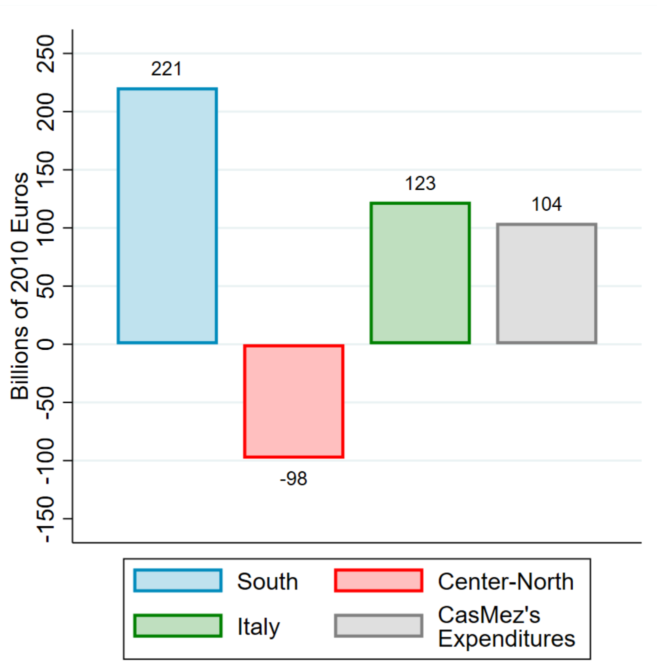


(c) Italy



Notes. The figure shows the dynamic of industrial production in the South, the Center-North, and the country as a whole (light blue lines) and the simulated counterfactual dynamic in the absence of CasMez's investments. Panel (a) shows the results of the model-based analysis for the South, Panel (b) for the Center-North, and Panel (c) for the whole country. The unit of measure is billions of 2010 Euro. The data used for the simulation come from SVIMEZ (2011). The parameters used for the simulation are listed in Table 1.9.

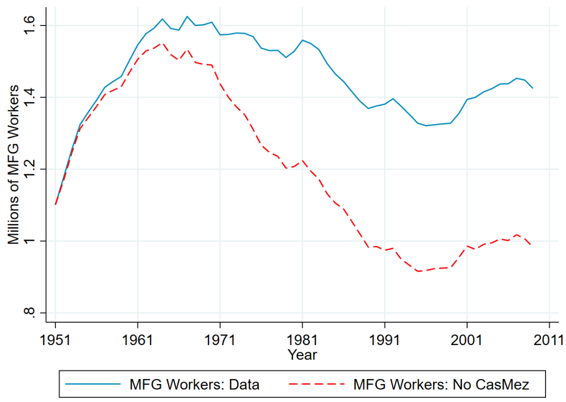
Figure 1.17: PDV of Industrial Production Gains and CasMez's Spending



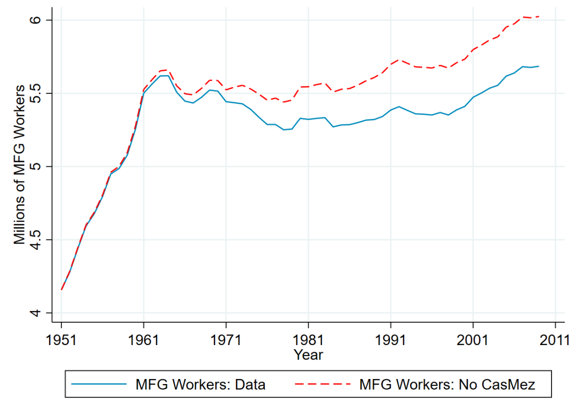
Notes. The first three bars indicate the net present value, discounted to 1951 with a real annual discount rate of 3%, of the stream of realized gains/losses accrued to the South, the Center-North, and the whole country. The last bar displays the net present value, discounted to 1951 with a real annual discount rate of 3%, of CasMez's investments. The calculation ignores all costs other than the operating costs related to the investments, as well as any cost of funds. The unit of measure is billions of 2010 Euros.

Figure 1.18: MFG Employment: Data vs. Counterfactual (No CasMez)

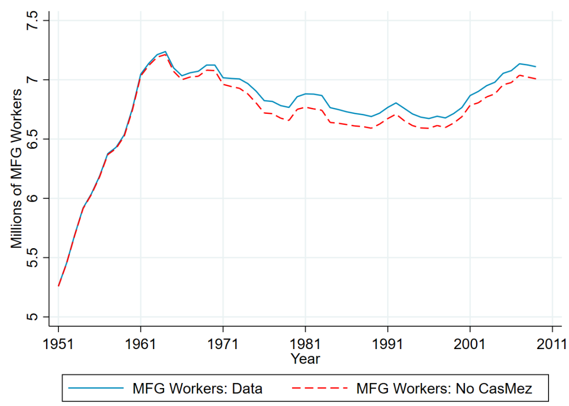
(a) South



(b) Center-North

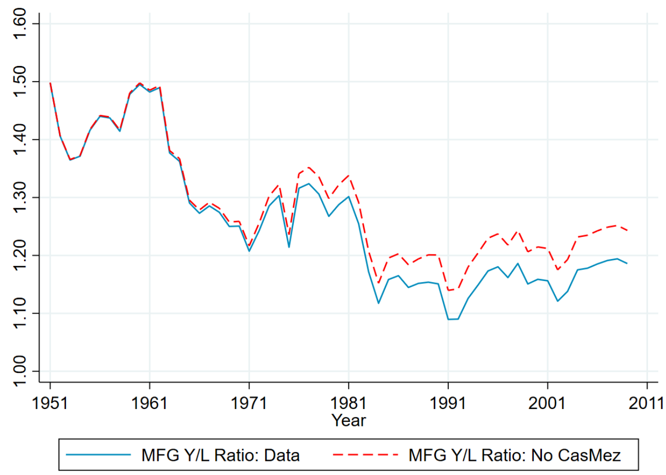


(c) Italy



Notes. The figure shows the dynamic of manufacturing employment in the South, the Center-North, and the country as a whole (light blue lines) and the simulated counterfactual dynamic in the absence of CasMez's investments. Panel (a) shows the results of the model-based analysis for the South, Panel (b) for the Center-North, and Panel (c) for the whole country. The unit of measure is million of workers. The data used for the simulation come from SVIMEZ (2011). The parameters used for the simulation are listed in Table 1.9.

Figure 1.19: Center-North vs. South Manufacturing Labor Productivity Ratio



Notes. The figure shows the dynamic of the Center-North vs. South manufacturing output per worker ratio. The light blue line reports the time series of this ratio. The red dashed line reports the evolution of the ratio in the absence of CasMez's investments, as simulated through the model. The data used for the simulation come from SVIMEZ (2011). The parameters used for the simulation are listed in Table 1.9.

Table 1.1: CasMez Endowment Over Time

Law n.	Date	Thousands of Euros (2010)	Thousands of USD (2010)
646/1950	August 10th, 1950	€17,500,601	$23,384,303
949/1952	July 25th, 1952	€4,284,333	$5,724,726
634/1957	July 29th, 1957	€10,097,702	$13,492,549
1349/1957	December 28th, 1959	€112,935	$150,904
622/1959	July 24th, 1959	€369,234	$493,371
454/1961	June 2nd, 1961	€361,518	$483,061
28/1962	January 30th, 1962	€48,730	$65,113
588/1962	June 11th, 1962	€30,590	$40,875
608/1964	July 6th, 1964	€805,388	$1,076,160
221/1965	March 30th, 1965	€28,945	$38,676
717/1965	June 26th, 1965	€15,823,029	$21,142,731
498/1967	June 21st, 1967	€2,411,085	$3,221,692
160/1969	April 8th, 1969	€8,015,985	$10,710,959
1034/1970	December 18th, 1970	€847,570	$1,132,523
205/1971	April 15th, 1971	€2,114,880	$2,825,903
853/1971	October 6th, 1971	€55,397,355	$74,021,946
868/1973	December 27th, 1973	€865,570	$1,156,575
371/1974	August 12th, 1974	€5,797,304	$7,746,357
493/1975	October 16th, 1975	€4,947,869	$6,611,342
183/1976	May 2nd, 1976	€68,792,273	$91,920,235
843/1978	December 21st, 1978	€12,470,382	$16,662,925
218/1978	March 6th, 1978	€789,650	$1,055,130
146/1980	April 24th, 1980	€4,561,039	$6,094,460
874/1980	December 22nd, 1980	€233,108	$311,479
119/1981	March 30th, 1981	€3,712,659	$4,960,855
13/1982	January 26th, 1982	€2,145,126	$2,866,318
546/1982	August 12th, 1982	€6,588,957	$8,804,165
132/1983	April 30th, 1983	€4,737,704	$6,330,520
651/1983	December 1st, 1983	€20,885,965	$27,907,826
64/1986	March 1st, 1986	€82,459,237	$110,182,033
113/1986	April 11th, 1986	€701,057	$936,753
Total:		€337,937,783	$451,552,465

Notes. The table reports all the laws that provided CasMez with resource endowments over the period 1950-1992. The first column indicates the law that was passed to confer transfers to CasMez and the second column reports the exact date in which the law was passed. The third and fourth columns report the amount of resources devolved to CasMez by each law in 2010 Euros and US dollars, respectively. At the bottom of the table, the total amount of resources devoted to CasMez over the whole period is reported. Source: SVIMEZ (2011).

Table 1.2: IDA Approvals Over Time

Decree	Year	Type	IDA
DPR 804/1960	1960	Area di Sviluppo Industriale	Bari
DPR 805/1960	1960	Area di Sviluppo Industriale	Brindisi
DPR 806/1960	1960	Area di Sviluppo Industriale	Taranto
DPR 1013/1961	1961	Nucleo di Industrializzazione	Potenza
DPR 1314/1961	1961	Area di Sviluppo Industriale	Salerno
DPR 1410/1961	1961	Area di Sviluppo Industriale	Cagliari
DPR 50/1962	1962	Nucleo di Industrializzazione	Valle del Basento
DPR 235/1962	1962	Nucleo di Industrializzazione	Trapani
DPR 235/1962	1962	Nucleo di Industrializzazione	Golfo di Policastro
DPR 236/1962	1962	Nucleo di Industrializzazione	Avellino
DPR 238/1962	1962	Nucleo di Industrializzazione	Foggia
DPR 293/1962	1962	Nucleo di Industrializzazione	Piana di Sibari
DPR 574/1962	1962	Nucleo di Industrializzazione	Messina
DPR 575/1962	1962	Area di Sviluppo Industriale	Caserta
DPR 770/1962	1962	Nucleo di Industrializzazione	Gela
DPR 1374/1962	1962	Nucleo di Industrializzazione	Avezzano
DPR 1554/1962	1962	Nucleo di Industrializzazione	Sassari
DPR 1589/1962	1962	Nucleo di Industrializzazione	Vasto
DPR 1601/1962	1962	Nucleo di Industrializzazione	Tortoli-Arbatax
DPR 1872/1962	1962	Area di Sviluppo Industriale	Napoli
DPR 2048/1962	1962	Nucleo di Industrializzazione	Teramo
DPR 2054/1962	1962	Nucleo di Industrializzazione	Crotone
DPR 791/1963	1963	Nucleo di Industrializzazione	Ragusa
DPR 808/1963	1963	Nucleo di Industrializzazione	Oristano
DPR 1016/1963	1963	Nucleo di Industrializzazione	Reggio Calabria
DPR 1328/1963	1963	Nucleo di Industrializzazione	Sulcis-Iglesias
DPR 1526/1963	1963	Nucleo di Industrializzazione	Frosinone
DPR 2390/1963	1963	Area di Sviluppo Industriale	Catania
DPR 75/1964	1964	Area di Sviluppo Industriale	Palermo
DPR 103/1964	1964	Nucleo di Industrializzazione	Ascoli Piceno
DPR 596/1964	1964	Area di Sviluppo Industriale	Siracusa
DPR 890/1964	1964	Nucleo di Industrializzazione	Olbia
DPR 1480/1964	1964	Nucleo di Industrializzazione	Caltagirone
DPR 1383/1965	1965	Nucleo di Industrializzazione	Rieti

Table 2: IDA Approvals Over Time (cont.)

Decree	Year	Type	IDA
DPR 562/1966	1966	Area di Sviluppo Industriale	Latina
DPR 609/1966	1966	Nucleo di Industrializzazione	Lecce
DPR 719/1967	1967	Nucleo di Industrializzazione	Gaeta-Formia
DPR 1019/1967	1967	Nucleo di Industrializzazione	Valle del Biferno
DPR 320/1968	1968	Nucleo di Industrializzazione	Santa Eufemia-Lamezia
DPR 657/1968	1968	Nucleo di Industrializzazione	Benevento
DPR 468/1969	1969	Area di Sviluppo Industriale	Valle del Pescara
DPR 15/1970	1970	Nucleo di Industrializzazione	Sulmona
DPR 88/1970	1970	Area di Sviluppo Industriale	L'Aquila
DPR 299/1970	1970	Nucleo di Industrializzazione	Sangro Aventino
DPR 1447/1970	1970	Nucleo di Industrializzazione	Vibo Valentia
DPR 205/1972	1972	Nucleo di Industrializzazione	Sardegna Centrale
DPR 153/1974	1974	Nucleo di Industrializzazione	Isernia-Venafro
DPR 414/1974	1974	Nucleo di Industrializzazione	Campobasso-Boiano

Notes. The table reports a comprehensive list of the approved Industrial Development Areas (IDAs) within CasMez's jurisdiction between 1960 and 1974. The first column indicates the Presidential Decree (*Decreto del Presidente della Repubblica*) that formally approves the IDA. The second column reports the year of IDA approval. The third column indicates the type of IDA (*Area di Sviluppo Industriale* or *Nucleo di Industrializzazione*). The last column reports the name of the IDA. The data to produce this table were collected by the author.

Table 1.3: 1-to-1 Matching Balance Table

	(1) Treated	(2) Matched Control	(3) Difference
1951 Sh. of Illiterate Pop.	25.12	25.51	-0.38
	(7.28)	(8.42)	(10.88)
1951 Employment Rate	51.60	51.26	0.34
	(10.53)	(11.68)	(14.97)
1951 Sh. Manufacturing Emp.	21.47	21.20	0.27
	(12.96)	(12.80)	(15.01)
1951 Log Population	8.66	8.67	-0.01
	(1.02)	(1.03)	(0.80)
1951 Log Employment	7.65	7.66	-0.01
	(0.98)	(0.96)	(0.81)
1951 Log Manufacturing Emp.	5.93	5.92	0.01
	(1.32)	(1.30)	(0.92)
1951 Log Agriculture Emp.	7.10	7.10	-0.00
	(0.87)	(0.89)	(1.06)
1951-1961 Change Sh. of Illiterate Pop.	-8.05	-8.30	0.25
	(3.43)	(3.47)	(4.91)
1951-1961 Change Employment Rate	-4.25	-3.76	-0.49
	(6.11)	(6.40)	(8.86)
1951-1961 Change Sh. Manufacturing Emp.	10.31	10.27	0.04
	(8.24)	(8.59)	(11.53)
1951-1961 Change Log Population	0.00	-0.01	0.01
	(0.15)	(0.13)	(0.15)
1951-1961 Change Log Employment	-0.08	-0.08	-0.00
	(0.20)	(0.18)	(0.22)
1951-1961 Change Log Manufacturing Emp.	0.40	0.40	-0.01
	(0.39)	(0.41)	(0.55)
1951-1961 Change Log Agriculture Emp.	-0.37	-0.37	0.01
	(0.31)	(0.29)	(0.41)
Observations	879	879	879

Notes. The table reports the means and standard deviations of all variables used to match each municipality belonging to IDAs with one municipality not belonging to an IDA, for both the treatment and the matched control group. The third column reports the difference between the means and its standard deviation. * ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

Table 1.4: Effect of €1,000 Investments Per Capita - 2SLS Estimates

Outcome Variables	(1) Identification I	(2) Identification II	(3) Identification III
Log MFG Employment	.051***	.031***	.024***
	(.014)	(.008)	(.003)
Log Agr. Employment	-.023	.004	-.002
	(.015)	(.005)	(.003)
Log Employment	.029***	.023***	.012***
	(.010)	(.007)	(.002)
Log Population	.016**	.021***	.009***
	(.007)	(.006)	(.002)
Employment Rate	.721***	.135	.170***
	(.257)	(.093)	(.046)
Observations	6,153	12,194	4,656
Municipalities	879	1,414	776
First Stage F-Stat	10.56	55.90	211.63
Municipality FE	✓		✓
Region × Time FE	✓		✓
Baseline Controls × Time FE	✓		✓

Notes. The table displays two-stage least squares (2SLS) coefficients obtained from regressions with different variables as outcomes and cumulative per-capita CasMez's investments as the main regressor. In all columns, an observation is a municipality-year. The first-stage and reduced-form regressions correspond to the static versions of the dynamic specifications described by Equation (1.1) for column (1) and Equation (1.2) for column (2), and Equation (1.3) for column (3), respectively. Each column reports the semi-elasticity of the municipality-level outcome variables to €1,000 (2010 Euro) additional CasMez's investments per capita. The table reports the number of observations, the number of unique units of observations, and the Kleibergen-Paap F-statistic for weak identification, for all three specifications. Controls include unit fixed effects, region-specific trends, and the interaction of baseline unit-level characteristics (i.e., log population, the manufacturing share of employment, manufacturing employment density, and the share of illiterate population) with time dummies. Controls are not present in the specification corresponding to column (2) because the sample is restricted to treated and control municipalities, already matched on baseline characteristics and trends. Standard errors in parentheses are clustered at the municipality level. * ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

Table 1.5: Cost Per Job and Manufacturing Job Multiplier

	(1) Identification I	(2) Identification II	(3) Identification III
Municipality-level			
Additional jobs per MFG job	0.6	1.2	1.2
Cost per job (in terms of 2010 Southern GDP per capita)	6.5	7.9	12.1
Province-level			
Additional jobs per MFG job	1.4		
Cost per job (in terms of 2010 Southern GDP per capita)	6.2		

Notes. The table reports the municipal and province-level manufacturing job multiplier and cost per job implied by the results in Table 1.4. The manufacturing job multiplier indicates the total number of new jobs stemming from a new manufacturing job and it is calculated as follows: (semi-elasticity of total employment to CasMez's cumulative investments × 1961 municipal total employment)/(semi-elasticity of manufacturing employment to CasMez's cumulative investments × 1961 municipal manufacturing employment)-1. A manufacturing job multiplier of 0.6 implies that one manufacturing job creates 0.6 additional non-manufacturing jobs in the same municipality. The cost per job is expressed in terms of 2010 Southern real GDP per capita and is calculated as follows: (€1,000 × 1961 municipal population)/(semi-elasticity of total employment to CasMez's cumulative investments × 1961 municipal total employment). This quantity is then normalized by the 2010 Southern real GDP per capita. A cost per job (in terms of 2010 Southern GDP per capita) of 6.5 means that the cost per new job created at the municipal level is equivalent to 6.5 times the 2010 GDP per capita in the South.

Table 1.6: Effect of €1,000 Investments Per Capita - 2SLS Estimates

Outcome Variables	(1) Municipality-Level	(2) Province-Level
Log MFG Employment	.051***	.037**
	(.014)	(.014)
Log Agr. Employment	-.023	.024
	(.015)	(.023)
Log Employment	.029***	.028**
	(.010)	(.011)
Log Population	.016**	.023**
	(.007)	(.009)
Employment Rate	.721***	.212
	(.257)	(.224)
Observations	6,153	266
Units	879	38
First Stage F-Stat	10.56	9.91
Unit FE	✓	✓
Region × Time FE	✓	✓
Baseline Controls × Time FE	✓	✓

Notes. The table displays two-stage least squares (2SLS) coefficients obtained from regressions with different variables as outcomes and cumulative per-capita CasMez's investments as the main regressor. In column (1), an observation is a municipality-year, while in column (2) an observation is a province-year. The first-stage and reduced-form regressions correspond to the static versions of the dynamic specifications described by Equation (1.1) for column (1) and Equation (1.4) for column (2), respectively. Column (1) and column (2) report the semi-elasticity of the municipality-level and province-level outcome variables to €1,000 (2010 Euro) additional CasMez's investments per capita, respectively. The table reports the number of observations, the number of unique units of observations, and the Kleibergen-Paap F-statistic for weak identification. Controls include unit fixed effects, region-specific trends, and the interaction of baseline unit-level characteristics (i.e., log population, the manufacturing share of employment, manufacturing employment density, and the share of illiterate population) with time dummies. Standard errors in parentheses in column (1) are clustered at the municipality level. * ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

Table 1.7: Effect of €1,000 Investments Per Capita - 2SLS Estimates

	(1) Log Pop.	(2) Net Mig.	(3) South	(4) Center-North
Investments Per Capita	.023***	.017**	.009*	.008**
	(.009)	(.007)	(.005)	(.004)
Observations	266	266	266	266
Units	38	38	38	38
First Stage F-Stat	9.91	9.91	9.91	9.91
Province FE	✓	✓	✓	✓
Region × Time FE	✓	✓	✓	✓
Baseline Controls × Time FE	✓	✓	✓	✓

Notes. The table displays two-stage least squares (2SLS) coefficients obtained from four regressions with different variables as outcomes and cumulative per-capita CasMez's investments as the main regressor. An observation is a province-year. Cumulative per-capita investments are instrumented by a triple interaction of the share of province-level population at baseline residing in a municipality that belongs to an IDA with a dummy for early-approved IDAs, and a dummy for the post-1961 periods. The first-stage and reduced-form regressions are described by Equation (1.1). Column (1) reports the semi-elasticity of the province-level population to €1,000 (2010 Euro) additional CasMez's investments per capita. Column (2) captures the percent population gains due to favorable internal net migration flows. Column (3) and column (4) decompose the effect estimated in column (2). Column (3) reports the percent population gains due to favorable net migration flows within the South. Column (4) reports the percent population gains due to favorable net migration flows between the South and the Center-North. The table reports the number of observations, the number of unique provinces, and the Kleibergen-Paap F-statistic for weak identification. Controls include province fixed effects and region-specific trends, and the interaction of baseline province-level characteristics (i.e., log population, the manufacturing share of employment, manufacturing employment density, and share of illiterate population) with time dummies. * ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

Table 1.8: IV Estimates of Agglomeration Elasticities

	(1) South	(2) Center-North	(3) Difference
$(\hat{\gamma/\beta})$	0.317***	0.394***	-0.077***
	(0.022)	(0.021)	(0.026)
Observations	13,155	25,555	38,710
Units	2,631	5,111	7,742
First Stage F-Stat	746.8	1032.7	
Municipality FE	✓	✓	✓
Region × Time FE	✓	✓	✓
Baseline Controls × Time FE	✓	✓	✓

Notes. The table displays the 2SLS coefficient obtained by estimating Equation (1.12), allowing the coefficient γ/β to differ between the Center-North and the South. An observation is a municipality-year and the panel covers the period 1971-2011. The dependent variable is municipal manufacturing employment. The main regressor is one-decade-lagged manufacturing employment density. The main regressor is instrumented with two-decade-lagged manufacturing employment density. The table reports the number of observations, the number of unique provinces, and the Kleibergen-Paap F-statistic for weak identification. Baseline controls include log population, the share of manufacturing employment, manufacturing employment density, the share of the illiterate population, and a dummy variable taking value 1 if the municipality belongs to an IDA. Observations are weighted by 1951 municipal population. Standard errors are clustered at the province level. * ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

Table 1.9: Structural Parameters and Measured Quantities

	Parameter	Value	Method	Source
	α	0.3	Calibration	Griliches (1967)
	$(1-\alpha)/\beta$	1.5	Calibration	Kline and Moretti (2014)
$\rightarrow$	β	0.47	Calibration	-
	γ_S/β	0.32	Estimation	Table 1.8
$\rightarrow$	γ_S	0.15	Estimation/Calibration	-
	γ_N/β	0.39	Estimation	Table 1.8
$\rightarrow$	γ_N	0.19	Estimation/Calibration	-
	$\eta/k_S^P(\beta-\gamma_S)$	0.037	Estimation	Table 1.6
$\rightarrow$	η/k_S^P	0.012	Estimation/Calibration	-
	Quantity		**Method**	**Source**
	$\mathrm{d}k_S^P$		Measurement	ASET
	y_S		Measurement	SVIMEZ (2011)
	y_N		Measurement	SVIMEZ (2011)
	ℓ_S		Measurement	SVIMEZ (2011)
	ℓ_N		Measurement	SVIMEZ (2011)
	$(\mathrm{d}\ell_S/\mathrm{d}k_S^P)$		Estimation/Calibration	Tables 1.6 and 1.7
	$(\mathrm{d}\ell_N/\mathrm{d}k_S^P)$		Estimation/Calibration	Tables 1.6 and 1.7

Notes. The table lists the structural parameters and quantities present in Equation (1.11). A parameter value is attached to each parameter in the second column. The third column specifies the methodology followed to retrieve the parameter or quantity of interest. The methodology is a “Calibration” if the parameter value is calibrated taking a value from an external source. In that case, the source is listed in the fourth column. The methodology is “Estimation” if the parameter value is estimated in the empirical analysis of the paper. In that case, the Table with the relevant result is listed in the fourth column. When the parameter value is obtained by combining calibration and estimation, the table reports “Estimation/Calibration”. For quantities measured directly from primary sources the table reports “Measurement” in the third column and the source in the last column.

Table 1.10: Contributions to Crowding-Out Effects

	Quantities/Parameters			
	MFG ℓ_i/P_i	MFG y_i/ℓ_i	γ_i	**Multiplier**
Baseline	$\neq$	$\neq$	$\neq$	1.3
	$=$	$\neq$	$\neq$	1.8
	$\neq$	$=$	$\neq$	1.4
	$\neq$	$\neq$	$=$	1.4
	$=$	$=$	$\neq$	1.9
	$=$	$\neq$	$=$	1.9
	$\neq$	$=$	$=$	1.5
	$=$	$=$	$=$	1.9

Notes. The table shows how the *long-run aggregate multiplier* changes when South vs. Center-North differences in three key quantities/parameters are removed. the long-run aggregate multiplier is the ratio between the stream of the national industrial production gains accrued up to 2011 and CasMez's expenditures discounted to 1951. The 3 quantities/parameters determining the size of the crowding-out effects are the manufacturing employment rate, MFG ℓ_i/P_i, the manufacturing output per worker, MFG y_i/ℓ_i, and the agglomeration elasticities, γ_i. The symbol "$\neq$" means that the South to Center-North regional differentials are not removed. The symbol "$=$" means that the value of the Center-North quantity/parameter is set at the level of the South.

Chapter 2

Inflation Since COVID: Demand or Supply

2.1 Introduction

In June 2022, the 12-month US inflation rate hit a 40-year high at 9% after averaging 2.2% between 2000 and 2020. At the same time, the US labor market reached exceptionally high levels of tightness (Crump et al. 2022; Michaillat and Saez 2022; Blanchard et al. 2022), while global markets suffered from remarkable spikes in commodity prices and supply chain disruptions. The Federal Open Market Committee statement of November 2, 2022, affirmed that "inflation remains elevated, reflecting supply and demand imbalances related to the pandemic, higher food and energy prices, and broader price pressures."

The debate among economists and policymakers has therefore focused on the distinct roles played by demand and supply factors in raising inflation (Di Giovanni 2022; Shapiro 2022a; Ball et al. 2022). Quantifying the extent to which demand-driven economic recovery is responsible for the increase in inflation is important for monetary policy. If demand factors drive inflation, a tighter monetary policy is required to cool down the economy, inducing firms to lower prices. If supply shocks force firms to raise prices, the monetary authority faces a trade-off between stabilizing inflation or output.

Macroeconomic models typically derive a structural relationship between inflation and the unemployment rate, commonly known as the Phillips curve (Phillips, 1958). This relationship formalizes the pattern in which workers ask for higher wages and firms increase prices during demand-driven booms. According to the New-Keynesian formulation of the Phillips curve, inflation is driven by shifts in expectations, supply-side shocks, and demand-side factors. The effect of demand-side factors on inflation is captured by the slope of the Phillips curve. Estimating the slope of the Phillips curve during and after the pandemic is challenging, as severe demand and supply shocks occurred contemporaneously and within an extremely narrow time frame, limiting statistical power.[1]

In this paper, we estimate the slope of the Phillips curve before, during, and after the COVID-19 pandemic. To do so, we combine the use of panel variation in inflation and unemployment at the US metropolitan area level with an instrumental variable approach. Panel data provide us with a larger sample size for parameter estimation than the time series (Mavroeidis et al., 2014). Our empirical strategy is based on a two-region New-Keynesian model of a monetary union that clarifies the threats to identification. Within the model, we derive the regional Phillips curve and relate it to its aggregate counterpart, showing that the slopes of the two coincide.

To our knowledge, this is the first paper providing quasi-experimental estimates of the causal effect of demand factors on inflation during and after COVID. Our benchmark estimates imply a notable flattening of the Phillips curve during COVID and a more than threefold steepening relative to pre-COVID in the aftermath of the pandemic. Considering the estimates provided by the literature for periods prior to 1990,[2] we conclude that the US Phillips curve has recently been steeper than at any time since the late 1970s. Moreover, we find that the slope of the Phillips curve increased more distinctively in the early post-COVID phase and has recently experienced a reversion toward pre-pandemic levels. Finally, our results indicate that the flattening of the Phillips curve during COVID is driven by services, while the subsequent steepening is driven by goods.

We use our benchmark estimates to quantify the contribution of demand factors to the recent increase in inflation. We find that demand-driven economic recovery explains about one-fourth of the post-COVID increase in all-items inflation. Between March 2021 and September 2022, inflation increased by 5.6 percentage points, while the unemployment gap decreased by 1.7 percentage points.[3] Multiplying the change in the unemployment gap (i.e., 1.7%) by our estimate of the slope of the Phillips curve (i.e., 0.85), we obtain an estimate of the change in inflation imputable to demand factors (i.e., $1.7\% \times 0.85 = 1.4\%$). The remaining variation is attributable to shifts in long-run inflation expectations and supply-side shocks. Had the slope of the Phillips curve remained unchanged after COVID, the demand contribution to the rise in inflation would have been small and statistically insignificant.

To guide our empirical exercise, we rely on a two-region New Keynesian general equilibrium model of a monetary union accounting for the supply-side drivers of COVID and post-COVID inflation dynamics, such as the Great Resignation or semiconductor shortages. We allow for shifts in labor supply preferences and outline a vertically-linked production

[1] Ball et al. (2022) explicitly state that they "do not present results for the pandemic period alone, which would mean estimating seven parameters with ten quarters of data."

[2] See, for instance, Hazell et al. (2022).

[3] For our purposes, the unemployment gap is the difference between the unemployment rate and the efficient unemployment rate, as defined in Michaillat and Saez (2022).

structure consisting of an international commodity market, a national perfectly competitive intermediate-input market, and local monopolistically competitive final-goods markets. Domestic firms operating in the intermediate-input sector use commodity and labor as factors of production, while final-goods firms employ intermediate input and labor to produce differentiated consumption goods. Conveniently, this structure matches the available MSA-level data on inflation, measured by the growth rate of all-items consumer price index (CPI).

In accordance with the resulting regional Phillips curve equation, local final-goods inflation is driven by short-run inflation expectations, the local unemployment rate, and three distinct cost-push shock terms. The first denotes the incidence of commodity and intermediate-input price shocks on local inflation, capturing the impact of supply chain disruptions. The second represents local shocks to households' disutility of labor, which likely increased during the pandemic causing labor shortages. The third captures local productivity shocks in the final-goods sector, the supply shock typically featured in standard New-Keynesian models. Distinguishing among these three terms of the cost-push shock allows us to address identification concerns stemming from supply-side factors in our empirical estimation.

Figure 2.1 plots the relationship between 12-month inflation and unemployment rates for 21 MSAs before, during, and after COVID. Raw data clearly point to a flattening of the correlation during the pandemic and a steepening thereafter. However, the simple correlation shown in Figure 2.1 could be driven by aggregate and local confounders. At the national level, the Federal Reserve Bank acted promptly to support the economy as it was being hit by COVID and to fight inflation in subsequent periods. Endogenous policy responses bias the estimation of the slope of the Phillips curve when using time-series data, as Fitzgerald and Nicolini (2014) have stressed. In our setting, time fixed effects control for federal policy responses and long-run inflation expectations driven by the monetary policy regime in place, as in Hazell et al. (2022). At the local level, the pandemic may have triggered relevant structural changes, plausibly reflected in heterogeneous natural unemployment rate dynamics across metropolitan areas. The inclusion of MSA fixed effects – allowed to shift across the pre-COVID, COVID, and post-COVID periods – enables us to absorb them, as in McLeay and Tenreyro (2020).

Identification further requires us to distinguish changes in local final-goods inflation and labor market tightness driven by demand from those driven by cost-push shocks. To isolate demand-driven fluctuations in unemployment rates from local labor supply shocks, we construct a shift-share instrument proxying for MSA-level productivity shocks in the tradable intermediate-input sectors (Bartik, 1991). The intuition behind our instrument is that positive productivity shocks in the intermediate-input sector boost labor demand, raising employment and wages. Demand for final goods consequently increases, thereby driving up prices. This mechanism has a differential impact across cities based on the employment shares of their intermediate-input sectors. For instance, a national productivity shock in the manufacturing sector affects demand for consumption goods relatively more in manufacturing-intensive cities like Detroit.

However, positive productivity shocks in the intermediate-input sector also act as cost-saving shocks, decreasing the price at which intermediate inputs are traded nationally and causing final-goods firms to lower prices. Since local relative intermediate-input prices are observable, we address this concern by directly controlling for them in our empirical exercise. This variable also absorbs the impact of commodity price shocks on local inflation channeled through changes in relative intermediate-input prices. This term controls, for instance, for the impact of an increase in prices of internationally traded semiconductors on local inflation transmitted through a higher price of domestically produced cars.

Because of the pandemic, both intermediate-input and final-goods sectors experienced large labor demand fluctuations (Guerrieri et al., 2022). One may therefore worry that the shocks proxied by our instrument are correlated with local productivity shocks in the final-goods sector. To address this concern, we include in our main specification a shift-share control that has the same structure as our instrument and proxies for local productivity shocks in the final-goods sector. As a result, the conditional exogeneity of our instrument stems from national industry-level employment changes in the intermediate-input sectors (Borusyak et al., 2022), plausibly uncorrelated with industry-level aggregates of local labor supply shocks.

We address potential concerns about the validity of our results through several robustness checks. Most importantly, we show that the flattening of the Phillips curve during COVID and its subsequent steepening are not mainly driven by the food, energy, and shelter components of the CPI. In addition, we estimate the slope of the Phillips curve proxying labor market tightness by the vacancy-to-unemployment ratio, in light of recent literature recommending it as a more appropriate measure of economic slack than the unemployment rate.[4] Since MSA-level data on vacancies are not publicly available before 2020, we perform this analysis for the COVID and post-COVID periods only. We find a substantial increase in the slope of the Phillips curve after the pandemic irrespective of the proxy used for labor market tightness.

Our paper fills a relevant gap in the literature on post-COVID inflation dynamics. No other study has yet identified and estimated the slope of the Phillips curve during and after COVID. Using a real-time decomposition of personal consumption expenditure (PCE) inflation,[5]Shapiro (2022a) estimates that demand explains around one-third of the surge in inflation that occurred until April 2022, relative to the pre-pandemic average. Next, Di

[4]See, for instance, Furman and Powell (2021) and Barnichon and Shapiro (2022).

[5]Shapiro (2022b) classifies PCE inflation rates by spending category as either demand- or supply-driven, based on the monthly correlation between unexpected movements in prices and quantities. Such surprises are computed as residuals from a reduced-form, one-month-ahead forecasting model relying on strong identifying assumptions.

Giovanni (2022) uses a model-based approach to quantify that around 60% of the increase in inflation from December 2019 to December 2021 is driven by aggregate demand shocks. Finally, Ball et al. (2022) decompose PCE headline inflation into core inflation and deviations of headline from core. After estimating the Phillips curve with pooled time-series data from 1985 to 2022, they conclude that labor market tightness explains about 2 out of the 6.9 percentage-point rise in inflation that occurred between December 2020 and September 2022.

Within the large literature on the Phillips curve, our work contributes to the strand that combines panel variation and theory to estimate the slope of the Phillips curve (Fitzgerald and Nicolini 2014, Babb and Detmeister 2017, Beraja et al. 2019, McLeay and Tenreyro 2020, Hooper et al. 2020, Fitzgerald et al. 2020, and Hazell et al. 2022). In particular, our paper most closely relates to Hazell et al. (2022), who rely on state-level panel variation of non-tradable inflation and unemployment to show that the US Phillips curve has been flat since the 1970s. They conclude that pre-COVID inflation dynamics were mostly driven by shifting expectations about long-run monetary policy, as opposed to a time-varying slope of the Phillips curve. With respect to their paper, we make a theoretical and an empirical contribution. First, our model additionally accounts for the simultaneous occurrence of severe and unprecedented COVID-related shocks. To do so, we feature a distinct, vertically-linked, production structure, and introduce shocks to the disutility of labor. Our model illustrates that it is possible to use all-items inflation, as opposed to non-tradable inflation, when estimating the slope of the Phillips curve with regional data. Second, we find that the post-COVID US Phillips curve is steep. Therefore, inflation expectations alone do not explain post-COVID inflation dynamics.

The remainder of the article is structured as follows. Section 2.2 describes the model and the derivation of the regional and aggregate Phillips curves. Section 2.3 discusses data sources and presents summary statistics. Section 2.4 introduces the empirical strategy and Section 2.5 shows our main results. Section 2.6 presents the robustness checks. Section 2.7 concludes.

2.2 Vertical Supply Chains and the Phillips Curve

We propose a two-region New-Keynesian model of a monetary union with a common commodity market, an intermediate-input sector, and a final-goods sector in each region. The purpose of the model is to derive the regional Phillips curve in an economic environment featuring labor supply shocks as well as commodity and intermediate-input price shocks within vertical supply chains that are relevant to COVID and post-COVID inflation dynamics. We show that the slopes of the regional and aggregate Phillips curves coincide. Our model also demonstrates that time fixed effects control for long-run inflation expectations.

Model Setup

The economy is made of two regions, Home (H) and Foreign (F), which share the same preferences, market structure, and firm behavior. Both regions are characterized by a continuum of population of size ζ and $(1-\zeta)$, respectively. Labor is immobile across regions and perfectly mobile across sectors within a region. A common monetary authority sets interest rates following a Taylor rule, featuring a long-run inflation target and a consistent unemployment rate target. In its simplest form, the model abstracts from fiscal policy. The representative household in each region consumes final goods, supplies labor, and invests in bonds. Financial markets are assumed to be complete and common across the two regions. Households have CES preferences over final-goods varieties and GHH preferences (Greenwood et al., 1988) over the final consumption good aggregator and labor. We capture labor supply shocks allowing households' disutility of labor to shift exogenously and denote the Frisch elasticity of labor supply by the parameter ϕ. Importantly, GHH preferences imply no income effects on labor supply.

The production side of the economy represents the novelty of our model. We feature three sectors vertically linked to capture the incidence of supply chain disruptions on inflation. We assume that commodities are traded on international markets and their inverse supply curve takes the form $P_t^o = c_t^o O_t$, where P_t^o denotes the commodity price, c_t^o denotes the marginal cost of production and is assumed to be exogenous, and O_t denotes the quantity of commodity produced. Firms operating in the intermediate-input sector use labor and commodities to produce a tradable homogeneous intermediate good, according to a constant return to scale (CRS) Cobb-Douglas production function characterized by region-specific technology. The intermediate input produced by local representative firms is traded on a perfectly competitive national market. Hence, its price is common across regions.

The final-goods sector in each region is characterized by a continuum of firms that use intermediate input and labor to produce non-tradable differentiated consumption goods. Production is carried out according to a CRS Cobb-Douglas technology with region-specific productivity shocks and satisfies local demand. Final-goods firms compete monopolistically, facing Calvo-style frictions in price setting (Calvo, 1983). They set their price equal to a constant markup over a weighted average of current and expected future marginal costs, as with some positive probability they will not be able to change their price in future periods. The price level in each region is an index over final-goods firms' prices. Appendix B presents a formal setup of the model, as well as all derivations.

Regional and Aggregate Phillips Curves

An equilibrium in this economy is an allocation consistent with households' and firms' optimization, the interest rate rule, and market clearing conditions. Log-linearizing the model around a zero-inflation steady state and combining optimal final-goods pricing and households' labor supply conditions, we obtain the following expression for the regional Phillips curve in H:

$$\pi_{Ht} = \beta E_t \pi_{Ht+1} - \kappa \hat{u}_{Ht} + \underbrace{\lambda(1-\alpha)\hat{p}^x_{Ht} + \lambda\alpha\hat{\chi}_{Ht} - \lambda\hat{a}^y_{Ht}}_{\nu_{Ht}}, \tag{2.1}$$

where π_{Ht} is regional inflation, $E_t\pi_{Ht+1}$ captures regional short-run inflation expectations, $\kappa = \lambda\phi^{-1}\alpha$ denotes the slope of the regional Phillips curve, and the parameter $\lambda = \frac{(1-a\beta)(1-a)}{a}$ captures frictions in price setting. We define unemployment in H as $u_{Ht} = 1 - N_{Ht}$. Then, to a first order approximation, $\hat{u}_{Ht} = -\hat{n}_{Ht}$, and the same applies in F. The regional cost-push shock ν_{Ht} is decomposed into three terms. First, $\hat{p}^x_{Ht} = \widehat{\left(\frac{P^x_t}{P_{Ht}}\right)}$ denotes the percentage deviation of the regional relative price of intermediate input (i.e., the ratio between the national intermediate-input price, P^x_t, and the regional price level, P_{Ht}) from its steady-state value. Next, $\hat{\chi}_{Ht}$ represents local shocks to households' disutility of labor, while $\hat{a}^y_{Ht}$ captures local shocks to final-goods sector productivity. Appendix B presents the formal derivation of the regional Phillips curve.

Combining the regional Phillips curves in H and F, we obtain the aggregate Phillips curve

$$\pi_t = \beta E_t \pi_{t+1} - \kappa \hat{u}_t + \underbrace{\lambda(1-\alpha)\hat{p}^x_t + \lambda\alpha\hat{\chi}_t - \lambda\hat{a}^y_t}_{\nu_t}. \tag{2.2}$$

The intuition behind the presence of $\hat{p}^x_{Ht}$ and $\hat{p}^x_t$ in the regional and aggregate Phillips curves is that inflation is increasing in the relative price of intermediate input. Given that the price of intermediate input, P^x_t, is common across regions, an identical absolute intermediate-input price change has a higher (lower) pass-through on regional inflation rates the lower (higher) the regional CPI level. A similar logic applies to the impact of intermediate-input price variations on aggregate inflation in the time series. This term shows how to properly control for the direct effect of supply-side shocks affecting intermediate-input prices (i.e., intermediate-input sector productivity shocks and shocks to marginal costs of commodity production) on final-goods inflation rate.

An important implication of this derivation is that the slopes of the regional and aggregate all-items Phillips curves coincide and are equal to κ. This result is different from the one obtained by Hazell et al. (2022) insofar as they show that the slope of the non-tradable regional and the all-items aggregate Phillips curves coincide. In addition, the coefficients on $\hat{u}_{Ht}$ and $\hat{p}^x_{Ht}$ in our Phillips curve equations are scaled by α and $(1-\alpha)$, respectively, where α denotes the final-goods CRS production function parameter. Both discrepancies reflect differences in the structure of the economy and sector-specific production functions between the two models. These derivations imply that regional Phillips curve estimates using all-items inflation rates as the dependent variable, as done in Fitzgerald and Nicolini (2014) and McLeay and Tenreyro (2020), can still be informative about the slope of the aggregate Phillips curve, provided that the relative intermediate-input price dynamics do not diverge substantially across regions.

From κ to ψ

To estimate the slope of the regional Phillips curve, we follow Hazell et al. (2022) and solve it forward, obtaining

$$\pi_{Ht} = E_t\pi_{t+\infty} - E_t\sum_{j=0}^{\infty}\beta^j\kappa\tilde{u}_{Ht+j} + \underbrace{E_t\sum_{j=0}^{\infty}\beta^j(\lambda(1-\alpha)\hat{p}^x_{Ht+j} + \lambda\alpha\hat{\chi}_{Ht+j} - \lambda\hat{a}^y_{Ht+j})}_{E_t\sum_{j=0}^{\infty}\beta^j\nu_{Ht+j}}, \quad (2.3)$$

where $\tilde{u}_{Ht} = u_{Ht} - E_t u_{Ht+\infty}$ denotes the deviation of the current regional unemployment rate from the expected long-run regional unemployment rate and $E_t\sum_{j=0}^{\infty}\beta^j\nu_{Ht+j}$ denotes the expected present discounted value of all current and future regional cost-push shocks. This expression for the regional Phillips curve is particularly convenient, as it shows how time fixed effects in a panel data setting control for long-run inflation expectations. Indeed, $E_t\pi_{t+\infty}$ is assumed to be common across regions and to depend solely on the monetary policy regime in place.

Assuming that $\tilde{u}_{Ht}$, $\hat{p}^x_{Ht}$, and $\hat{a}^y_{Ht}$ follow an AR(1) process with autocorrelation coefficients ρ_u, ρ_{p^x}, and ρ_{a^y}, the regional Phillips curve takes the form

$$\pi_{Ht} = E_t\pi_{t+\infty} - \psi\tilde{u}_{Ht} + \delta\hat{p}^x_{Ht} - \eta\hat{a}^y_{Ht} + \omega_{Ht}, \quad (2.4)$$

where $\psi = \frac{\kappa}{(1-\beta\rho_u)}$, $\delta = \frac{\lambda(1-\alpha)}{(1-\beta\rho_{p^x})}$, $\eta = \frac{\lambda}{(1-\beta\rho_{a^y})}$, and $\omega_{Ht} = E_t\sum_{j=0}^{\infty}\beta^j\lambda\alpha\hat{\chi}_{Ht+j}$. Equations (2.3) and (2.4) are useful to acknowledge the difference between κ and ψ. κ denotes the effect of current unemployment on current inflation, while ψ denotes the effect of current and expected future deviations of unemployment from its long-run steady state on current inflation. Since unemployment is fairly persistent, ψ is typically larger than κ.

As we lack sufficient forward periods in the post-COVID sample to provide insightful estimates of κ, we estimate ψ only. Our estimates provide an upper bound for the effect of contemporaneous demand-driven labor market tightness on inflation. Indeed, they capture the impact of current and future expected unemployment, and high unemployment today typically implies high expected unemployment in future periods. Since the persistence of unemployment declined after COVID, the estimate of ψ in the post-COVID period is closer to κ than it is in the pre-COVID period. As a result, the Phillips curve steepening that we document based on the estimates of ψ is likely to provide a lower bound than the steepening based on estimates of κ.

The result that the slope of the regional Phillips curve coincides with the slope of the aggregate Phillips curve relies on several assumptions. The estimates we provide therefore might not deliver the exact slope of the aggregate Phillips curve. They do, however, constitute useful empirical moments characterizing post-COVID inflation dynamics.

2.3 Data and Descriptive Evidence

We collect data covering 21 US metropolitan areas from January 1990 to September 2022. The Bureau of Labor Statistics (BLS) provides monthly or bi-monthly MSA-level CPI data. All prices are collected monthly in the New York, Chicago, and Los Angeles metropolitan areas. In other locations, food and energy prices are collected monthly, and the prices of other items are collected bi-monthly. The starting date of CPI data collection differs among the metropolitan areas included in the sample. For a more detailed description of CPI data, we refer to Klenow and Kryvtsov (2008) and Nakamura and Steinsson (2008). We focus on broad item categories, such as all items, all items excluding energy, all items excluding food and energy (i.e., core CPI), all items excluding shelter, goods, and services.

We use these data to construct our dependent variables (i.e., inflation) as 12-month percent differences in the CPI. We linearly interpolate MSA-specific CPI series that are collected bi-monthly to fully exploit the variation of MSA-level unemployment rates and instrumental variables in the COVID and post-COVID samples. Since interpolation introduces measurement error in our dependent variable (i.e., inflation) only, our estimates do not suffer from attenuation bias. Appendix C discusses the properties of the inflation interpolation errors in more detail, showing that they are centered at zero and do not correlate with the instrumental variable used in our empirical strategy. We acknowledge that all inflation measures are potentially subject to various types of error, particularly so during a pandemic that dramatically shifted consumers' habits. In this regard, Reinsdorf (2020) and Cavallo (2020) argue that CPI weights reflecting pre-COVID consumption bundles are likely to bias the measure of inflation during COVID.

Monthly MSA-level labor market data are available through the BLS's Local Area Unemployment Statistics (LAUS). The LAUS program uses non-survey methodologies to estimate the number of employed and unemployed individuals for sub-national areas, using the national not-seasonally-adjusted estimates from the Current Population Survey as controls. LAUS provide MSA-level estimates of the labor force, employment, unemployment, and unemployment rate. We use the unemployment rate as our main independent variable to proxy for labor market tightness.

To construct our shift-share instrument and control variables, we need additional data. For the shift components, we draw monthly data on national employment by industry, starting from January 1990, from the Current Population Survey. For the share components, we construct MSA-level industry employment shares from the 1990 Census, our baseline period. Finally, we collect national producer price index (PPI) data for the manufacturing sector from the BLS from January 1990 to September 2022 to construct the relative intermediate-input price index.

Recent studies have argued that a more suitable measure of labor market tightness is the vacancy-to-unemployment ratio, as it provides superior inflation forecasts for prices and wages than the unemployment rate (Barnichon et al. 2021; Furman and Powell 2021; Barnichon and Shapiro 2022). In the post-COVID period, the US vacancy-to-unemployment ratio has dramatically increased, reaching its highest level since World War II in March 2022 (Michaillat and Saez, 2022). Unfortunately, publicly available MSA-level data on vacancies are not available before 2020. We collect data on city-level online vacancies from the Burning Glass Help Wanted OnLine Index available from January 2020 onward. We use these data to check the robustness of our COVID and post-COVID estimates to measuring labor market tightness through the vacancy-to-unemployment ratio.

The resulting dataset is a panel of MSA-year-month observations. Table B.1 in the Appendix shows pre-COVID, COVID, and post-COVID descriptive statistics of inflation and unemployment rates, as well as the CPI data collection starting date for all MSAs included in the sample. Figure 2.2 reveals a remarkable degree of geographical heterogeneity in inflation across MSAs. As of September 2022, Phoenix-Mesa-Scottsdale (Arizona) was the MSA with the highest 12-month, all-items inflation rate in the US (12.6%), while San Francisco-Oakland-Hayward (California) was the MSA experiencing the lowest inflation rate (5.8%). Unemployment rates instead vary to a lesser extent across MSAs. As of September 2022, Chicago-Naperville-Elgin (Illinois-Indiana-Wisconsin) experienced the highest unemployment rate (4.4%), while Minneapolis-St.Paul-Bloomington (Minnesota-Wisconsin) displayed the lowest figure in the sample (1.9%).

The simple correlation between inflation and unemployment across US metropolitan areas presents clear non-linearities. Figure 2.1 in Section 2.1 shows that MSA-level inflation rates decrease non-linearly in the unemployment rate. That is, the response of the inflation rate to the unemployment rate is higher at low rather than at high unemployment rate levels. This descriptive non-linearity is particularly visible in the COVID and post-COVID periods and motivates our empirical estimation.

2.4 Empirical Strategy

Our empirical exercise aims to estimate the parameter ψ in Equation (2.4) before, during, and after COVID. We define the COVID period as starting in March 2020, when the first COVID cases were reported in the US, and the post-COVID period as starting in March 2021, when real consumption expenditures reverted to their pre-pandemic trend. Figure B.1 in the Appendix shows the time series of US real consumption expenditures from January 2018 to September 2022, as measured by the Bureau of Economic Analysis.

To perform our analysis, we specify the following empirical model:

$$\pi_{it} = \alpha_i + \gamma_t - \psi u_{it} + \delta \hat{p}^x_{it} - \eta z^y_{it} + \omega_{it} \tag{2.5}$$

In our benchmark specification, π_{it} denotes the 12-month, all-items inflation rate in MSA i and year-month t. Using the 12-month inflation rate as dependent variable allows us to eliminate seasonality. α_i denotes MSA fixed effects, absorbing time-invariant characteristics of metropolitan areas, such as differences in long-run economic fundamentals across cities. MSA-specific constant terms are allowed to shift between the three periods to control for structural changes at the MSA level brought about by the pandemic. γ_t denotes year-quarter fixed effects, absorbing aggregate shocks, such as endogenous fiscal and monetary policies. As we show in Section 2.2, the inclusion of time fixed effects is essential to difference out common beliefs about the long-run monetary policy regime in place, a major determinant of sudden fluctuations in inflation (Sargent, 1982). u_{it} denotes the unemployment rate in city i and year-month t.

Identifying the parameter ψ in Equation (2.5) further requires u_{it} to be uncorrelated with regional cost-push shocks that might bias OLS estimates of Equation (2.5). As we derive formally in the model, cost-push shocks are driven by local relative intermediate-input price, unobserved local labor supply, and local final-goods sector productivity shocks – $\hat{p}^x_{it}$, $\hat{\chi}_{it}$, and $\hat{a}^y_{it}$ in Equation (2.1), respectively. To isolate demand-driven fluctuations in local unemployment rates from unobserved local labor supply shocks, $\hat{\chi}_{it}$, included in the error term ω_{it}, we construct a shift-share instrument capturing productivity shocks in the tradable intermediate-input sectors. Positive shocks have two distinct effects on inflation. They act as local labor demand shifters, thus decreasing unemployment, raising wages, and causing final-goods firms to increase prices. This is the channel we intend to capture through our instrument.

However, productivity shocks in the intermediate-input sector lower marginal costs of production, decreasing the price of intermediate inputs common across regions. As a consequence, local final-goods firms decrease prices. Since intermediate-input prices are observable, we control for their direct incidence on local inflation including $\hat{p}^x_{it}$ in our main specification. This variable is measured as the ratio between the US manufacturing PPI and the CPI in MSA i and year-month t. $\hat{p}^x_{it}$ also absorbs the impact of commodity price shocks on local inflation channeled through changes in relative intermediate-input prices. For instance, this term differences out changes in car prices set by local dealers due to a sudden increase in prices of internationally traded semiconductors that raises marginal costs for domestic car producers. in the Appendix, we show model-based impulse response functions of endogenous variables to a positive productivity shock in the intermediate-input sector, summarizing the mechanisms at the basis of our identification strategy.

Our instrument takes the following form:

$$z_{it}^{x} = \sum_{j=1}^{N^x} \frac{E_{ij1990}}{E_{i1990}} \times \Delta_{3Y} \log E_{USjt},$$

where $j = 1, ..., N^x$ denotes 1990 2-digit tradable intermediate-input Census industries. These industries include agriculture, mining, manufacturing of durable and non-durable goods, wholesale trade, and financial services. $\frac{E_{ij1990}}{E_{i1990}}$ denotes MSA-level industry employment shares measured in 1990, the baseline period in our sample. Finally, $\Delta_{3Y}\log E_{USjt}$ denotes the three-year percentage change in national employment by industry as in Hazell et al. (2022), capturing labor demand shocks in the intermediate-input sectors at business cycle frequencies. Differences in national employment by industry capture short-run shifts in labor demand, while baseline MSA-level industry employment shares measure local exposure to such national shocks.

We address the remaining concern that local unemployment rates are correlated to the productivity shocks of the local final-goods sectors, $\hat{a}_{it}^{y}$. This has likely been the case especially during and after COVID (Guerrieri et al. 2022), when local economies experienced robust labor demand recoveries across all sectors. If local productivity shocks in the final-goods sector correlate with the variation in unemployment rates captured by our instrument, then our estimate of ψ would be biased. As our model illustrates, a positive productivity shock in the final-goods sector increases labor market tightness but also negatively affects final-goods prices. We therefore follow Borusyak et al. (2022) and include in our main specification the shift-share control variable z_{it}^{y}, proxying for productivity shocks in the final-goods sectors in MSA i and year-month t. This variable has the same structure as our shift-share instrument and is constructed using 2-digit Census non-tradable final-goods industries (i.e., construction, retail, business, personal, recreation, and professional services).

The conditional exogeneity of our instrument stems from the shocks (Borusyak et al., 2022) rather than from the shares (Goldsmith-Pinkham et al., 2020). Our identifying assumption is therefore that, conditioning on MSA fixed effects, time fixed effects, $\hat{p}_{it}^{x}$, and z_{it}^{y}, industry-level employment growth rates in the intermediate-input sectors capture labor demand shocks plausibly uncorrelated with industry-level aggregates of regional labor supply shocks. Finally, we instrument the term $\hat{p}_{it}^{x}$ with $\hat{p}_{it}^{x*} = \widehat{\left(\frac{P_t^x}{P_{it-24}}\right)}$ to offset the negative mechanical correlation between $\hat{p}_{it}^{x}$ and the dependent variable π_{it}, induced by the presence of P_{it} in the numerator of π_{it} and in the denominator of $\hat{p}_{it}^{x}$.

Table 2.1 shows the first-stage coefficients and F-statistics for the pre-COVID, COVID, and post-COVID periods. In columns (1) to (3), the dependent variable is the unemployment rate, u_{it}. The instrument z_{it}^{x} significantly predicts labor market tightness, exhibiting negative coefficients across all specifications. The F-statistics indicate that our instruments are relatively strong in all periods. Column (2) shows that $\hat{p}_{it}^{x*}$ is positively correlated with u_{it} during COVID, suggesting that the supply shocks that occurred during the pandemic harmed local labor markets. Moreover, z_{it}^{y} positively and significantly correlates with labor market tightness in the post-COVID sample, pointing to the importance of this control in the aftermath of the pandemic. In columns (4) to (6), we show that the instrument $\hat{p}_{it}^{x*}$ strongly predicts the local relative intermediate-input price, $\hat{p}_{it}^{x}$.

2.5 Results and Aggregate Implications

Demand-Driven Inflation After COVID

Table 2.2 summarizes our main results, documenting that the Phillips curve flattened during COVID and steepened in the aftermath of the pandemic. A naive OLS estimation of the slope of the Phillips curve delivers an almost eightfold steepening from pre- to post-COVID, compared to the more than threefold steepening estimated following our empirical strategy. The results from the OLS specification controlling only for MSA fixed effects are reported in column (1). We allow the MSA-specific constant terms to shift between the three periods to absorb COVID-induced structural changes that occurred at the local level. The estimate of ψ increases from 0.18 before COVID to 1.36 after COVID, dropping to 0.09 in the COVID period.

Adding a control for the relative price of intermediate inputs in column (2) halves the coefficient in the post-COVID period, reflecting the importance of commodity and intermediate-input supply shocks in driving inflation dynamics after the pandemic. In column (3), we further control for year-quarter fixed effects. The inclusion of such a control shrinks the coefficients on u_{it} toward zero in the COVID and post-COVID periods. This result points to the relevance of aggregate shocks (e.g., changes in inflation expectations, endogenous policy responses, etc.) in explaining the recent spike in inflation. The coefficients on $\hat{p}_{it}^{x}$ turn negative in all periods. The reason is the presence of P_{it} (i.e., the CPI in MSA i in period t) in the numerator of π_{it} and in the denominator of $\hat{p}_{it}^{x}$, inducing a negative mechanical correlation between the two variables in the cross-section.

We address the simultaneity between local demand and supply shocks in column (4). To do so, we proceed as follows. First, we isolate demand-driven variations in local unemployment rates from contemporaneous local labor supply shocks instrumenting u_{it} with z_{it}^{x}, our proxy for productivity shocks in the intermediate-input sector. Second, we condition on the shift-share control variable z_{it}^{y} to absorb the productivity shocks of the final-goods sectors. Finally, we instrument $\hat{p}_{it}^{x}$ with $\hat{p}_{it}^{x*}$ to deal with the aforementioned negative mechanical correlation between π_{it} and $\hat{p}_{it}^{x}$. Column (4) reports our preferred estimates of ψ, i.e., 0.25 before COVID, -0.02 during COVID, and 0.85 after COVID. These results indicate that the Phillips curve flattened during the pandemic and steepened by a factor of more than three afterward, relative to the pre-COVID period.

The difference between the post-COVID estimates of ψ in columns (3) and (4) suggests that the instrument effectively addresses potential measurement error of the independent variable and simultaneity concerns. Such biases drive the OLS estimates toward zero and might even deliver positive coefficients in the presence of relevant local supply shocks. Figure 2.3 plots the 12-month inflation rate against the predicted unemployment rate binned residuals, providing a graphical representation of the estimates in column (4). These results also show that the inflation rate during COVID was mostly driven by supply shocks from the commodity and intermediate-input sectors. Indeed, the coefficient on $\hat{p}^x_{it}$ in column (4) increased from 0.06 in the pre-COVID period to 0.33 in the COVID period. In the aftermath of the pandemic, the estimated coefficient is 0.2, indicating that supply shocks might still be playing a relevant role. Considering the most recent historical estimates of ψ from the regional Phillips curve literature, we infer that, since the mid-1970s, the slope of the US Phillips curve has never been as high as in recent times.[6]

Our benchmark estimates imply that demand-driven economic recovery explains about one-fourth of the post-COVID spike in inflation. Between March 2021 and September 2022, all-items inflation increased by 5.6 percentage points, while the unemployment gap decreased by 1.7 percentage points. We define the unemployment gap as the difference between the unemployment rate and the efficient unemployment rate, following Michaillat and Saez (2022). According to their Beveridgean framework, the efficient unemployment rate is affected only by supply shocks. The right panel of Figure B.2 in the Appendix shows the evolution of the unemployment and the efficient unemployment rates from January 2018 to September 2022. The unemployment gap was close to zero before the pandemic, increased dramatically during COVID, and fell below zero from May 2021. Multiplying the change in the unemployment gap (i.e., 1.7%) by our benchmark estimate of the slope of the Phillips curve (i.e., 0.85), we obtain an estimate of the change in inflation imputable to demand factors (i.e., $1.7\% \times 0.85 = 1.4\%$). The remaining variation is attributable to shifts in long-run inflation expectations and supply-side shocks.

Figure 2.4 shows that the demand contribution to the rise in inflation would have been small and statistically insignificant had the slope of the Phillips curve not steepened after COVID. The blue line shows the evolution of the 12-month, all-items inflation rate relative to March 2021, while the red line represents the demand-driven component of this increase, assuming a steepening of the post-COVID Phillips curve. The green line indicates that the same decrease in unemployment would have explained only about 0.4 of the 5.6 percentage-point inflation increase, had the Phillips curve's slope remained unchanged. These observations imply that Phillips curve steepening is quantitatively important in explaining the

[6] The IV estimates of ψ obtained by Hazell et al. (2022) using state-level variation for the pre-1990 and post-1990 periods are 0.42 and 0.33, respectively.

post-COVID increase in inflation.

Heterogeneity of Phillips Curve Steepening Over Time

Labor market tightness reached its highest level since World War II in March 2022 (Michaillat and Saez, 2022), one year into our post-COVID period. Figure B.2 in the Appendix shows that the unemployment rate had dropped to 3.6% by the same time, in line with its pre-pandemic level, and has remained relatively constant after that. Motivated by this fact, we perform a heterogeneity analysis to test the sensitivity of our estimates of ψ to different horizons of the post-COVID period. We estimate the slope of the Phillips curve from March 2021 to March 2022 (when most of the recovery occurred), to June 2022, and to September 2022.

Table 2.3 shows that the slope of the Phillips curve reached its highest level by March 2022, as displayed in column (3). Including subsequent months – columns (4) and (5) – significantly decreases our point estimate of ψ, suggesting that the slope of the Phillips curve might be experiencing a reversion toward pre-pandemic levels in more recent months. Interestingly, inflation dynamics in the early post-COVID period were not significantly affected by supply shocks but by labor market tightness and productivity shocks of final-goods sectors. From March 2022 onward, however, supply shocks have become progressively more relevant, as they were during COVID.

Heterogeneity of Phillips Curve Steepening Across Items

Distinct CPI item categories have not equally contributed to the increase in inflation experienced by the US since March 2021. The right panel of Figure B.3 in the Appendix shows the 12-month inflation rates for goods and services in recent months. The post-pandemic increase in inflation seems driven more by goods than by services. As noted by many, real consumption expenditures for goods have been steadily stationed above their pre-COVID trend in the past two years, while those for services have reached their pre-pandemic levels only in March 2022 (Figure B.3, left panel). Such a dynamic indicates that demand has shifted from services to goods during and after COVID, reflecting a sluggish return to pre-pandemic consumption habits in the aftermath of the recession. A natural extension of the analysis in Table 2.2 would therefore be to investigate the extent to which the post-COVID increase in the slope of the Phillips curve is driven by goods and services, respectively.

The heterogeneity analysis in Table 2.4 shows that the post-COVID Phillips curve steepening is mainly driven by goods rather than services. At the same time, the flattening that occurred during COVID is driven by services rather than goods. Columns (1) to (3) document an exponential increase in the slope of the goods Phillips curve, which grows from 0.12 before COVID to 0.21 during COVID, reaching 2.24 after COVID. Columns (4) to (6) show that there is no significant steepening in the services Phillips curve. Unexpectedly, supply shocks occurring in the commodity and intermediate-input markets affect goods more than services across all periods, especially during and after COVID. These results are consistent with the presence of congestion in the goods market since the onset of the pandemic, with demand stationing above trend and supply struggling to expand.

2.6 Robustness Checks

In this section, we address potential concerns about the validity of our results. First, using all-items inflation as the dependent variable may mislead the interpretation of our results, as labor market tightness is often considered a driver of core inflation only. Indeed, the food and energy components of the CPI are disproportionally more responsive to supply-side shocks (e.g., oil price shocks) and more volatile as a result. Moreover, a large share of expenditures in core inflation is represented by the shelter component of housing services, measured by rents. As the pandemic significantly affected real estate market dynamics within large US cities (Ramani and Bloom 2021; Mondragon and Wieland 2022), we assess the extent to which rents drive our results. We therefore check whether the post-COVID Phillips curve steepening documented with all-items inflation is robust to different inflation measures (i.e., all items excluding energy, core, or excluding shelter).

Reassuringly, Table B.2 in the Appendix shows that the slope of the Phillips curve decreased during COVID and substantially increased afterward, independently of the outcome variable used. The estimates of ψ for all-items inflation excluding energy in column (2) are almost identical to the estimates of ψ for all-items inflation in column (1). Conversely, the estimated coefficients on $\hat{p}^x_{it}$ diverge during and after COVID, being higher and statistically significant only in the specification with all-items inflation as the dependent variable. This result highlights the ability of $\hat{p}^x_{it}$ to control for relevant supply shocks.

The post-COVID estimated slope of the Phillips curve with core inflation in column (3) is slightly smaller than those with all-items and all-items excluding energy. This result suggests that the food component contributed more than other CPI items to the steepening of the Phillips curve. Replicating our back-of-the-envelope calculation to quantify the contribution of demand factors to core inflation in the post-COVID period, we find that they explain 1.2 ($= 1.7 \times 0.71$) of the 5 percentage-point increase in core inflation experienced by the US between March 2021 and September 2022 (i.e., around one-fourth of the variation). Finally, column (4) shows that the estimate of ψ for inflation excluding shelter is close to zero before and during COVID and slightly higher than other estimates after COVID. These results imply that the shelter component contributed to the impact of tightness on inflation before COVID but does not drive the post-COVID Phillips curve steepening.

Second, Furman and Powell (2021) and Barnichon and Shapiro (2022) argue that the unemployment rate underestimates labor market tightness, pointing to the vacancy-to-unemployment ratio as a more suitable measure. We, therefore, assess whether our main result about post-COVID Phillips curve steepening is sensitive to measuring labor market tightness by the vacancy-to-unemployment ratio. Since city-level vacancy data are publicly available only from January 2020 onward, we are able to estimate ψ for the COVID and post-COVID periods only. Table B.3 in the Appendix shows the results of this robustness check with different measures of inflation as dependent variables, instrumenting the vacancy-to-unemployment ratio v_{it} with z_{it}^{x}. The vacancy-to-unemployment ratio has a positive and statistically significant effect on inflation only from March 2021 onward, implying that the Phillips curve steepened after COVID independently of the adopted measure of labor market tightness. Moreover, the coefficient on $\hat{p}_{it}^{x}$ drops in the post-COVID period relative to our benchmark specification, pointing to a less significant impact of supply shocks on inflation once labor market tightness is measured through the vacancy-to-unemployment ratio.

Third, one may worry that the long span of the pre-COVID period covers substantial changes in the slope of the Phillips curve between January 1990 and February 2020. To address this concern, we evaluate the sensitivity of our estimates to different definitions of the pre-COVID period. We consider two alternative starting dates (i.e., January 2000 or 2010), instead of January 1990. Columns (1) to (3) in table B.4 of Appendix A show that the estimates of ψ are fairly stable across pre-COVID period samples. The slope of the Phillips curve estimated from 1990 until the onset of the pandemic represents an upper-bound (i.e., 0.25 relative to 0.10 from 2000 and 0.18 from 2010).

Since our estimates of ψ capture the effect of current and expected future unemployment on current inflation, they could mainly be driven by expectations about local future economic conditions. If we observed short-run inflation expectations at the local level, we could estimate Equation 2.1. In that case, the estimated slope of the Phillips curve would only capture the effect of current unemployment on current inflation, the parameter κ. Coibion and Gorodnichenko (2015) argue that households may form their short-run inflation expectations by observing the changes in prices of salient goods, such as gasoline. We therefore proxy for local short-run inflation expectations by the 12-month MSA-level gasoline inflation rate and include this control in our main specification.

Our estimates are robust to the inclusion of this proxy for local inflation expectations. Table B.5 in the Appendix shows that the only coefficients significantly affected by the inclusion of this control are the ones on $\hat{p}_{it}^{x}$ in the pre-COVID and COVID periods. This likely reflects a mechanical correlation between local gasoline prices and relative intermediate-input prices, driven by oil price dynamics. To the extent that local inflation expectations are influenced by gasoline inflation, they do not seem to drive the steepening of the Phillips curve after COVID.

Finally, in Section 2.4 we explain how we control for the rapid structural changes in local economies brought about by the pandemic. By adding MSA-period fixed effects to our benchmark specification, we allow time-invariant structural economic conditions of cities, such as the natural unemployment rate, to vary across the three periods considered in our analysis (i.e., before, during, and after COVID). If such conditions changed more frequently within periods, however, this would bias our estimates of ψ. We, therefore, estimate Equation (2.5) interacting MSA fixed effects with tighter time fixed effects (i.e., year, year-semester, and year-quarter fixed effects), to absorb higher-frequency local shocks.

Table B.6 in the Appendix shows that our results are qualitatively robust to the inclusion of higher-frequency MSA-time fixed effects. Columns (2) to (4) control for MSA-year, MSA-year-semester, and MSA-year-quarter fixed effects, respectively. The pre-COVID and post-COVID coefficients progressively diverge from column (2) to column (4), while the coefficient during COVID is fairly similar across specifications. If anything, these results reveal a more pronounced steepening of the Phillips curve after COVID when controlling for higher-frequency local shocks. We are cautious, however, in interpreting these results, as the identifying variation comes from higher frequency changes in the unemployment rate and inflation, which might be driven by measurement error.

2.7 Conclusion

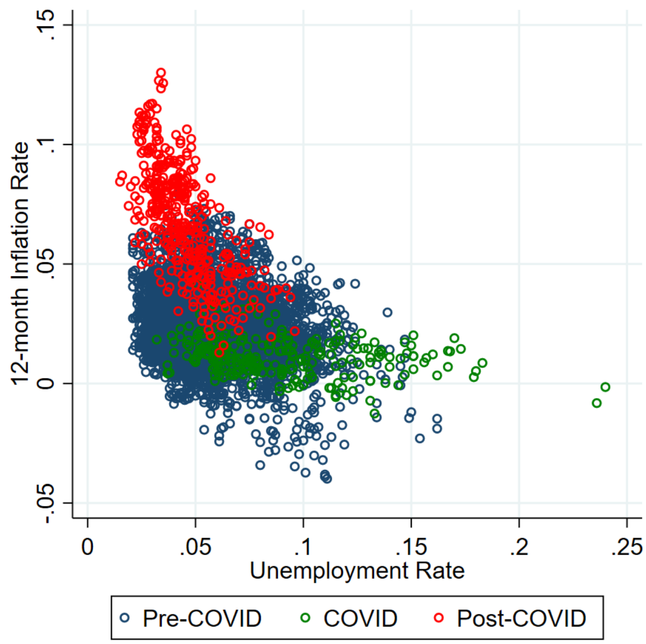
In this paper, we estimate the slope of the Phillips curve before, during, and after COVID to quantify the share of the post-COVID increase in inflation in the US that is attributable to demand-driven economic recovery. To do so, we exploit MSA-level variation in inflation and unemployment combined with an instrumental variable approach. We relate our cross-sectional estimates to the aggregate parameter through a two-region New-Keynesian model of a monetary union. The model features labor supply shocks and a production side with a vertical supply chain (i.e., an international commodity market, a national intermediate-input market, and local final-goods sectors) to capture supply-side shocks relevant to post-COVID inflation dynamics. We derive the regional and aggregate Phillips curves, showing that their slopes coincide.

To our knowledge, this is the first paper to provide quasi-experimental estimates of the slope of the Phillips curve during and after COVID. In our benchmark specification, we estimate the Phillips curve to have flattened during COVID and substantially steepened subsequently. Our estimates show that 1.4 out of the 5.6 percentage-point increase in inflation between March 2021 and September 2022 are due to the contemporaneous decrease in the unemployment gap. Not allowing the slope of the Phillips curve to change across the three periods makes the demand contribution to the recent rise in inflation small and statistically insignificant. Furthermore, we perform a heterogeneity analysis showing that the increase in the slope of the Phillips curve was more pronounced in the early phase of the post-COVID period and was driven mainly by goods rather than services.

Our results point to the presence of non-linearities in the Phillips curve during and in the aftermath of the pandemic. The literature, however, has not yet established the precise mechanisms behind this result. In our model, the slope of the Phillips curve depends on the frequency of price change and the elasticity of labor supply. Structural changes of these parameters potentially explain the non-linearities, although different models can point to alternative channels. For instance, Harding et al. (2022) provide a possible explanation based on a quasi-kinked demand curve. One may also think about a model featuring structural non-linearities in the aggregate supply, as in Eggertsson et al. 2019. Other potential causes are listed in Del Negro et al. (2020). This is an exciting path for future research.

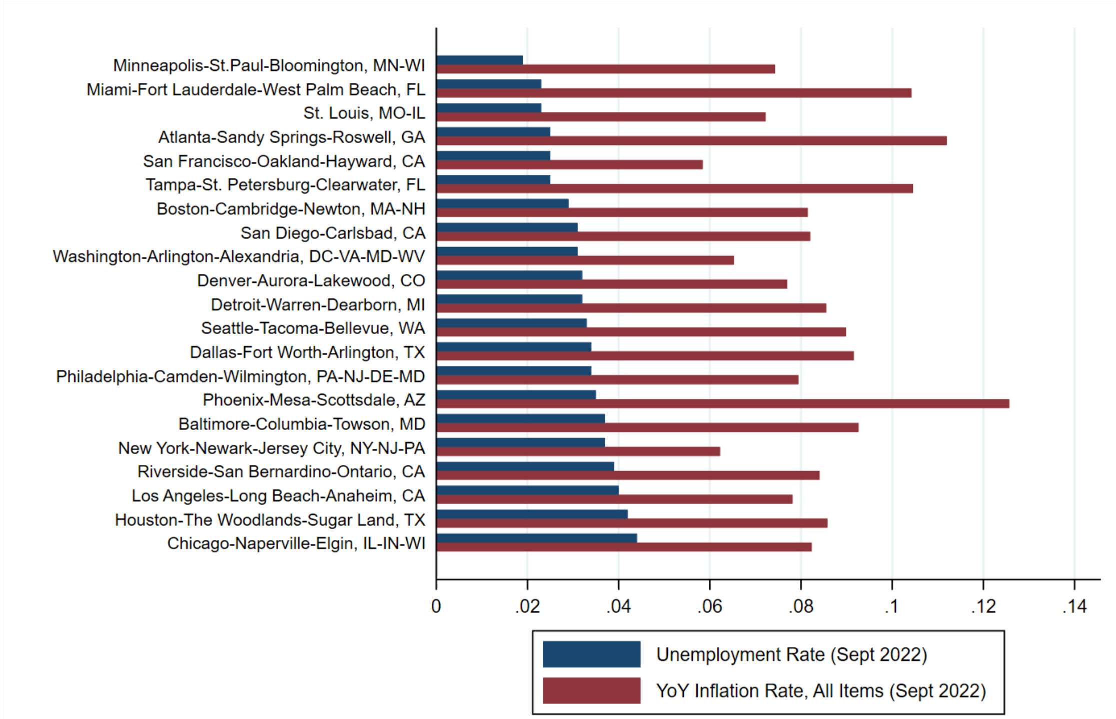
Chapter 2: Figures and Tables

Figure 2.1: The Phillips Correlation Across US Cities



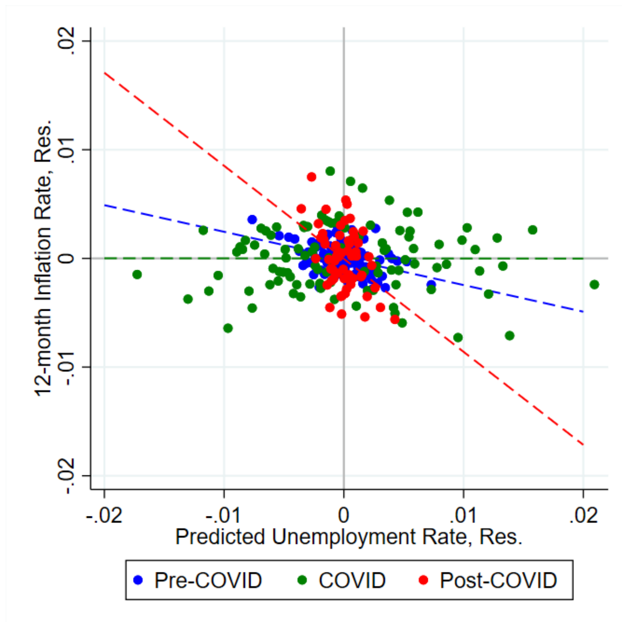
Notes. The scatter plot shows the relationship between the 12-month, all-items inflation rate and the unemployment rate for all observations in our sample. The blue dots denote observations belonging to the pre-COVID period (i.e., Jan 1990-Feb 2020), the green dots denote observations belonging to the COVID period (i.e., Mar 2020-Feb 2021), and the red dots denote observations belonging to the post-COVID period (i.e., Mar 2021-Sep 2022).

Figure 2.2: Unemployment and Inflation Across US Cities



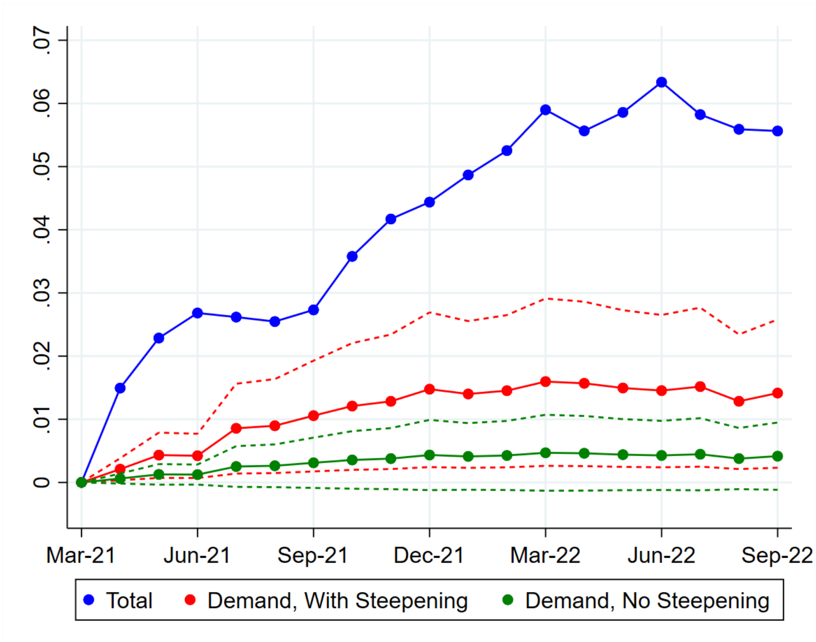
Notes. The bar graph shows the September 2022 unemployment rate (blue bar) and the September 2022 12-month, all-items inflation rate (red bar) by US metropolitan area. Monthly unemployment rates at the MSA level come from the LAUS. Monthly CPI data at the MSA level come from the BLS. Inflation rates are computed as 12-month percent differences of MSA-level CPIs.

Figure 2.3: Estimates of ψ from column (4) of Table 2.2



Notes. This figure provides a graphical representation of our benchmark estimates of ψ before, during, and after COVID (Table 2.2, column (4)). The figure plots binned residuals from a regression of the 12-month, all-items inflation rate on MSA fixed effects, year-quarter fixed effects, the relative intermediate-input price, and the final-goods shift-share control variable z_{it}^{y} against binned residuals of the same specification with predicted unemployment rate as dependent variable. Predicted unemployment rate (i.e., $\hat{u}_{it}$) comes from the first-stage regression using z_{it}^{x} to instrument u_{it}. Blue, green, and red dots denote observations belonging to the pre-COVID, COVID, and post-COVID samples, respectively. The dashed lines represent the best linear fits in the three periods, showing the flattening of the Phillips curve during COVID and its steepening after COVID, relative to the pre-COVID period.

Figure 2.4: 12-month Inflation Rate, Change relative to March 2021



Notes. The figure shows the evolution of the 12-month, all-items inflation rate (blue line), the demand-driven component of this increase with no steepening (green line) and with steepening (red line) of the post-COVID Phillips curve, as reported by the coefficients in Table 2.2, column (4). Between March 2021 and September 2022, all-items inflation increased by 5.6 percentage points, while the unemployment gap – computed following the Beveridgean framework outlined by Michaillat and Saez (2022) – decreased by 1.7 percentage points. The estimates of ψ before and after COVID are 0.25 and 0.85, respectively. The red line indicates that the decrease in unemployment explains 1.4 ($= 1.7 \times 0.85$) out of the 5.6 percentage-point increase in inflation. The green line indicates that the same decrease in unemployment would have explained only about 0.4 ($= 1.7 \times 0.25$) of the 5.6 percentage-point increase in inflation, had the slope of the Phillips curve remained unchanged.

Table 2.1: First Stage Coefficients

	u_{it}			$\hat{p}^x_{it}$		
	(1) Pre-COVID	(2) COVID	(3) Post-COVID	(4) Pre-COVID	(5) COVID	(6) Post-COVID
z^x_{it}	-0.49***	-2.01***	-0.71***	-0.19	-0.05	-0.31
	(0.09)	(0.53)	(0.16)	(0.12)	(0.14)	(0.18)
$\hat{p}^{x*}_{it}$	0.02	0.23**	0.02*	0.86***	0.55***	0.58***
	(0.02)	(0.07)	(0.01)	(0.04)	(0.06)	(0.02)
z^y_{it}	-0.10	0.05	-0.48***	-0.20	0.09**	-0.16
	(0.16)	(0.13)	(0.10)	(0.18)	(0.03)	(0.15)
Observations	5211	252	399	5211	252	399
F-Statistic	25.11	11.80	9.40	273.69	78.27	544.94
MSA-Period FE	✓	✓	✓	✓	✓	✓
Year-Quarter FE	✓	✓	✓	✓	✓	✓

Notes. This table presents the first stage regression coefficients for IV estimation of Equation 2.5. In columns (1) to (3), the dependent variable is the unemployment rate, u_{it}. In columns (4) to (6), the dependent variable is local relative price of intermediate input, $\hat{p}^x_{it}$. The first and fourth columns present the first-stage coefficients for the pre-COVID period (i.e., from January 1990 to February 2020), the second and fifth columns present the first-stage coefficients for the COVID period (i.e., from March 2020 to February 2021), while the third and sixth columns present the first-stage coefficients for the post-COVID period (i.e., from March 2021). The shift-share instrument constructed with tradable intermediate-input industries, z^x_{it}, and intermediate-input price relative to 24-month lagged local CPI, $\hat{p}^{x*}_{it}$, denote the main independent variables. The shift-share variable constructed with non-tradable final-goods industries, z^x_{it}, denotes the main control variable. The specification includes MSA-specific constant terms and year-quarter fixed effects. Standard errors in parentheses are clustered at the MSA level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.2: Estimates of ψ from Equation (2.5)

	(1) OLS	(2) OLS	(3) OLS	(4) IV
Pre-COVID				
u_{it}	-0.18***	-0.29***	-0.20***	-0.25
	(0.03)	(0.04)	(0.04)	(0.15)
$\hat{p}^x_{it}$		0.09***	-0.06***	0.06**
		(0.02)	(0.02)	(0.03)
z^y_{it}				0.13
				(0.09)
COVID				
u_{it}	-0.09***	-0.08***	-0.04	0.02
	(0.02)	(0.02)	(0.02)	(0.07)
$\hat{p}^x_{it}$		0.02	-0.23**	0.33***
		(0.05)	(0.08)	(0.08)
z^y_{it}				0.01
				(0.04)
Post-COVID				
u_{it}	-1.36***	-0.71***	0.10	-0.85**
	(0.15)	(0.12)	(0.13)	(0.34)
$\hat{p}^x_{it}$		0.24***	-0.08	0.20***
		(0.03)	(0.05)	(0.04)
z^y_{it}				-0.14
				(0.16)
Observations	5862	5862	5862	5862
MSA-Period FE	✓	✓	✓	✓
Year-Quarter FE			✓	✓

Notes. This table presents estimates of ψ from Equation (2.5) for the pre-COVID (i.e., from January 1990 to February 2020), COVID (i.e., from March 2020 to February 2021), and post-COVID (i.e., from March 2021) periods. All specifications feature the 12-month, all-item inflation rate as dependent variable and u_{it} as the main independent variable. Columns (1) to (3) display OLS coefficients, column (4) displays IV coefficients. Column (1) controls for MSA fixed effects, allowed to shift across the pre-COVID, COVID, and post-COVID periods. Column (2) adds a control for the local relative price of intermediate input, $\hat{p}^x_{it}$. Column (3) additionally controls for year-quarter fixed effects. Column (4) displays IV estimates of ψ obtained by instrumenting u_{it} with the shift-share instrument z^x_{it}, controlling for the productivity shocks of the non-tradable final-goods sectors, z^y_{it}, and instrumenting $\hat{p}^x_{it}$ with $\hat{p}^{x*}_{it}$. The first-stage coefficients for the specification in column (5) are displayed in Table 2.1. Standard errors in parentheses are clustered at the MSA level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.3: IV Estimates of ψ from Equation (2.5) for different post-COVID periods

	(1) Pre-COVID	(2) COVID	(3) March 2022	(4) June 2022	(5) September 2022
u_{it}	-0.25	0.02	-1.18***	-1.04**	-0.85**
	(0.15)	(0.07)	(0.37)	(0.33)	(0.34)
$\hat{p}^x_{it}$	0.06**	0.33***	0.10	0.21***	0.20***
	(0.03)	(0.08)	(0.06)	(0.04)	(0.04)
z^y_{it}	0.13	0.01	-0.54**	-0.39**	-0.14
	(0.09)	(0.04)	(0.17)	(0.18)	(0.16)
Observations	5211	252	273	336	399
MSA-Period FE	✓	✓	✓	✓	✓
Year-Quarter FE	✓	✓	✓	✓	✓

Notes. This table presents estimates of ψ from Equation (2.5) for the pre-COVID period in column (1) (i.e., from January 1990 to February 2020), for the COVID period in column (2) (i.e., from March 2020 to February 2021), and for different post-COVID periods, i.e., from March 2021 to March 2022 in column (3), from March 2021 to June 2022 in column (4), and from March 2021 to September 2022 in column (5) – our baseline post-COVID period. All specifications feature the 12-month, all-items inflation rate as outcome variable and control for MSA fixed effects (allowed to shift across the pre-COVID, COVID, and post-COVID periods), year-quarter fixed effects, relative intermediate-input prices, and the shift-share control variable z^y_{it}. All columns display IV estimates of ψ obtained by instrumenting u_{it} with the shift-share instrument z^x_{it} and $\hat{p}^x_{it}$ with $\hat{p}^{x*}_{it}$. Standard errors in parentheses are clustered at the MSA level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.4: IV Estimates of ψ from Equation (2.5) for broad CPI categories

	Goods			Services		
	(1) Pre-COVID	(2) COVID	(3) Post-COVID	(4) Pre-COVID	(5) COVID	(6) Post-COVID
u_{it}	-0.12	-0.21**	-2.24***	-0.32	0.10	-0.35
	(0.15)	(0.08)	(0.65)	(0.23)	(0.06)	(0.36)
$\hat{p}^x_{it}$	0.24***	0.65**	0.64***	0.05**	0.08	0.09**
	(0.07)	(0.27)	(0.09)	(0.02)	(0.07)	(0.04)
z^y_{it}	0.08	-0.07	-0.57*	0.12	0.04	0.01
	(0.07)	(0.10)	(0.31)	(0.15)	(0.04)	(0.19)
Observations	5211	252	399	5211	252	399
MSA-Period FE	✓	✓	✓	✓	✓	✓
Year-Quarter FE	✓	✓	✓	✓	✓	✓

Notes. This table presents estimates of ψ from Equation (2.5) for the pre-COVID (i.e., from January 1990 to February 2020), COVID (i.e., from March 2020 to February 2021), and post-COVID (i.e., from March 2021) periods. Columns (1) to (3) use the 12-month goods inflation rate as dependent variable. Columns (4) to (6) use the 12-month services inflation rate as dependent variable. All specifications control for MSA fixed effects (allowed to shift across the pre-COVID, COVID, and post-COVID periods) year-quarter fixed effects, intermediate-input prices relative to the corresponding CPI categories, and the shift-share control variable z^y_{it}. All columns display IV estimates of ψ obtained by instrumenting u_{it} with the shift-share instrument z^x_{it} and $\hat{p}^x_{it}$ with $\hat{p}^{x*}_{it}$. Standard errors in parentheses are clustered at the MSA level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Chapter 3

Balanced Budget Requirements and Local Austerity Multipliers

3.1 Introduction

Fiscal consolidation programs often entail balanced budget requirements (BBRs) for local governments. Such requirements typically consist of zero-deficit rules imposed by central governments to discipline the public finances of sub-national governments and ensure their participation in the consolidation efforts. As shown in Figure 3.1, all EU-27 countries had a BBR in place in 2019 and the share of their aggregate government expenditures of subject to these rules has been constantly increasing over time, reaching around 90% in 2019. Constitutional or statutory limitations restricting the ability of state and local governments to run deficits are also present in the US (Bohn and Inman, 1996).

The literature on BBRs has focused on two crucial questions. First, whether these rules are effective in enforcing fiscal discipline. Second, in case they are effective, what costs they impose in terms of increased output variability (Alesina and Bayoumi, 1996). Answering these questions requires estimating the response of budget surplus and income to a plausibly random assignment of BBRs. Unfortunately, BBRs are not randomly assigned to local governments and detailed longitudinal information about local public finances is hardly available to the public. These challenges have made it difficult for researchers to credibly identify, characterize, and estimate the effects of BBRs on local economic activity. Most quasi-experimental estimates of local fiscal multipliers in the literature range between 1.5 and 1.8 (Chodorow-Reich, 2019), implying a high short-term output cost of fiscal consolidation efforts. However, these estimates are obtained from temporary windfall expansionary shocks rather than permanent fiscal adjustments apt to maintain intertemporal budget balance (Clemens and Miran, 2012). It is reasonable to hypothesize that those multipliers are not symmetric, as governments endogenously seek to minimize the impact of fiscal consolidations on their constituency.

This paper exploits the 2013 extension of tight budget rules to Italian municipalities below 5,000 residents as a quasi-experimental setting to study the impact of BBRs on local public finance and economic activity. We estimate local fiscal multipliers induced by BBRs adopting a novel two-stage least squares (2SLS) difference-in-discontinuities approach. We provide two main results. First, treated municipalities comply with the newly introduced fiscal rules by increasing net municipal budget surplus by 1% of local income. As a result, municipal borrowings also decrease by 1% of local income, indicating that BBRs are effective in disciplining local public finance. To reach this objective, treated municipalities decrease municipal capital expenditures, rather than cutting current outlays or raising taxes. Second, municipal fiscal consolidation has a limited impact on the income level of residents over a six-year horizon. Our baseline estimate of the local fiscal multiplier is 0.25, not significantly different from zero, and we can exclude it is higher than 1.5 with 95% confidence within six years from the shock.

This estimate is smaller than the ones prevailing in the literature (Serrato and Wingender, 2016; Corbi et al., 2019; Shoag, 2013; Pennings, 2021), which are all around or above 1.5. Such differences can be rationalized in several ways. First, through crowding-out effects on firms (Pinardon-Touati, 2021) or "Ricardian" effects on consumers (Clemens and Miran, 2012), which differentiate persistent local budget shocks from transitory windfalls induced by central government spending shocks. Second, the size of the fiscal multipliers depends on numerous characteristics of the policies and the recipient local economies (Ramey, 2016, 2019). For example, Clemens et al. (2022)'s estimates of the windfall multiplier associated with the pandemic federal aid to state and local governments in the US are centered on zero.

Another hypothesis is that fiscal contraction of small municipalities can spill over to neighboring municipalities, an instance that is well recognized in the theoretical literature, suggesting that higher openness entails lower local multipliers (Farhi and Werning, 2016). Especially in the case of a fiscal contraction, local governments might deliberately resort to *beggar-thy-neighbor* policies and concentrate cuts on budget items disproportionately affecting other municipalities. We test this spillover mechanism by estimating spatial spillovers of local fiscal consolidation efforts. Specifically, we examine the extent to which the income of residents in untreated municipalities is affected by fiscal consolidation efforts of neighboring treated municipalities. We estimate a negative but not statistically significant impact, implying a local fiscal multiplier lower than 1.5 even after accounting for potential spatial spillovers. Overall, our results point to relatively low short-term output costs of fiscal consolidation implemented through BBRs imposed on local governments.

This paper relates to two strands of the literature. First, we contribute to the literature on the effect of budgetary shocks on local public finance. The relaxation of budget constraints induces an increase in municipal expenditures (Dahlberg et al., 2008; Adelino et al., 2017), although in some contexts mayors opt for reducing taxes (Grembi et al., 2016). Few papers have studied stricter budget rules, finding that these primarily cause a cut in outlays (Bohn and Inman, 1996; Clemens and Miran, 2012; Daniele and Giommoni, 2021; Coviello et al., 2022).[1] Our findings are in line with the latter stream of the literature, highlighting that cuts are concentrated in capital expenditures (Venturini, 2020; Mühlenweg and Gerling, 2023). Second, we contribute to the literature on local fiscal multipliers by estimating the impact of BBRs on local economic activity in the European context and employing a novel and robust identification strategy. The literature on local fiscal multipliers has reached a wide consensus on estimates ranging between 1.5 and 1.8 (Chodorow-Reich, 2019). Such consensus is based on several studies that estimated the impact of the American Recovery and Reinvestment Act (i.e., ARRA) after the Great Recession, exploiting heterogeneity of Federal spending across US locations. A comprehensive list of these studies include Chodorow-Reich et al. (2012), Feyrer and Sacerdote (2012), Wilson (2012), Conley and Dupor (2013), Dupor and Mehkari (2016), Dube et al. (2018), and Dupor and McCrory (2018). Other studies studying non-ARRA-induced geographical variation in Federal spending find similar estimates overall. For instance, Nakamura and Steinsson (2014) exploit state-level variation in US military spending, estimating a local fiscal multiplier of 1.5.[2] We estimate low and non-significant multipliers, in contrast with the recent literature on local fiscal multipliers, and more in line with earlier evidence on the impact of fiscal rules in the US provided by Alesina and Bayoumi (1996) and Clemens and Miran (2012).

The remainder of the the paper is structured as follows. In Section 2, we describe the institutional setting in which our quasi-experimental study takes place. In Section 3, we discuss the data sources and the identification strategy. Section 4 presents our findings on the impact of BBRs. Section 5 concludes.

3.2 Institutional Setting

Municipalities constitute the lowest level of sub-national government in Italy. The country counts roughly 8,000 municipalities, with a median population of around 2,500 and a mean of around 7,400 in 2011. Each municipality is administered by an elected mayor, an executive body appointed by the mayor, and an elected council. The total amount of municipalities' budgets was around 75 billion Euros in 2004 (5.2% of GDP) and progressively decreased to 57 billion Euros in 2018 (3.2% of GDP). Municipalities provide services within their competence, which include local administration, utilities and waste management, maintenance of public spaces, municipal roads and transportation, school buildings, social housing and services, sports facilities, and small services for tourism and economic development. Revenues come in large part from own fiscal revenues (32%), namely property tax and a surcharge

[1] In addition, Alpino et al. (2022) found changes in the composition of tax revenues, with lower progressivity in tax rates following tighter budget rules.

[2] Other relevant contributions in this literature include Acconcia et al. (2014), Adelino et al. (2017), Corbi et al. (2019), Shoag (2013), Leduc and Wilson (2013), and Serrato and Wingender (2016).

on the income tax, and from non-fiscal revenues (21%), such as fees from building permits, traffic fines, parking and utilities fees. The upper levels of administration – the regions and the central government – contribute to the financing of municipalities by covering on average 37% of municipal revenues with current and capital transfers. Finally, municipalities are also allowed to borrow, as 10% of the budget on average is raised through loans (historically from the public development bank *Cassa Depositi e Prestiti*, but increasingly also from private banks) or issuing bonds.[3]

Since 1999, Italian municipalities have been subject to the so-called Domestic Stability Pact (DSP), which aims at controlling municipal budget deficits. The need for these rules arose as Italy faced challenges in adhering to the limitations imposed by the European Monetary Union (EMU) on member states' general government deficit, defined as the sum of central and local government deficits. Besides debt reduction and compliance with European rules, the central government also aimed at preventing moral hazard from lower levels of government (Alesina and Tabellini, 1990; Vannutelli, 2020). Bail-out or default of lower administrations is not uncommon in Italy,[4] and the risk is worsened by the low salience of municipal finances (Murtinu et al., 2022) and by criminal infiltration (Acconcia et al., 2014; Fenizia and Saggio, 2020).

The rules imposed by the DSP have changed over time, as summarized in Table C.1 in the Appendix. Between 1999 and 2004, the DSP targeted deficit growth, imposing either zero or minimal growth relative to two years before. In the 2005-2006 period, a stricter joint cap on current and capital expenditure was enforced. From 2007 onward, our period of interest, the DSP turned into a proper BBR, initially imposing zero-growth in deficit.[5] From 2011 onward, the DSP became increasingly restrictive, requiring a structural zero-deficit goal. Municipalities which did not comply with the DSP were subject to penalties, including a cap on the growth of current expenditures, bans on new hires and on borrowing to finance investment, a cut in administrators' bonuses and wages, and a reduction of central government transfers. Crucially for our identification strategy, while municipalities below 5,000 residents were exempted from the DSP since 2001, in 2013 the DSP was extended to all municipalities above 1,000 residents.[6] Finally, starting in 2016 the DSP was formally abolished, although a zero-deficit requirement on an accrual basis is still in place.

[3]The remaining revenues are accounted for by clearing entries and transactions on behalf of others, such as retained social security contributions from employees.

[4]For example, in the case of Rome (Law 122/2010), and recently during the COVID pandemic (Law Decree 73/2021). In 2013, the European Court for Human Rights has even imposed remarkable liabilities for credits of defaulted municipalities to Italy (De Luca vs. Italy, 2013).

[5]Note that, from 2008 to 2015, the deficit considered to assess the compliance to the DSP rules started being calculated on a "Mixed basis", meaning that current revenues and expenditures were accounted for on an accrual basis while capital revenues and expenditure were accounted on a cash basis.

[6]Municipalities between 3,000 and 5,000 residents were initially foreseen to be subject to the DSP in 2005 and 2006, but their inclusion was suspended and never reconsidered.

3.3 Data and Identification

Administrative Sources

We collect data from two administrative sources. First, we use balance sheets from Italian municipalities made available by the Italian Ministry of Interior, which contain detailed information about all revenues and expenditures of Italian municipalities from 1998 to 2018. From this dataset, we extract revenues and expenditures on an accrual basis, the breakdown of revenues into fiscal vs. non-fiscal revenues, borrowing and transfers, and the breakdown of expenditures into current and capital ones and by functional destination.[7]

Second, we use data on income tax declarations at the municipality level elaborated by the Italian Ministry of Finance. This source covers all income subject to the standard income tax in Italy declared by individuals every year. Hence, it fails to cover individuals with only income from capital invested in firms with more than one employee, capital income from housing rents, or the informal sector. On average, income reported in income tax declarations corresponds to roughly half of Italian GDP. The information in the dataset includes the total number of declarations, total income declared, income tax due, income from different sources (i.e., labor, self-entrepreneur, rents, pensions) and from declarations belonging to different tax brackets.

We build a dataset covering the period 2007-2018 including all municipalities for which it is possible to recover a fiscal code.[8] We then operate three restrictions to obtain our sample of analysis. First, we keep only municipalities from the 15 ordinary regions.[9] Second, we drop municipalities that were merged, and restrict the dataset to municipalities with no missing information in either balance sheet or income data between 2007 and 2018 to obtain a balanced panel. Finally, we keep municipalities having a number of residents between

[7]The format of the balance sheet used by Italian municipalities underwent a change in 2015, which modified the way some of our variables of interest are reported. In Figure C.1 in the Appendix we plot the average value for all our variables of interest across the 2015 discontinuity. No clear discontinuity appears in the relevant variables.

[8]In fact, the correct association of balance sheets to municipalities requires using correspondence tables between municipality balance sheet code and fiscal code, provided by the Italian Ministry of Interior, which fail to cover older municipalities and determines a loss of municipalities in earlier periods. Table C.3 in the Appendix reports descriptive statistics of the dataset obtained by merging our sources. We split descriptive statistics for the 2007-2012, which is our pre-shock period; for 2013-2015, i.e. three years after the shock; and for 2016-2018, i.e. after the format change in balance sheet data.

[9]Even though the DSP applied also to special statute regions of Sardinia and Sicily (Daniele and Giommoni, 2021), regions with special statute are not subject to standardized costs for services, which are taken into consideration for defining penalties in case a municipality does not respect the DSP (Art. 20 D.L. 98/2011). Moreover, weaker budget rules apply to Sardinia and Sicily regional governments (Rapporto 2013, *Corte dei Conti*), so that more fiscal autonomy could be used to transfer larger funds to municipalities that become subject to DSP in 2013. In the Appendix, we show that all our results are robust to the inclusion of Sardinia and Sicily.

3,500 and 6,500 in the 2011 Census, which comprise all the municipalities in the different bandwidths around the threshold of 5,000 we use.[10]

Our main outcomes of interest are the municipal budget surplus net of transfers from other government bodies and total income declared by municipal residents. We measure the net municipal budget surplus as the difference between fiscal and non-fiscal current revenues net of current transfers from other branches of government, plus capital and financial revenues net of capital transfers from other branches of government, minus current and capital expenditures. Transfers are netted out from revenues because these entries are not raised within the municipality, thus they do not constitute a direct loss of income or resources for taxpayers of the municipality. We winsorize outliers in per-capita income and net budget surplus at the 1% level. We express all monetary values in 2012 Euros.

Identification

To estimate the local fiscal multiplier of the DSP extension, we adopt a novel two-stage least squares (2SLS) difference-in-discontinuities approach (Grembi et al., 2016). The DSP was sharply applying to municipalities above 5,000 residents between 2001 and 2012, and was then extended to municipalities with population between 1,000 and 5,000 residents from 2013 onward. Our treatment and control groups are made of municipalities just below and just above the 5,000 residents cutoff, respectively. Treatment group municipalities are, before 2013, comparable in all fundamental characteristics to municipalities above the threshold but differ sharply in BBR assignment and its correlated aspects (Daniele and Giommoni, 2021). However, administrative rules on the composition and election of municipal councils vary around the 5,000 threshold (Gagliarducci and Nannicini, 2013), making the assumptions of a traditional regression discontinuity design fail. Hence, we exploit the longitudinal variation provided by the extension of DSP to difference out these confounders.

Let i denote municipalities and t denote years. We restrict our sample to municipalities with 2011 population in the interval $P_i \in [P_c - b, P_c + b]$, where P_c denotes the 5,000 residents threshold and b denotes the chosen bandwidth. Our specification takes the following form:

$$Y_{it} = \eta_i + \sum_{t \neq 2012} \left(\alpha_t + \beta_t P_i^{\star} + \delta_t D_i P_i^{\star} \right) + \gamma D_i T_t + \varepsilon_{it} \tag{3.1}$$

where η_i denotes municipality fixed effects, α_t denotes time fixed effects, D_i is a dummy variable capturing treatment status (i.e., 2011 population below 5,000 residents), T_t denotes

[10] Table **??** in the Appendix reports descriptive statistics for the sample obtained. Throughout the paper, we use 2011 population as that is the one legally binding at the time of the DSP exension. 2011 is also the latest Census before DSP extension, hence population is more precisely measured and not dependent on municipalities' birth registries as an intra-Census source.

a dummy taking value 1 for all time periods after 2013, and $P_i^\star = P_i - P_c$ denotes normalized municipal population. The coefficients β_t and δ_t partial-out any confounding difference proportional to the normalized municipal population. We allow such impact to vary by group-year and we assume it affects the outcome linearly. The remaining coefficient γ is the difference-in-discontinuity estimator capturing the impact of budget balance requirements from DSP on the outcome variable Y_{it}. We also use a fully dynamic specification, where we include a set of year-specific treatment dummies $\sum_{t\neq 2012}\gamma_t D_i \tau_t$ instead of $\gamma D_i T_t$, where τ_t is a dummy for a specific year. In the first-stage regression, Y_{it} is budget surplus as a share of baseline municipal residents' income. In the reduced-form regression, Y_{it} is the log of income normalized by population in 2011 ("per-capita income", thereafter). We present our main estimates using the ±1,000 bandwidth, and we include robustness checks for ±750, ±1,250, and ±1,500 in the Appendix. Figure C.2 in the Appendix shows a map of treatment and control groups in our benchmark specification with 1,000 residents population bandwidth. We cluster standard errors at the municipality level, following Bertrand et al. (2004) and Abadie et al. (2017).

The identifying assumption of our model requires parallel trends in the difference of outcomes of municipalities just above and below the 5,000 residents discontinuity, i.e., Common Trend in Discontinuities (CTD). A failure of our CTD assumption requires not only a sharp difference at the threshold of 5,000 residents, such as mayor's salary (Gagliarducci and Nannicini, 2013), but also that these sharp discontinuities vary significantly over time or have a time-varying impact on our outcomes. No further change in fiscal rules at the 5,000 threshold occurs in the period of our analysis. Moreover, we can test an implication of the CTD assumption, namely that the coefficients $\hat{\gamma}_t$ from our fully dynamic version of specification (3.1) are not significantly different from zero for all years preceding the shock, when $t < 2013$.

Under CTD, the estimated coefficient in the first-stage regression captures the causal effect of the DSP extension on budget surplus as a share of baseline local income, $\gamma_t^{Surplus}$. The same coefficient in the reduced-form regression captures the percentage change in per-capita income caused by the DSP extension, γ_t^{Income}. Following Angrist et al. (1996), the ratio between the reduced-form and the first-stage coefficients is an estimator of the percentage change in local income caused by a unitary increase in budget surplus as a share of baseline local income.

This ratio identifies the local fiscal multiplier induced by the BBR (Nakamura and Steinsson, 2014). Formally:

$$
\begin{aligned}
\frac{\gamma_t^{Income}}{\gamma_t^{Surplus}} &= \mathbb{E}_{i,t}\left(\frac{\ln(Income_{i,t}/POP_{i,2012}) - \ln(Income_{i,2012}/POP_{i,2012})}{Surplus_{i,t}/Income_{i,2012} - Surplus_{i,2012}/Income_{i,2012}}\right) \\
&= \mathbb{E}_{i,t}\left(\frac{\ln Income_{i,t} - \ln Income_{i,2012}}{(Surplus_{i,t} - Surplus_{i,2012})/Income_{i,2012}}\right) \\
&= \mathbb{E}_{i,t}\left(\frac{(Income_{i,t} - Income_{i,2012})/Income_{i,2012}}{(Surplus_{i,t} - Surplus_{i,2012})/Income_{i,2012}}\right) \\
&= \mathbb{E}_{i,t}\left(\frac{Income_{i,t} - Income_{i,2012}}{Surplus_{i,t} - Surplus_{i,2012}}\right)
\end{aligned}
$$

.

3.4 Results

Budget Surplus and Local Income

Table 3.1 reports our difference-in-discontinuities estimates of the effect of the DSP extension on surplus-to-income ratio and local per-capita income from specification (3.1). Columns (1) and (2) provide the first-stage and reduced-form estimated coefficients using a bandwidth of 1,000 residents around the 5,000 threshold. The table also shows the estimated fiscal multiplier, i.e., the coefficient of an IV regression with log per-capita income as the dependent variable and net budget surplus-to-income ratio as the independent variable, instrumented by the DSP extension. The results point to a strong and significant effect of the DSP extension on the net municipal budget surplus run by treated municipalities, which increases by 1% of local income. This result indicates that BBRs were effective in disciplining local public finance and making municipalities participate in the national fiscal consolidation effort. Despite this large increase in municipalities' budget surplus, per-capita income does not react significantly. The estimated coefficient in columns (2) is negative, but is not statistically different from zero and its magnitude is small (i.e., -0.25%). Using our preferred bandwidth, we estimate a local austerity multiplier of 0.25, not significantly different from zero. Standard errors imply that we can exclude wuth 95% confidence that the multiplier is 1.5 or larger.[11]

Figure 3.2 reports the results from the fully dynamic version of specification (3.1), with a selected bandwidth of 1,000 residents.[12] The left panel displays the estimated coefficients $\hat{\gamma}_t$

[11]Table C.5 in the Appendix shows that the results hold for different bandwidths. Moreover, Table **??** in the Appendix proves that the results are robust to limiting the time frame of the analysis to 2015, the year when municipalities' balance sheets format changes. Results are also robust to the inclusion of Sardinia and Sicily in the sample, as shown in Appendix Table C.6.

[12]Figure C.3 in the Appendix shows the same results including Sardinia and Sicily in the sample.

for the fully dynamic first-stage and reduced-form specifications. The vertical red line is set between 2012 and 2013, right before the introduction of the BBR, and all the coefficients are expressed relative to 2012. The blue dots provide striking evidence of the response of municipal net budget surpluses to the DSP extension. After five years of parallel trends, treated municipalities immediately react to the introduction of the BBR by increasing their surplus. Conversely, income remains mostly unaffected. In the right panel, we compute the implied multipliers at different horizons after the shock. The estimated coefficients are consistently around zero, although they become noisier at longer horizons, as the estimated effect on per-capita income becomes less precise. The dashed line is set at 1.5, the lower bound of local fiscal multipliers estimates prevailing in the literature (Chodorow-Reich, 2019). As the figure shows, we can exclude that the multiplier we estimate is greater than or equal to 1.5 with 95% confidence up to a six-period horizon after the introduction of the BBR.

Composition of Municipal Budget Shock

In this section, we focus on the composition of the municipal fiscal adjustment, examining the differential impact of the DSP extension on different balance sheet items. Table 3.2 reports the results of our first-stage regression using different components of the net budget surplus as outcome variables and a population bandwidth of 1,000 residents. Columns (1) and (2) report the impact of the DSP extension on current and capital budget surpluses, respectively. The fiscal consolidation induced by the DSP extension is totally accounted for by an increase in the capital surplus. This result is confirmed by columns (3) to (6), which report the breakdown by total current revenues and expenditures, and total capital revenues and expenditures, as a share of residents' income in 2012. On the one hand, the estimated impact on revenues is positive, but not significant and close to zero, indicating that higher taxes (i.e., current revenues) and higher capital revenues do not explain the increase in net budget surplus. On the other hand, the estimated impact on capital expenditures is negative, large, and extremely significant. In particular, the estimated coefficient in column (6) indicates that capital expenditures decreased by 0.89% of local income, thus explaining most of the increase in capital surplus reported in column (2) (i.e., 0.91% of local income) and of the increase in net budget surplus reported in Table 3.2, column (3) (i.e., 1% of local income).[13] An additional piece of corroborating evidence is reported in column (7), where we estimate the effect on new municipal borrowings. The coefficient is negative, significant, and its magnitude matches exactly the increase in net budget surplus reported in Table 3.2, column (3). Moreover, the coefficients estimated from a dynamic specification displayed in Figure C.4 in the Appendix show that the reduction in borrowing is very stable, following the same dynamic of the net budget surplus. This suggests that the shock to surplus induced by the DSP corresponds to a persistent decrease in capital expenditures and a reduction of municipality borrowings. From these results, we conclude that BBRs are effective in disci-

[13]Figure C.5 in the Appendix shows the coefficients estimated from the dynamic specification.

plining local public finance.[14]

Thanks to the detailed information contained in our dataset, we can further break down the effect of DSP on capital expenditures by budget items, exploiting the thematic categorization of expenditures present in the municipal balance sheets. Expenditure categories are defined based on standardized criteria established by the central government for accountability purposes. Column (4) of Table C.8 in the Appendix reveals that the cut in expenditures is significantly concentrated in Sports Facilities and Urban Planning. These two expenditure items account for about half of the cuts in capital expenditures induced by the DSP extension.

Spillovers

We further investigate whether the increase in net budget surplus in treated municipalities spilled over to neighboring municipalities, and estimate a local fiscal multiplier that accounts for these potential spatial externalities. If local economies are sufficiently interconnected, the DSP extension could have a significantly attenuated impact on the municipality itself, as the effect of the budget cut is spread over a larger area including other municipalities not subject to the treatment. Indeed, the theoretical literature acknowledges that high openness in small local economies can reduce the size of the local fiscal multiplier (Farhi and Werning, 2016). This occurrence is even more likely in the case of a fiscal contraction, as local governments could deliberately try to concentrate cuts on budget items disproportionately affecting other municipalities, rather than their constituencies (i.e., *beggar-thy-neighbor* policies). The fact that municipalities mostly cut capital expenditures, affecting workers not necessarily residing in the municipality, rather than raising taxes on residents, points in this direction.

To formally test for the presence of spillover effects, we restrict our attention to untreated municipalities above 5,000 residents, focusing only on the ones counting up to 15,000 residents to maintain comparability with treated municipalities.[15] For such untreated municipalities, we define a neighborhood O_i including all municipalities in a radius of 20 minutes driving by car, and calculate the share of total income in the neighborhood accounted by municipalities between 1,000 and 5,000 residents, which become subject to the DSP after 2013, denoted D_{O_i}.[16] We provide robustness checks for all results using neighborhoods defined with 15, 25, and 30-minute radii in Appendix Table C.10. Figure C.6 in the Appendix shows an example, with the municipality of Crescentino (Piedmont) surrounded by a mix of municipalities always having DSP, and others switching to DSP in 2013. We can then write down our

[14] Table C.7 in the Appendix re-runs the analysis focusing only on 2007-2015, when municipalities balance sheets format changes, finding similar results.

[15] In Table C.9 in the Appendix, we provide robustness checks for different upper limits to the set of municipalities considered.

[16] The 20-minute radius is in line with evidence that job search declines sharply with geographical distance, making labor markets very local (Manning and Petrongolo, 2017; Marinescu and Rathelot, 2018).

specification as follows, to identify the effect of neighboring municipalities switching to DSP on the municipality at the center of the ring:

$$Y_{it} = \eta_i + \sum_{t \neq 2012} \left(\alpha_t + \beta_t p_{O_i} + \delta_t p_{O_i}^2 + \theta_t p_{O_i}^3 \right) + \gamma D_{O_i} T_t + \varepsilon_{it} \tag{3.2}$$

Equation (3.2) corresponds to a difference-in-differences specification with continuous treatment, where treatment dosage is D_{O_i} representing the share of 2012 economic activity around municipality i that gets affected by DSP extension in 2013, and T_t is a dummy taking value 1 for all years after 2012. Consistently with our difference-in-discontinuities approach in the estimation of main effects, the specification includes municipality fixed effects η_i and time fixed effects α_t. Yet, an important difference is that in the difference-in-discontinuities approach the identifying variation in net budget surplus was coming only from municipalities close to the 5,000 residents threshold, focusing on a narrow bandwidth and linearly controlling for time-varying and group-varying population trends. Conversely, our neighborhoods O_i also include very large municipalities. Neighbors including large municipalities could be on different time trends in terms of budget surplus or income and weigh a lot in the neighborhood-level ratio of total surplus to total income. To account for this potential bias, we allow for flexible (i.e., 3rd-degree polynomial) and time-varying controls of baseline average neighborhood population, p_{O_i}.[17]

In the first-stage regression, Y_{it} is the total surplus of municipalities within a 20-minute drive distance, normalized by their 2012 total income, denoted by $s_{O_i t}$. In this case, our coefficient of interest γ identifies the causal effect of the 2013 DSP extension on neighborhood-level surplus. In the reduced-form regression, Y_{it} is the log of per-capita income in municipality i (i.e., the municipality around which the neighborhood is defined), and γ identifies the change in the income of the municipality at the center of the neighborhood following the DSP extension in the neighborhood, scaled as-if all municipalities in the neighborhood became subject to the DSP. Standard errors are clustered at the municipality level.

Table 3.3 presents our results.[18] Columns (1) and (2) display the estimates without additional controls. The coefficient in column (1) is highly significant and very close in magnitude to the one in Table 3.1 column (3), indicating that switching to DSP leads to an approximately 1.2% increase in net budget surplus in neighboring municipalities. Yet, column (2) suggests that such an increase in budget surplus in neighboring municipalities

[17] An alternative approach would be to focus on neighborhoods including *only* municipalities in a narrow bandwidth around 5,000 residents. This is not feasible in our case, as the number of neighborhoods would be extremely small (e.g., only 38 neighborhoods with *only* municipalities with $5,000 \pm 2,500$ residents), making it impossible to achieve sufficient statistical power.

[18] Results are robust to the inclusion of municipalities from Sardinia and Sicily, as shown in Appendix Table C.11.

does not result in a significant change in income. However, one concern with columns (1) and (2) is the low F-statistic, indicating potential weakness of our instrument. This might be attributed to confounding time-varying factors, such as large regional differences in income growth that are often observed in the Italian context. To address this, in columns (3) and (4), we include region-specific time trends. The F-statistic becomes larger, indicating improved instrument strength, while the results remain consistent. The estimated multiplier is positive and is not significantly different from zero. In our benchmark specification (i.e., columns 3 and 4), we can exclude a multiplier of 1.5 or above with more than 95% confidence, while in columns (1) and (2) we can exclude a multiplier of 1.5 or above with about 90% confidence.[19]

The sum of the multipliers estimated in Table 3.1 and Table 3.3 captures a comprehensive estimate of the impact of extra surplus induced by the extension of BBRs, including spatial spillovers. In other words, if all municipalities in a neighborhood increased budget surplus by 1 percentage point of their income, income in each municipality would decrease by 0.25% due to the direct effect estimated in Table 3.1, and by 0.04% due to the effect of spillovers estimated in Table 3.3 (columns 3 and 4). Therefore, the estimate of the local austerity multiplier comprising spatial spillovers is 0.29, still far below the prevailing estimate of 1.5 in the literature.

3.5 Conclusion

This paper estimates local austerity multipliers when fiscal consolidation is implemented through balanced budget requirements (BBRs) imposed to local governments. To do so, we exploit the 2013 extension of tight fiscal rules to municipalities below 5,000 residents enacted in Italy. We use a dynamic difference-in-discontinuity approach to isolate the effect of budget tightening on local income, thus obtaining a quasi-experimental estimate of the local fiscal multiplier. Our approach only assumes that no confounding variation sharply affecting municipalities just above and below the threshold is present in the period of interest. Under such mild assumption, municipalities above and below the threshold are fully comparable except for BBR assignment.

We find that tighter budget rules result in persistently higher budget surplus net of transfers (1% of local income), mostly driven by cuts in capital expenditures. Such cuts cause a persistent decrease in municipal borrowings and are concentrated in local infrastructures, such as sports facilities and urban planning expenditure categories. We estimate a low and not statistically significant causal effect of BBR-induced austerity policies on local income, with a local fiscal multiplier never significantly different from zero and lower than 1.5 with 95% confidence over a six-year horizon. We also test for the presence of spatial spillovers to

[19] We can also define a fully dynamic specification including $\sum_{t \neq 2012} \gamma_t D_{O_i}$ instead of $\gamma D_{O_i} T_t$, as in Appendix Figure C.7, which confirms our results.

neighboring municipalities, finding similar results.

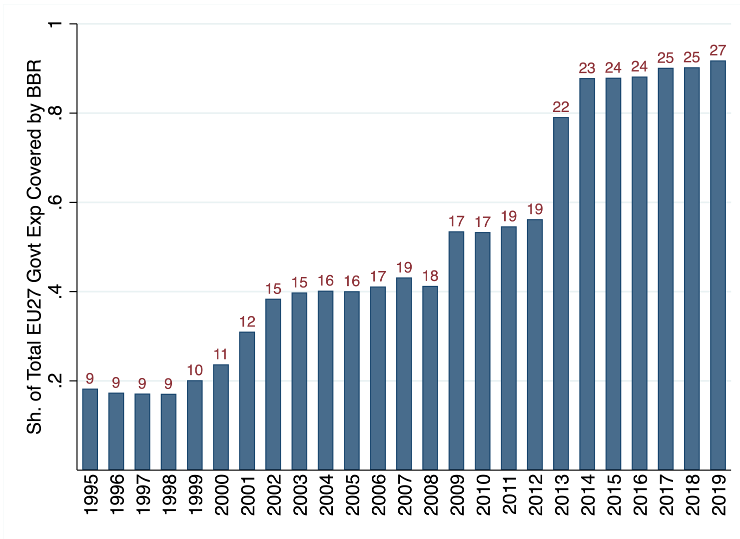
Our findings indicate that the local fiscal multiplier induced by BBRs is lower than the estimates prevailing in the literature on local fiscal multipliers. Such differences may be due to a variety of factors. First, local governments behave differently when they are forced to consolidate the budget relative to when they are allowed to relax it. Grembi et al. (2016) document that relaxing local budgets results in higher deficits and lower taxes, while we find that budget tightening results in lower deficits driven by cuts in capital expenditures. This asymmetry could be driven by economic motives – if lowering taxes is more expansionary than capital spending – or by strategic motives – if taxes are more electorally salient than capital expenditures. We find this question very relevant and potentially interesting for future research.

Second, differently from most studies in the literature about local fiscal multipliers, our shock is not a windfall from the central government, but rather a budgetary shock, which may induce local Ricardian effects (Clemens and Miran, 2012). If lower expenditures today result in lower taxes tomorrow, the negative impact of a permanent decrease in expenditures can at least partially be counterbalanced by higher private spending, thus compressing the multiplier. Recent studies have also shown that when municipalities increase their borrowings, local banks decrease their loans to local firms (Pinardon-Touati, 2021). This crowding-out effect in the capital market may constitute another reason why we estimate a lower multiplier. Finally, fiscal multipliers could be reduced-form approximations of complex economic phenomena, and their magnitude could depend on numerous contingent factors of economies and policies (Ramey, 2016, 2019; Clemens et al., 2022).

Our results suggest that the short-term output cost of BBRs is relatively low. However, they do not exclude other types of adverse effects for the local population, such as a long-run deterioration in local amenities or human capital. In this respect, the literature focusing on the effect of the DSP extension has provided mixed results. Daniele and Giommoni (2021) exclude negative effects of budget tightening on publicly provided goods and services, while Pavese and Rubolino (2021) point to negative effects of lower municipal capital expenditures in schools on students' performance. Overall, our results indicate that effectively enforced BBRs imposed to local governments may be a viable tool to reduce fiscal deficits and increase debt sustainability with relatively low short-term costs for local economic activity.

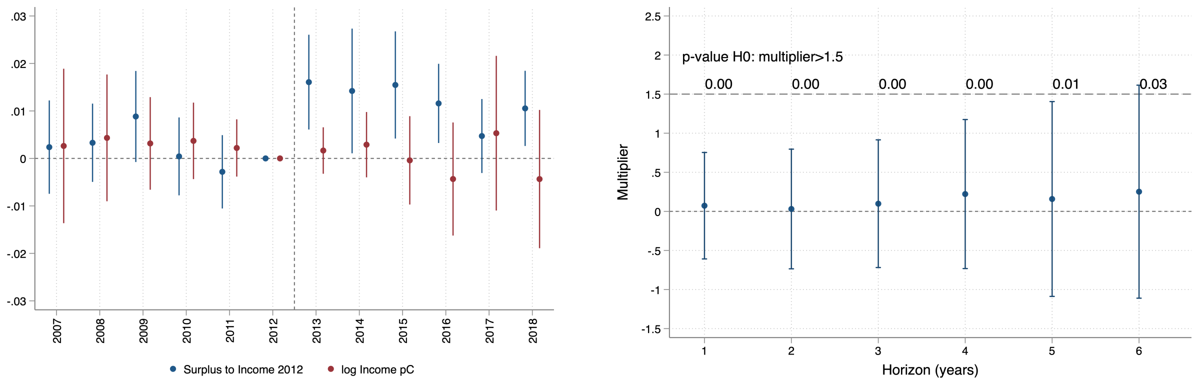
Chapter 3: Figures and Tables

Figure 3.1: Share of Total EU27 Government Expenditures Covered by BBRs



Notes. The figure shows the share of the aggregate EU-27 government expenditures covered by BBRs over time. Each country's share of expenditures covered by BBRs is weighted by the ratio between the country's government expenditures and EU-27 total general government expenditures. On top of each bar, the figure reports the total number of EU-27 countries with at least one BBR in place in that year. Source: Fiscal governance database of the European Commission.

Figure 3.2: Dynamic Effect of DSP Extension on Net Budget Surplus and Per-Capita Income



Notes. The left-hand panel of the figure displays difference-in-discontinuities estimates of the effect of the extension of the Domestic Stability Pact (DSP) to Italian Municipalities below 5,000 residents from 2013 on their net budget surplus and the log of municipal per-capita income. The net budget surplus is scaled by 2012 total income of municipal residents. We report the estimated coefficients $\hat{\gamma}_t$ from specification (3.1) in its fully dynamic form. Standard errors are clustered at the municipality level. The multiplier estimate and its standard errors are displayed on the right-hand panel of the figure. They are the coefficients of an IV regression with log income per-capita as the dependent variable and net budget surplus as the main independent variable, instrumented by the DSP dummy, keeping observations only up to a specific horizon after the shock. The p-values displayed in the right-hand panel of the figure are obtained from one-sided tests for the multiplier being below 1.5.

Table 3.1: Effect of DSP on Surplus and Local Income

	(1) Surplus to 2012 Income	(2) Log Per-Capita Income
$D_i \times T_t$	0.01007***	-0.00254
	(0.00260)	(0.00699)
Observations	8048	8048
R-squared	0.505	0.987
Bandwidth	1000	1000
Years	2007-2018	2007-2018
Mean in 2012	-0.02552	9.39266
Specification		Diff-in-disc
KP F-stat		14.974
Multiplier		.251
		[.694]
H0: Multiplier $\geq$ 1.5		.036

Notes. The table reports difference-in-discontinuities estimates of the effect of the 2013 extension of the Domestic Stability Pact (DSP) to Italian Municipalities between 1,000 and 5,000 residents on their net budget surplus and the log of municipal per-capita income. We report the estimated coefficient $\hat{\gamma}$ from specification (3.1). The table presents results from our benchmark specification with a population bandwidth of 1,000 residents around the threshold of 5,000 residents. Standard errors are clustered at the municipality level. The reported F-statistic on the excluded instrument corresponds to the Kleibergen-Paap F-statistic for weak identification. The multiplier estimate and its standard errors are obtained from an IV regression with log income per-capita as the dependent variable and net budget surplus as the main independent variable, instrumented by the DSP dummy. The last row reports the p-value obtained from the one-sided test for the multiplier being below 1.5. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.2: Composition of the Municipal Budget Shock Induced by DSP Extension

	(1) Curr. Surpl. to 2012 Income	(2) Cap. Surpl. to 2012 Income	(3) Cur. Rev. to 2012 Income	(4) Cur. Exp. to 2012 Income	(5) Cap. Rev. to 2012 Income	(6) Cap. Exp. to 2012 Income	(7) Borrow. to 2012 Income
$D_i \times T_t$	0.00006	0.00914***	-0.00144	-0.00173	-0.00018	-0.00885***	-0.01042***
	(0.00119)	(0.00193)	(0.00179)	(0.00140)	(0.00061)	(0.00204)	(0.00288)
Observations	8048	8048	8048	8048	8048	8048	8048
R-squared	0.635	0.376	0.818	0.903	0.249	0.378	0.519
Bandwidth	1000	1000	1000	1000	1000	1000	1000
Years	2007-2018	2007-2018	2007-2018	2007-2018	2007-2018	2007-2018	2007-2018
Mean in 2012	-0.01077	-0.01504	0.04841	0.05933	0.00539	0.01661	0.00728

Notes. The table reports difference-in-discontinuities estimates of the effect of the 2013 extension of the Domestic Stability Pact (DSP) to Italian Municipalities between 1,000 and 5,000 residents on the different components of their net budget surplus. We report the estimated coefficient $\hat{\gamma}$ from specification (3.1) for several different outcome variables. Specifically, columns (1) and (2) report the impact on current and capital surplus, respectively. Columns (3) to (6) report the impact on current revenues, current expenditures, capital revenues, and capital expenditures, respectively. Finally, column (7) reports the impact on municipal borrowings. All outcome variables are scaled by the 2012 total income of municipal residents. The table presents results from our benchmark specification with a population bandwidth of 1,000 residents around the threshold of 5,000 residents. Standard errors are clustered at the municipality level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.3: Effect on Neighborhood Surplus and Spillovers on Local Income

	(1) Surplus to 2012 Income	(2) Log Per-Capita Income	(3) Surplus to 2012 Income	(4) Log Per-Capita Income
% GDP of Neighb. under DSP	0.01237**	-0.00560	0.01439***	-0.00051
	(0.00412)	(0.00995)	(0.00408)	(0.00930)
Observations	16176	16176	16176	16176
R-squared	0.812	0.990	0.857	0.992
Time trend	-	-	Region	Region
Mean in 2012	-0.02172	9.42025	-0.02172	9.42025
Multiplier		.453		.035
		[.827]		[.646]
H0: Multiplier $\geq$ 1.5		.103		.012

Notes. The table reports the impact of neighborhood-level exposure to the 2013 extensions of the Domestic Stability Pact (DSP) on neighborhood-level net budget surplus and municipal log per-capita income. Columns (1) and (3) report the coefficient $\hat{\gamma}$ from specification (3.2) with neighborhood-level net budget surplus scaled by neighborhood-level income in 2012 as the dependent variable (i.e., first-stage regression). Columns (2) and (4) report the coefficient $\hat{\gamma}$ from specification (3.2) with municipal log per-capita income as the dependent variable (i.e., reduced-form regression). Columns (3) and (4) include region-specific time fixed effects. Standard errors are clustered at the municipality level. The multiplier estimate and its standard errors are obtained from an IV regression with municipal log income per-capita as the dependent variable and the neighborhood-level net budget surplus as the main independent variable, instrumented by the neighborhood-level exposure to the 2013 DSP extension interacted with a dummy taking value 1 for all years after 2012. The last row reports the p-values obtained from one-sided tests for the multiplier being below 1.5. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix A

Appendix of How Big Is the Big Push? The Macroeconomic Effects of a Large-Scale Regional Development Program

A.1 Raw Data

One of the contributions of this study is to construct a panel dataset of CasMez's investments at the municipality level. This Appendix section describes the data sources and the procedure followed to construct this dataset in detail, as well as providing basic descriptive statistics. Data covering the universe of projects financed by CasMez were collected from historical archives, digitized, and made available at https://aset.acs.beniculturali.it/aset-web/. Figure A.1 provides an example of a public infrastructure project in the data. Specifically, it reports information about the majestic Flumendosa dam, built between 1953 and 1958 in Sardinia. The dam forms a large water basin (17 km $\times$ 0.5 km) containing 317 million cubic meters of water, useful for energy production in the region.

The descriptive information provided comprises the project's identification number (i.e., *intervento n.*), a broad sector in which the project is classified (i.e., *settore*), the location (i.e., *ubicazione*), the specific project category (i.e., *categoria*), a brief description of the project (i.e., *descrizione*), the agency responsible for executing the project (i.e., *concessionario*). Importantly, the dataset provides information on the amount approved by CasMez's (i.e., *importo approvato*), as well as subsequent changes to the amount of financial resources deployed by CasMez for the project (i.e., *modifiche concessione*, *perizie suppletive*). Finally, the start date (i.e., *data di inizio lavori*) and the end date (i.e., *data di fine lavori*) of the projects are provided. Figure A.1 inform us that the construction of the Flumendosa dam started in 1953 and ended in 1958. Figure A.2 below displays a picture of the dam in 1959.

The information provided by the dataset is different when it comes to firm grants and financial concessions. Figure A.3 shows the raw information provided regarding each firm grant or financial concession approved by CasMez. Specifically, the figure refers to funds granted (i.e., *contributi a fondo perduto*) to the firm Alfa Romeo Avio, operating in the industry of motor vehicles, for the expansion of the establishment in Pomigliano d'Arco (Naples). This information comprises the identification number and name of the beneficiary (i.e., *beneficiario*), a description of the beneficiary's activity (i.e., *descrizione attività*), the location of the establishment (i.e., *sede*). Importantly, the raw data contain information regarding the amount of funds granted by CasMez (i.e., *finanziamento concesso*), as well as the date in which the grant was approved.

Figure A.3 reports that the grant for the establishment expansion was approved in 1967. Figure A.4 shows a picture of the establishment in 1969. The fact that this establishment is still active today constitutes an interesting piece of anecdotal evidence of the persistent effects of CasMez's investments on Southern industrial production. Figure A.5 indeed, shows the interior of the same establishment in 2022. Other examples of successful industrial clusters in the South of Italy that received funds from CasMez are the aerospace district around Brindisi (Apulia) and the semiconductors district around Catania (Sicily).

A.2 Additional Reduced-Form Results

First-Stage Composition

Recall that CasMez's investments comprise public infrastructure spending and firm grants. Therefore, the first-stage coefficients shown in the main figures are the combination of both types of spending. In this subsection, I show the composition of the first-stage coefficients displayed in Figures 1.7, 1.10, and 1.14. Figure A.6 shows the composition of decade-specific investments and cumulative investments received by municipalities belonging to early IDAs (i.e., treatment group in my first identification strategy) relative to municipalities belonging to late IDAs (i.e., control group in my first identification strategy). About 1/4 of the first-stage coefficient is explained by public infrastructure investments, while 3/4 is explained by firm grants.

Figure A.7 below displays the composition of decade-specific investments received by municipalities belonging to an IDA (i.e., treatment group in my second identification strategy) relative to 1-to-1 matched municipalities (i.e., control group in my second identification strategy). Panel (a) displays group means in public infrastructure investments, while Panel (b) shows group means in firm grants. In this case, the first stage is almost totally driven by firm grants, with public infrastructure investments accounting for small differences in the 1980s and 1990s.

Finally, Figure A.8 shows the composition of decade-specific investments and cumulative investments received by municipalities located just South of CasMez's jurisdiction border (i.e., treatment group in my third identification strategy) relative to municipalities located just North (i.e., control group in my third identification strategy). As in the cause of the first identification strategy, about 1/4 of the first-stage coefficient is explained by public infrastructure investments, while 3/4 is explained by firm grants.

To summarize, the estimated effects of CasMez's investments on local economic activity in two of my three identification strategies identify the joint impact of infrastructure and firm grants, while in the 1-to-1 matched sample, they mostly identify the impact of firm grants. It is important to keep in mind that agglomeration economies influence the amount of resources a municipality receives, as it is a function of firms' investment decisions. Therefore, to the extent that public infrastructure investments increase local productivity, firm grants endogenously increase.

1-to-1 Matched Sample: Estimated Coefficients Plots

In this section, I plot the difference-in-differences coefficients already displayed in Figures 1.10 and 1.11. The main figures report only the simple means of the treatment and the control groups. The advantage of reporting means is that the time trends acting on both groups can be observed, therefore noticing the decrease in manufacturing employment affecting both groups, especially from the 1990s onward. However, plotting the difference-in-differences coefficients has the advantage of showing standard errors, thus providing a sense of the statistical uncertainty around the estimated coefficients. Figure A.9 and A.10 and plots the difference-in-differences coefficients from Equation (1.2). Both first-stage and reduced-form coefficients are statistically significant.

Results on Municipality-Level Human Capital

Some theories of agglomeration economies involve knowledge spillovers, the idea that a higher density of workers increases the productivity of all workers because of social interactions and knowledge transfers (Moretti, 2004). This channel could be at play, especially when agglomeration increases the density of highly qualified workers. Since my main results point to the presence of agglomeration economies, I test this hypothesis by examining the impact of CasMez's investments on the concentration of college-educated individuals in municipalities more exposed to investments. Specifically, I estimate the reduced-form coefficients from Equations (1.1), (1.2), and (1.3) with the college-educated share of the population as the dependent variable.

As shown by Figure A.11, I do not find unequivocal support for a long-term effect of CasMez's investments on municipality-level human capital, as measured by the share of the college-educated population. Panel (a) shows that comparing municipalities belonging to early IDAs with municipalities belonging to late IDAs I find null coefficients. Panel (b) instead shows that comparing municipalities belonging to IDAs to matched municipalities not belonging to IDAs I find a positive long-term effect on the share of the college-educated population. Specifically, this share increased 1.5 percentage points more in IDA municipalities relative to non-IDA municipalities from 1951 to 2011. Finally, Panel (c) shows that the long difference-in-discontinuities design at the border of CasMez's jurisdiction delivers negative long-term effects of CasMez's investments on the share of the college-educated population.

Different hypotheses could be made about the impact of manufacturing-oriented public investments on local human capital both in the short and in the long run. On the one hand, relatively high-paying manufacturing jobs could increase the demand for education from families. Also, manufacturing density could increase the local demand for knowledge-intensive services. This would translate into a positive effect on the share of the college-educated population which, in turn, could drive agglomeration economies in the long run. On the other hand, the specialization of the local economy in manufacturing could increase the demand for non-college-educated workers and crowd out investments in human capital accumulation. This would translate into a negative effect on the share of the college-educated population. The evidence provided does not shed light on which mechanism prevails. However, it casts doubt on the possibility that the persistent effects on manufacturing and total employment estimated consistently across the three identification strategies could be driven by local human capital accumulation.

Cross-Sectoral Agglomeration Elasticities

The reduced-form analysis provides robust evidence of substantial long-run effects of CasMez's investments on manufacturing activity at the local level. Manufacturing employment gains in turn increase employment in other sectors, mainly services. However, the model-based counterfactual analysis reveals that a place-blind allocation of CasMez's investments would have generated larger gains in national manufacturing output. This happens mostly because reallocating factors of production from high-productivity to low-productivity regions exacerbates crowding-out effects and decreases the aggregate gains from the policy. Moreover, I have shown that the Center-North is characterized by stronger agglomeration elasticities in the manufacturing sector than the South.

Expanding the perspective to take into account cross-sectoral spillovers, one could argue that the *within-manufacturing* agglomeration elasticity is lower in the South than in the Center-North but the *cross-sectoral* agglomeration elasticity (i.e., the percent increase in the service sector productivity due to a 1% increase in manufacturing density) might still be stronger in the South than in the Center-North. This would imply that the larger crowding-out effects within the manufacturing sector might be more than compensated by stronger cross-sectoral agglomeration economies in the targeted regions. This in turn would induce aggregate gains.

To test this hypothesis, I estimate Equation (A.1):

$$\ln(\ell_{it}^S) = \kappa_i + \delta_{rt} + \frac{\gamma^{MS}}{\beta} \ln\left(\frac{\ell_{it-1}^M}{A_i}\right) + \mathbf{X}'_{i1951}\Gamma_t + \nu_{it}, \tag{A.1}$$

where $\ln(\ell_{it}^S)$ denotes municipal employment in the services sector, κ_i denotes unit fixed effects, δ_{rt} controls for regional trends, γ^{MS} is the cross-sectoral agglomeration elasticity, $\frac{\ell_{it-1}^M}{A_i}$ is the municipality-level manufacturing employment density, and $\mathbf{X}_{i1951}$ denotes a vector of baseline control variables interacted with time dummies to control for heterogeneous trends induced by differences in size, baseline agglomeration, education levels, and manufacturing concentration across municipalities. I estimate (γ^{MS}/β) separately for the Center-North and the South to test for heterogeneous agglomeration elasticities.

Table A.1 reports the results. Strong cross-sectoral agglomeration economies are present in the South, confirming my reduced-form analysis of positive spillover effects from manufacturing density to services employment. However, agglomeration economies of similar strength are detected in the rest of the country. I estimate a non-significant difference in cross-sectoral agglomeration elasticities between the Center-North and the South. A natural implication of this finding is that heterogeneous cross-sectoral elasticities between the South and the Center-North hardly provide efficiency grounds to motivate regional development programs.

A.3 Alternative Allocation: Place-Blind Program

This section summarizes the results from the model-based analysis simulating the long-run impact of a public investment program of the same size as CasMez but place-blind (i.e., assigning the same investments per capita across regions). To perform this counterfactual analysis, I need to take a stance on the impact of public investments on regional productivity in the Center-North (i.e., η/k_N^P). Since I do not observe k_N^P and k_S^P and I only estimate η/k_S^P, I conservatively assume that $\eta/k_N^P = 1/2 \times \eta/k_S^P$. In practice, this means that the first-order impact of one Euro spent in the Center-North on regional productivity is 50% smaller than in the South, capturing the idea that the Center-North may be more endowed with public capital at baseline.

Figure A.12 compares industrial production under the place-blind program and with no investment. Contrary to the results in Figure 1.16, industrial production increases much less in the South, more in the Center-North, and more in aggregate. The intuition for this result is that by concentrating investments evenly across regions, the program limits crowding-out effects stemming from the reallocation of factors from the Center-North (i.e., the more productive region) to the South (i.e., the less productive region). Figure A.13

shows the present discounted value of industrial production gains accrued between 1951 and 2011 to the South (blue bar), the Center-North (red bar), and the country as a whole (green bar). The figure compares these gains to the present discounted value of the program's costs. The simulation shows that the long-run aggregate industrial production gains from the program would have been larger (i.e., €174 vs. €134 billion). As a consequence, the long-run aggregate multiplier of the program would have been 1.7 instead of 1.3.

A.4 Appendix A: Figures and Tables

Figure A.1: Flumendosa Dam (1952-1958)

intervento n. 010-00001483

settore: BONIFICA INTEGRALE

categoria opera: DIGHE E GRANDI GALLERIE

descrizione: DIGA FLUMENDOSA / ESTERZILI, NURRI, ORROLI, VILLANOVA TULO ED ESCLAPLANO

concessionario: EAF, ENTE AUTONOMO FLUMENDOSA - CAGLIARI

importo approvato: 4.421.388 di cui a carico Cassa: 4.421.388 in data 01/04/1953

primo appalto: 4.266.695 di cui a carico Cassa: 4.266.695

modifiche concessione: 0 di cui a carico Cassa: 0
-.154.692 di cui a carico Cassa: -.154.692

perizie suppletive: 93.347 di cui a carico Cassa: 93.347

revisione prezzi: 524.925 di cui a carico Cassa: 524.925

contributo CEE: 0

data di inizio lavori: 08/06/1953

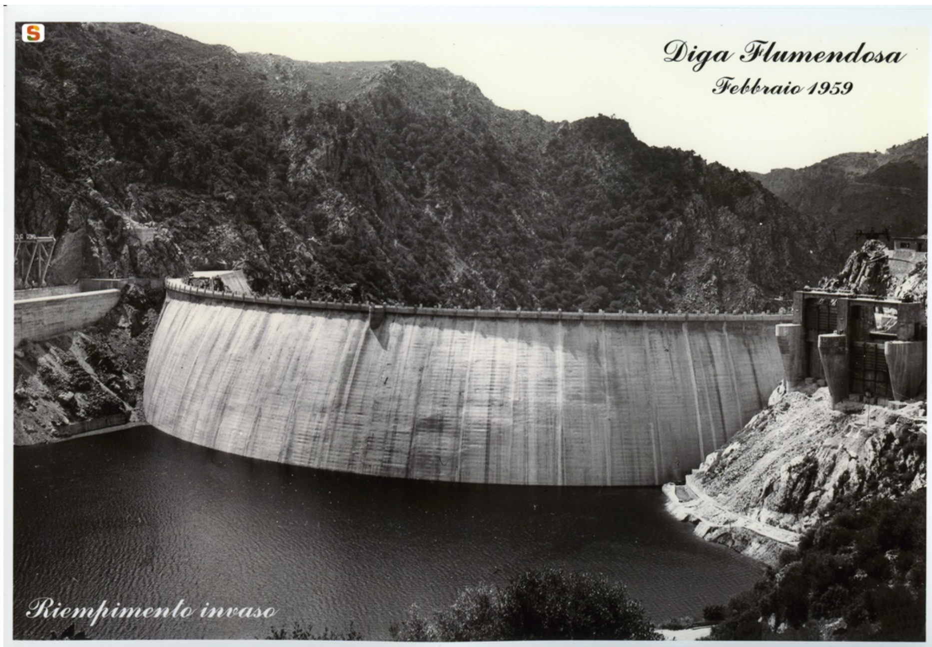
data di fine lavori: 10/06/1958

collaudo: PZ in data 20/03/1964 per un importo di 2.759.895 di cui a carico Cassa: 2.759.895

trasferito alla Regione in data 16/10/1980 (TR.RG.DP66552)

Notes. The figure provides an example of raw data available at https://aset.acs.beniculturali.it/aset-web/. Specifically, the picture summarizes the information regarding the financing of the Flumendosa dam project, carried out in the region of Sardinia between 1952 and 1958.

Figure A.2: Picture of Flumendosa Dam (Sardinia, 1959)



Notes. The figure provides an example of raw data available at https://aset.acs.beniculturali.it/aset-web/. Specifically, the picture shows the Flumendosa dam (Sardinia) in 1959, just after its construction carried out between 1952 and 1958.

Figure A.3: Raw Data: Grants to Alfa Romeo in Pomigliano d'Arco (1967)

Beneficiario: 0017800 - ALFA ROMEO AVIO - SOCIETA' AEROMOTORISTICA PER AZIONI -

Descrizione attività: MECCANICHE AVIO-MOTORI

Sede: POMIGLIANO D'ARCO (Campania, provincia di Napoli)

AREA NAPOLI

Contributi a fondo perduto

01 del 01/01/1967 per ampliamento

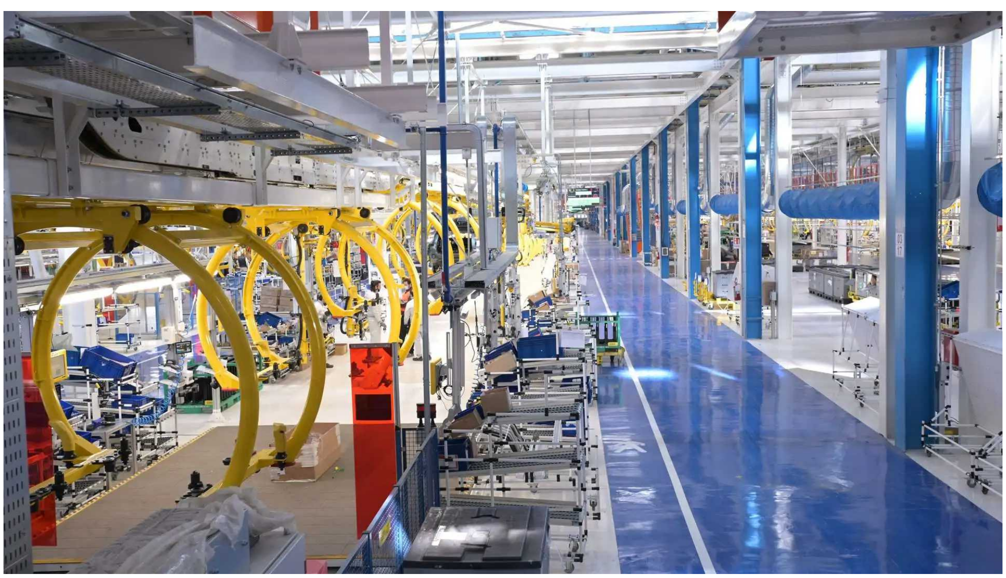
- Istituto istruttore: ISVEIMER
- Investimento: 377.013 di cui 290.248 in impianti
- Spese per opere murarie: 0 di cui ammesse 0
- Spese per macchinari (Centro-Nord): 0 di cui ammesse 645.771
- Spese per macchinari (Sud): 0 di cui ammesse 193.224
- Finanziamento concesso = 1.239.496

Notes. The figure provides an example of raw data available at https://aset.acs.beniculturali.it/aset-web/. Specifically, the figure summarizes the information regarding CasMez's co-financing of an Alfa Romeo Avio establishment expansion in Pomigliano d'Arco (Naples).

Figure A.4: Picture of Alfa Romeo Avio Establishment (Pomigliano d'Arco, 1969)

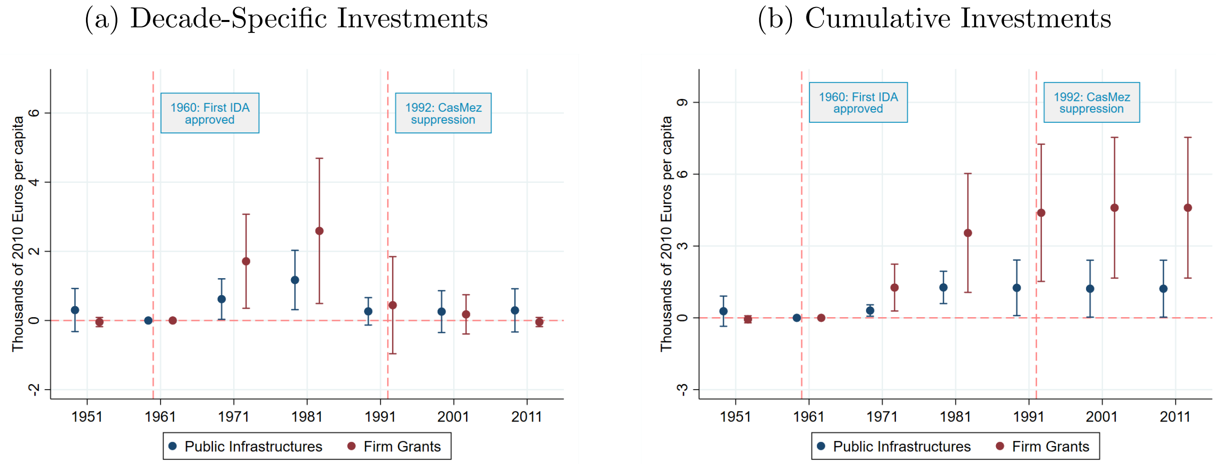
Notes. The figure provides an example of raw data available at https://aset.acs.beniculturali.it/aset-web/. Specifically, it displays the Alfa Romeo establishment in Pomigliano d'Arco (Naples) that received CasMez's funds listed in Figure A.3 in 1969, two years after the grant.

Figure A.5: Picture of Stellantis Establishment (Pomigliano d'Arco, 2022)



Notes. The figure provides an example of raw data available at https://aset.acs.beniculturali.it/aset-web/. pecifically, it displays the Alfa Romeo establishment in Pomigliano d'Arco (Naples) that received CasMez's funds listed in Figure A.3 in 2022.

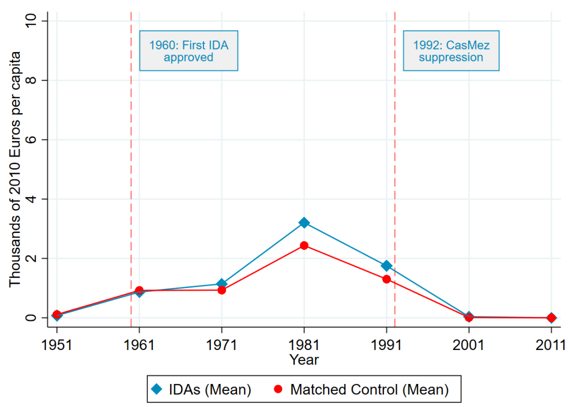
Figure A.6: Early IDAs vs. Late IDAs - First Stage Composition



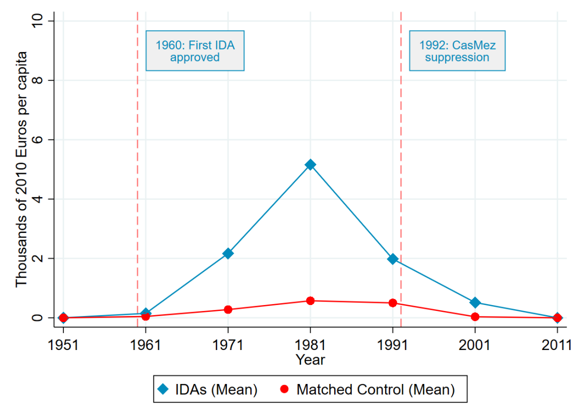
Notes. The figure shows the coefficients $\hat{\beta}_t$ estimated from Equation (1.1). Recall that the unit of observation is a municipality and this dynamic difference-in-differences design compares municipalities belonging to early-approved IDAs (1960-1965) with municipalities belonging to late-approved IDAs (1966-1974). The outcome variable in Panel (a) is decade-specific per-capita investments. while the outcome variable in Panel (b) is cumulative per-capita investments. Investments comprise public infrastructure spending and firm grants and they are converted in per-capita terms by dividing for 1961 municipal population. The blue dots denote the first-stage coefficients on public infrastructure investments. The red dots denote the first-stage coefficients on firm grants. The period assigned to each investment is the year in which the project was approved by CasMez. Observations are weighted by 1961 population and standard errors are clustered at the municipality level.

Figure A.7: 1-to-1 Match - Decade-Specific Investments Composition

(a) Public Infrastructures



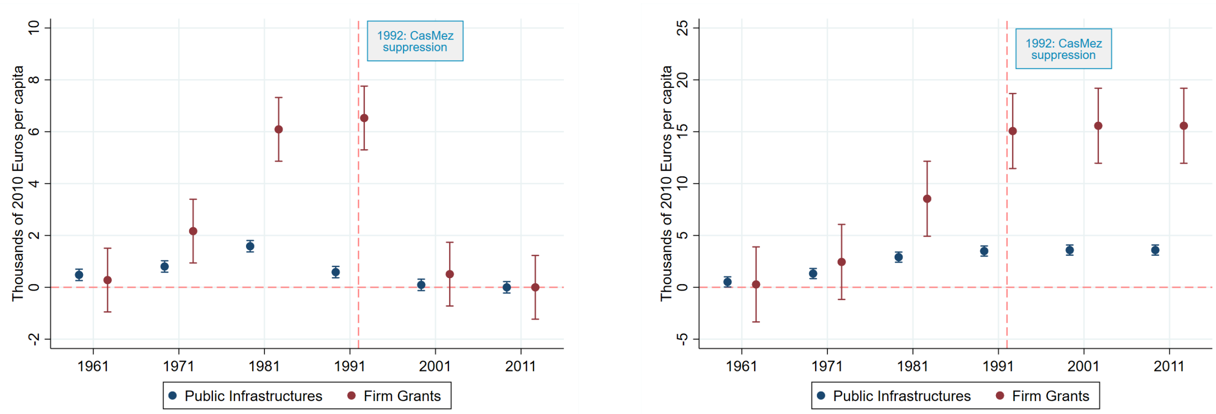
(b) Firm Grants



Notes. The figure shows the coefficients $\hat{\beta}_t$ estimated from Equation (1.2). Recall that the unit of observation is a municipality and this dynamic difference-in-differences design compares each municipality belonging to IDAs with one municipality not belonging to IDAs, matched on 1951 characteristics and 1951-1961 trends. The outcome variable in Panel (a) is decade-specific per-capita investments in public infrastructures, while the outcome variable in Panel (b) is decade-specific per-capita firm grants. Investments are converted in per-capita terms by dividing for 1961 municipal population. The period assigned to each investment is the year in which the project was approved by CasMez.

Figure A.8: Dynamic Long Difference-in-Discontinuities - First Stage Composition

(a) Decade-Specific Investments (b) Cumulative Investments

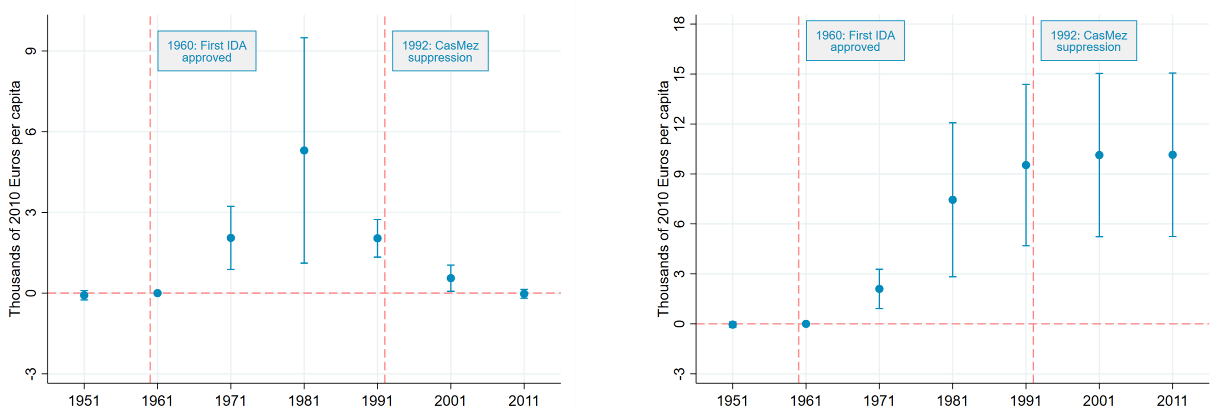


Notes. The figure shows the coefficients $\hat{\beta}_t$ estimated from Equation (1.3). Recall that the unit of observation is a municipality, and this dynamic long difference-in-discontinuities design compares municipalities just South vs. North of CasMez's jurisdiction border. The outcome variable in Panel (a) is decade-specific public infrastructure investments per capita, while in Panel (b) is firm grants per capita. Measures of investments are divided by the 1961 municipal population.

Figure A.9: 1-to-1 Match - First Stage

(a) Decade-Specific Investments

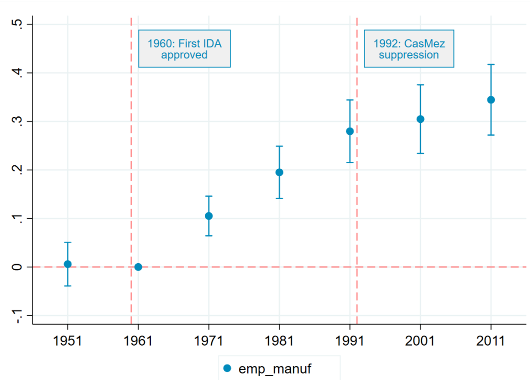
(b) Cumulative Investments



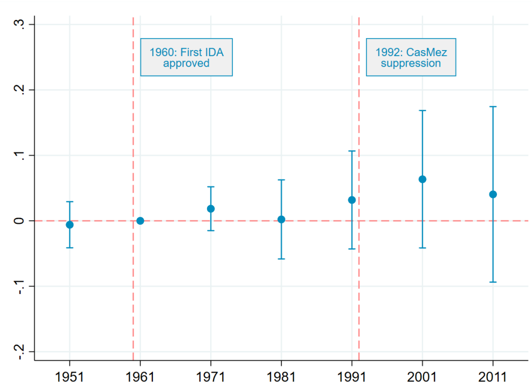
Notes. The figure shows the coefficients $\hat{\beta}_t$ estimated from Equation (1.2). Recall that the unit of observation is a municipality and this dynamic difference-in-differences design compares each municipality belonging to IDAs with one municipality not belonging to IDAs, matched on 1951 characteristics and 1951-1961 trends. The outcome variable in Panel (a) is decade-specific per-capita investments, while the outcome variable in Panel (b) is cumulative per-capita investments. Investments comprise public infrastructure spending and firm grants and they are converted in per-capita terms by dividing for 1961 municipal population. The period assigned to each investment is the year in which the project was approved by CasMez. Observations are weighted by 1961 population and standard errors are clustered at the municipality level.

Figure A.10: 1-to-1 Match - Reduced Form

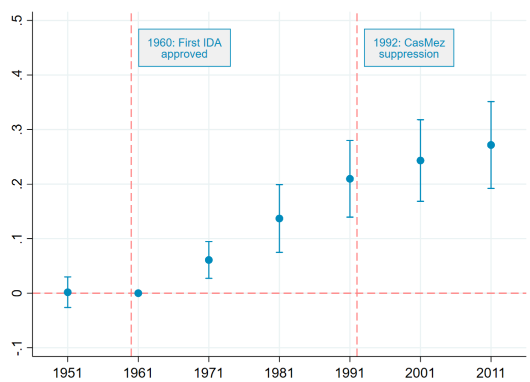
(a) Log Manufacturing Employment



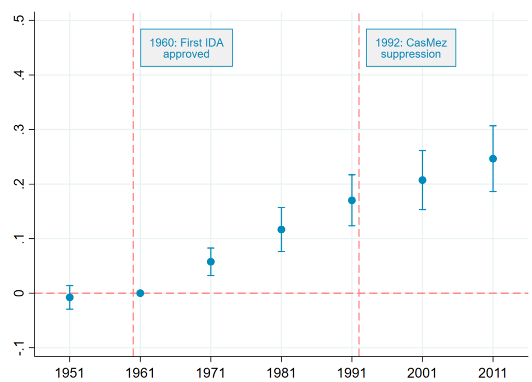
(b) Log Agriculture Employment



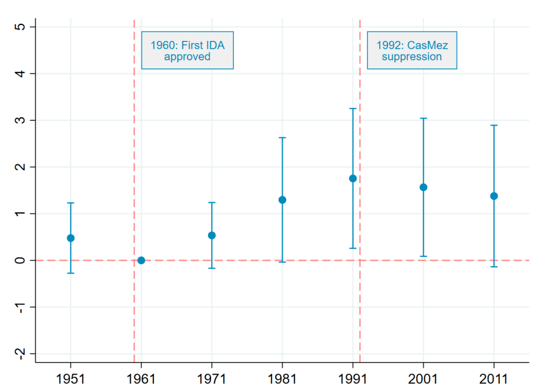
(c) Log Employment



(d) Log Population



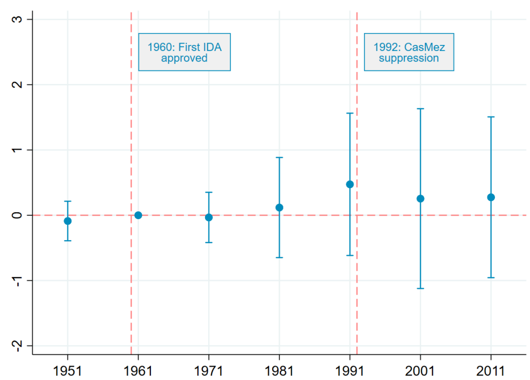
(e) Employment Rate



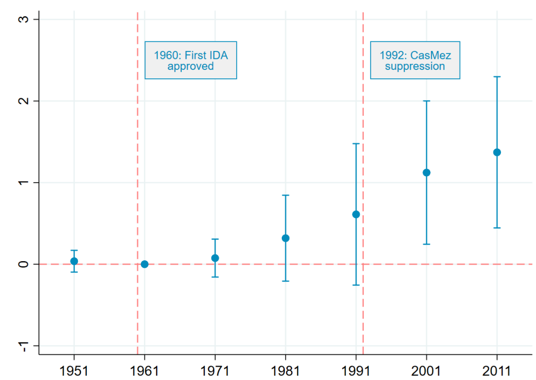
Notes. The figure shows the coefficients $\hat{\beta}_t$ estimated from Equation (1.2) for five different outcome variables. Panel (a): log manufacturing employment; Panel (b): log agriculture employment; Panel (c): log employment; Panel (d): log population; Panel (e): employment rate. Recall that the unit of observation is a municipality and this dynamic difference-in-differences design compares each municipality belonging to IDAs with one municipality not belonging to IDAs, matched on 1951 characteristics and 1951-1961 trends.

Figure A.11: Reduced-Form Coefficients - Share of College-Educated Population

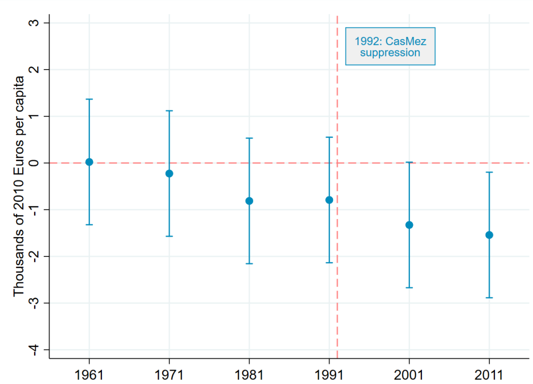
(a) Early IDAs vs. Late IDAs



(b) 1-to-1 Matched Sample



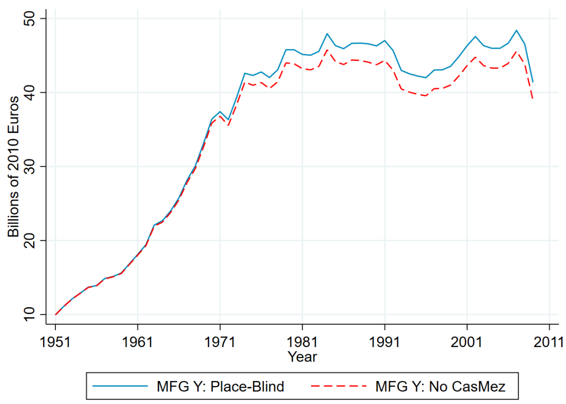
(c) Long Difference-in-Discontinuities



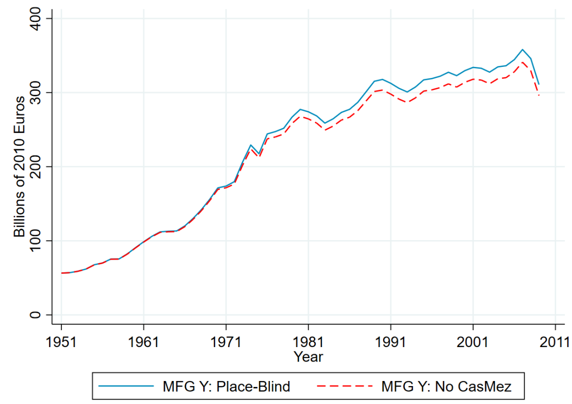
Notes. The figure shows the coefficients $\hat{\beta}_t$ estimated from Equations (1.1), (1.2), and (1.3) with the college-educated share of the population as an independent variable. Each panel reports the coefficient from a distinct empirical strategy. Panel (a): Early vs. Late IDAs; Panel (b): 1-to-1 Matched Sample; Panel (c): Long difference-in-discontinuities. Recall that the unit of observation is a municipality.

Figure A.12: MFG Output: Place-Blind Program vs. No CasMez

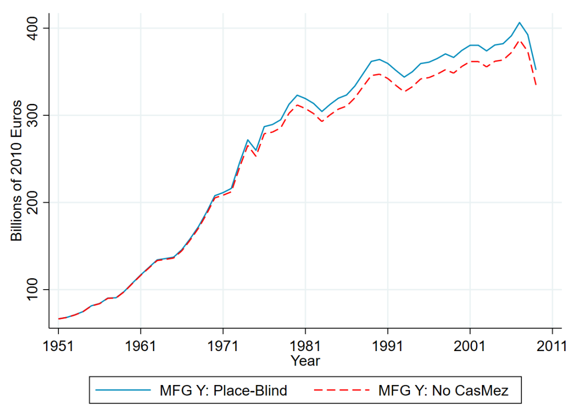
(a) South



(b) Center-North

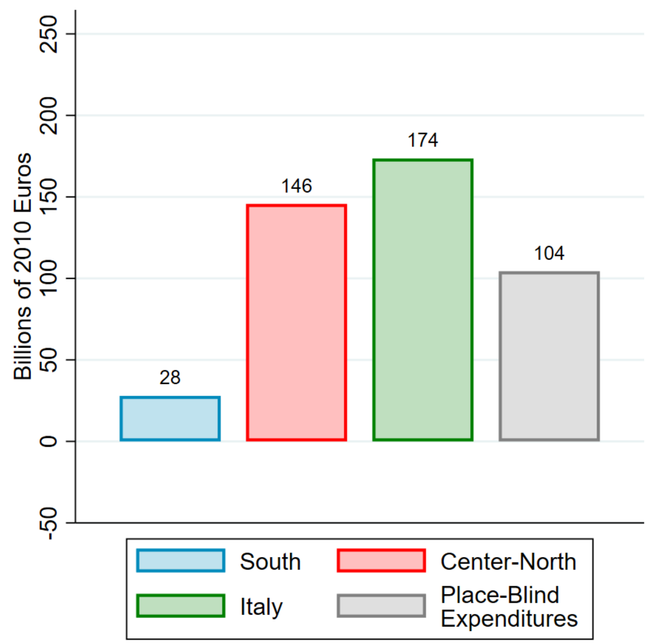


(c) Italy



Notes. The figure shows the simulated counterfactual dynamic of manufacturing employment in the South, the Center-North, and the country as a whole (light blue lines) in response to a place-blind program of the same size as CasMez and the simulated counterfactual dynamic in the absence of CasMez's investments. Panel (a) shows the results of the model-based analysis for the South, Panel (b) for the Center-North, and Panel (c) for the whole country. The unit of measure is millions of workers. The data used for the simulation come from SVIMEZ (2011). The parameters used for the simulation are listed in Table 1.9. These simulations assume that $\eta/k_N^P = 1/2 \times \eta/k_S^P$.

Figure A.13: PDV of Industrial Production Gains and Place-Blind Program's Spending



Notes. The first three bars indicate the net present value, discounted to 1951 with a real annual discount rate of 3%, of the stream of realized gains/losses accrued to the South, the Center-North, and the whole country from a counterfactual, place-blind program, of the same size as CasMez. The last bar displays the net present value, discounted to 1951 with a real annual discount rate of 3%, of CasMez's investments. The calculation ignores all costs other than the operating costs related to the investments, as well as any cost of funds. The unit of measure is billions of 2010 Euros. These simulations assume that $\eta/k_N^P = 1/2 \times \eta/k_S^P$.

Table A.1: IV Estimates of Cross-Sectoral Agglomeration Elasticities

	(1) South	(2) Center-North	(3) Difference
$(\widehat{\gamma^{MS}}/\beta)$	0.583***	0.638***	-0.055
	(0.034)	(0.032)	(0.070)
Observations	13,155	25,555	38,710
Units	2,631	5,111	7,742
First Stage F-Stat	217.81	582.53	
Municipality FE	✓	✓	✓
Region × Time FE	✓	✓	✓
Baseline Controls × Time FE	✓	✓	✓

Notes. The table displays the 2SLS coefficient obtained by estimating Equation (A.1), allowing the coefficient γ^{MS}/β to differ between the Center-North and the South. An observation is a municipality-year and the panel covers the period 1971-2011. The dependent variable is municipal services employment. The main regressor is one-decade-lagged manufacturing employment density. The main regressor is instrumented with two-decade-lagged manufacturing employment density. The table reports the number of observations, the number of unique provinces, and the Kleibergen-Paap F-statistic for weak identification. Baseline controls include log population, the share of manufacturing employment, manufacturing employment density, the share of the illiterate population, and a dummy variable taking value 1 if the municipality belongs to an IDA. Observations are weighted by the 1951 municipal population. Standard errors are clustered at the province level. * ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$).

Appendix B

Appendix of Inflation Since COVID: Demand or Supply

B.1 Model

Households Problem

The representative household in H seeks to maximize

$$E_0 \sum_{t=0}^{\infty} \beta^t u(C_{Ht}, N_{Ht}).$$

Households have CES preferences over varieties and GHH preferences over the final consumption good aggregator and labor, such that

$$u(C_{Ht}, N_{Ht}) = \frac{\left(C_{Ht} - \chi_{Ht} \frac{N_{Ht}^{1+\phi^{-1}}}{1+\phi^{-1}}\right)^{1-\sigma^{-1}}}{1-\sigma^{-1}},$$

where χ_{Ht} is an exogenous variable governing the intensity of disutility of labor, ϕ denotes the Frisch elasticity of labor supply, and

$$C_{Ht} = \left[\int_0^1 C_{Ht}(z)^{\frac{\theta-1}{\theta}} \, dz \right]^{\frac{\theta}{\theta-1}},$$

where $C_{Ht}(z)$ denotes consumption of variety z in H. The parameter $\theta > 1$ denotes the elasticity of substitution between different varieties. The representative household is subject to the following budget constraint

$$\int_0^1 C_{Ht}(z) P_{Ht}(z) \, dz + E_t[M_{Ht,t+1} B_{H,t+1}] \leq B_{Ht} + W_{Ht} N_{Ht} + \int_0^1 \Pi_{Ht}(z) \, dz,$$

where $P_{Ht}(z)$ denotes the price of variety z, B_{Ht} is a random variable denoting payoffs of the state contingent portfolio held in period t, $M_{Ht,t+1}$ is the one-period-ahead stochastic discount factor, W_{Ht} denotes the nominal wage rate, $\Pi_{Ht}(z)$ are the profits of the firm producing variety z. There is a complete set of financial markets across the two regions. To rule out Ponzi schemes, we assume that household debt cannot exceed the present value of future income in any state.

Households in H trade off current consumption, C_{Ht} and current labor supply, N_{Ht}. Given that the utility function is assumed to have a GHH form, the optimal labor supply takes the following form:

$$\chi_{Ht} N_{Ht}^{\phi^{-1}} = \frac{W_{Ht}}{P_{Ht}}, \tag{B.1}$$

where P_{Ht} denotes the lowest cost of purchasing a unit of the composite consumption good C_{Ht}.

Households optimally trade off consumption in the current and in the next periods, as captured by the following Euler equation:

$$\left(C_{Ht} - \chi_{Ht}\frac{N_{Ht}^{1+\phi^{-1}}}{1+\phi^{-1}}\right)^{-\frac{1}{\sigma}} = \beta R_t E_t \left[\left(C_{Ht+1} - \chi_{Ht}\frac{N_{Ht+1}^{1+\phi^{-1}}}{1+\phi^{-1}}\right)^{-\frac{1}{\sigma}} \frac{P_{Ht}}{P_{Ht+1}}\right], \tag{B.2}$$

where R_t is the gross nominal interest rate, common to both H and F. Furthermore, household optimization implies that a standard transversality condition must hold, and that the stochastic discount factor takes a standard form.

Households choose how much to purchase of each variety, $C_{Ht}(z)$, in order obtain the desired level of consumption C_{Ht} at a minimal expense. The minimization problem implies the following demand curve for variety z:

$$C_{Ht}(z) = C_{Ht}\left(\frac{P_{Ht}(z)}{P_{Ht}}\right)^{-\theta} \tag{B.3}$$

and the following price index:

$$P_{Ht} = \left[\int_0^1 P_{Ht}(z)^{1-\theta} dz\right]^{\frac{1}{1-\theta}}. \tag{B.4}$$

The problem is analogous for the representative household in F.

Commodity Sector

Commodities are supplied by an international market, according to the following production process:

$$P_t^o = c_t^o O_t,$$

where P_t^o is the international price of commodities, c_t^o is an exogenous marginal cost shock, and O_t is the quantity of commodity produced.

Intermediate-Input Sector

The intermediate-input sector is tradable and is characterized by perfect competition. Hence, the price of intermediate input, P_t^x, is common across the two regions. The representative firm in H uses commodity O_{Ht} and labor N_{Ht}^x to produce a homogeneous good, according to the following production function

$$X_{Ht} = A_{Ht}^x N_{Ht}^{x\gamma} O_{Ht}^{1-\gamma},$$

where A_{Ht}^X denotes local exogenous technology of the intermediate-input sector. In every period, the representative firm solves the static maximization problem

$$\max_{O_{Ht}, N_{Ht}^x} \; P_t^x A_{Ht}^x O_{Ht} - W_{Ht} N_{Ht}^x - P_t^o O_{Ht},$$

implying the following demands for commodity and labor:

$$P_t^o O_{Ht} = (1-\gamma) MC_{Ht}^x X_{Ht}, \tag{B.5}$$

$$W_{Ht} N_{Ht}^x = \gamma MC_{Ht}^x X_{Ht}, \tag{B.6}$$

where $MC_{Ht}^x = \frac{1}{A_{Ht^x}} \left(\frac{W_{Ht}}{\gamma}\right)^{\gamma} \left(\frac{P_t^o}{1-\gamma}\right)^{1-\gamma}$. The problem for intermediate-input firms in F is analogous.

Final-Goods Sector

The final-goods sector in H is composed by a continuum of monopolistically competitive firms indexed by z. Each firm specializes in the production of a differentiated good consumed locally. The production function is characterized by constant returns to scale

$$Y_{Ht}(z) = A_{Ht}^y X_{Ht}(z)^{1-\alpha} N_{Ht}^y(z)^{\alpha},$$

where A_{Ht}^y denotes local productivity of the final-goods sector, and $X_{Ht}(z)$ and $N_{Ht}^y(z)$ denote, respectively, the quantity of intermediate good and labor used by firm z. Final-goods firm z maximizes

$$E_t \sum_{k=0}^{\infty} M_{Ht,t+k} [P_{Ht+k}(z) Y_{Ht+k}(z) - W_{Ht+k} N_{Ht+k}(z) - P_{t+k}^x X_{t+k}]$$

subject to the production technology and

$$Y_{Ht}(z) = Y_{Ht}\left(\frac{P_{Ht}(z)}{P_{Ht}}\right)^{-\theta},$$

which denotes the demand for its product. The maximization problem takes this dynamic form as, in each period, final-goods producers are able to reset their price only with probability $a < 1$. The optimal choices of labor and intermediate input imply the following demand curves:

$$W_{Ht}N_{Ht}(z) = \alpha MC^{y}_{Ht}Y_{Ht}, \tag{B.7}$$

$$P^{x}_{t}X_{Ht}(z) = (1-\alpha)MC^{y}_{Ht}Y_{Ht}, \tag{B.8}$$

where $MC^{y}_{Ht} = \frac{1}{A^{y}_{Ht}}\left(\frac{P^{x}_{t}}{1-\alpha}\right)^{1-\alpha}\left(\frac{W_{Ht}}{\alpha}\right)^{\alpha}$. If firm z is able to reoptimize its price in t, it will set $P_{Ht}(z)$ to satisfy

$$\sum_{k=0}^{\infty} a^{k}E_{t}\left[M_{Ht,t+k}Y_{Ht+k}(z)\left(\frac{P_{Ht}(z)}{P_{Ht-1}} - \frac{\theta}{\theta-1}RMC^{y}_{Ht+k}\frac{P_{Ht+k}}{P_{Ht-1}}\right)\right] = 0, \tag{B.9}$$

where $M_{Ht,t+k}$ is the stochastic discount factor between period t and $t+k$ and $RMC^{y}_{Ht} = \frac{MC^{y}_{Ht}}{P_{Ht}}$ denotes real marginal costs. Intuitively, the firm will set its price to be equal to a constant markup, $\frac{\theta}{\theta-1}$, over a weighted average of current and expected future marginal costs, as with probability a^{k} the firm will not be able to change price in future period $t+k$. The problem for final-goods firms in F is analogous.

Monetary Authority

The monetary authority implements a common monetary policy across the two regions following the Taylor rule

$$r^{n} = \phi_{\pi}(\pi_{t} - \bar{\pi}_{t}) - \phi_{u}(\hat{u}_{t} - \bar{u}_{t}) + \varepsilon_{rt},$$

where hatted variables represent deviations from a zero-inflation steady state and lower-case variables are logs of upper-case variables. $\pi_{t} = \zeta\pi_{Ht} + (1-\zeta)\pi_{Ft}$ denotes economy-wide inflation, where $\pi_{Ht} = p_{Ht} - p_{Ht-1}$ is consumer price inflation in H and π_{Ft} is the counterpart in F. Within this framework, we define unemployment in H as $u_{Ht} = 1 - N_{Ht}$. Then, to a first order approximation, $\hat{u}_{Ht} = -\hat{n}_{Ht}$, and the same applies in F. Hence, $\hat{u}_{t} = -\hat{n}_{t} = -(\zeta\hat{n}_{Ht} + (1-\zeta)\hat{n}_{Ft})$ denotes the deviation of aggregate unemployment rate from its steady-state value. Finally, $\bar{\pi}_{t}$ represents a time-varying inflation target. We assume that the monetary authority targets an unemployment rate consistent with its long-run inflation target, i.e. $\bar{u}_{t} = \frac{(1-\beta)\bar{\pi}_{t}}{\kappa}$. Finally, ϕ_{π} and ϕ_{u} ensure a unique locally bounded equilibrium, and ε_{rt} denotes a transitory monetary shock, assumed to follow an AR(1) process. The model in its simplest form abstracts from fiscal policy, as the government does not tax, spend, nor issues debt, and monetary policy has no fiscal implications.

Derivation of Regional and Aggregate Phillips Curve

Log-linearazing Equation (B.9) around the zero inflation steady state yields

$$p_{Ht}(z) - p_{Ht-1} = (1 - a\beta) \sum_{k=0}^{\infty} (a\beta)^{\kappa} E_t \left[\hat{mc}_{Ht+k} - (p_{Ht+k} - p_{Ht-1}) \right],$$

where

$$\hat{mc}_{Ht} = -\hat{a}^y_{Ht} + (1 - \alpha)(p^x_{Ht} - p_{Ht}) + \alpha(\hat{w}_{Ht} - p_{Ht}). \tag{B.10}$$

Rearranging the equation, we obtain

$$p_{Ht}(z) - p_{Ht-1} = a\beta E_t \left[p_{Ht+1}(z) - p_{Ht} \right] + (1 - a\beta)\hat{mc}_{Ht} + \pi_{Ht}, \tag{B.11}$$

where π_{Ht} is derived from the definition of the price index in Equation (B.4). Indeed, only $(1 - a)$ firms are able to reset their price, and since they are faced by the same probability of changing price in the future and the same current and expected same marginal costs, they will choose the same price P^*_{Ht}. Hence, the price index becomes

$$P^{1-\theta}_{Ht} = aP^{1-\theta}_{Ht-1} + (1 - a)P^{*1-\theta}_{Ht}.$$

Taking a log-linear approximation of this last expression yields

$$p_{Ht} = ap_{Ht-1} + (1 - a)p^*_{Ht},$$

which implies

$$\pi_{Ht} = (1 - a)(p^*_{Ht} - p_{Ht}). \tag{B.12}$$

Substituting Equation (B.12) in Equation (B.11), after some manipulations we obtain

$$\pi_{Ht} = \beta E_t \pi_{Ht+1} + \lambda \hat{mc}_{Ht}, \tag{B.13}$$

where

$$\lambda = \frac{(1 - a\beta)(1 - a)}{a}.$$

The log-linearized equation of the labor supply is

$$\hat{w}_{Ht} - p_{Ht} = \hat{\chi}_{Ht} + \phi^{-1}\hat{n}_{Ht}. \tag{B.14}$$

Combining Equations (B.10) and (B.14), we get

$$\hat{mc}_{Ht} = -\hat{a}^y_{Ht} + (1 - \alpha)(p^x_{Ht} - p_{Ht}) + \alpha(\hat{\chi}_{Ht} + \phi^{-1}\hat{n}_{Ht}). \tag{B.15}$$

Substituting Equation (B.15) into Equation (B.13), we obtain the regional Phillips curve

$$\pi_{Ht} = \beta E_t \pi_{Ht+1} + \kappa \hat{n}_{Ht} + \underbrace{\lambda(1 - \alpha)\hat{p}^x_{Ht} + \lambda\alpha\hat{\chi}_{Ht} - \lambda\hat{a}^y_{Ht}}_{\nu_{Ht}}, \tag{B.16}$$

where $\kappa = \lambda\alpha\phi^{-1}$ and $\hat{p}^x_{Ht} = p^x_{Ht} - p_{Ht}$. The regional cost-push shock, ν_{Ht}, is decomposed into three terms: $\hat{p}^x_{Ht}$, $\hat{\chi}_{Ht}$, and $\hat{a}^y_{Ht}$.

In order to derive the aggregate Phillips curve, we start by the definition of aggregate inflation, which is

$$\pi_t = \zeta\pi_{Ht} + (1-\zeta)\pi_{Ft}. \tag{B.17}$$

Substituting Equation (B.16) and its foreign counterpart into Equation (B.17), after some manipulation we obtain the aggregate Phillips curve

$$\pi_t = \beta E_t\pi_{t+1} + \kappa\hat{n}_t + \underbrace{\lambda(1-\alpha)\hat{p}^x_t + \lambda\alpha\hat{\chi}_t - \lambda\hat{a}^y_t}_{\nu_t}, \tag{B.18}$$

as

- $\hat{n}_t = \zeta\hat{n}_{Ht} + (1-\zeta)\hat{n}_{Ft}$,
- $p^x_t = \zeta p^x_{Ht} + (1-\zeta)p^x_{Ft}$,
- $p_t = \zeta p_{Ht} + (1-\zeta)p_{Ft}$,
- $\hat{\chi}_t = \zeta\hat{\chi}_{Ht} + (1-\zeta)\hat{\chi}_{Ft}$,
- $\hat{a}^y_t = \zeta\hat{a}^y_{Ht} + (1-\zeta)\hat{a}^y_{Ft}$.

B.2 Interpolation

As discussed in Section 2.3, the CPI data we use from the BLS are made available at a monthly frequency for three metropolitan areas (i.e., Chicago-Naperville-Elgin, Los Angeles-Long Beach-Anaheim, and New York-Newark-Jersey City) and at a bi-monthly frequency for all other MSAs. We linearly interpolate the data for all MSAs for which the CPI is made available at a bi-monthly frequency. On the one hand, by doing so, we obtain a larger sample size, which greatly benefits statistical power for parameter estimation in the post-COVID period, characterized by a narrow time window. On the other hand, our imputed measures of the CPI are affected by measurement error. In this Appendix section, we discuss how large the measurement error we introduce is likely to be and address potential concerns about the correlation between imputed inflation values and our instrument.

To evaluate how large the interpolation measurement error might be, we conduct the following exercise. For the three metropolitan areas for which CPI data are reported at a monthly frequency, we produce interpolated CPI time series, by declaring CPI observations missing in odd (or even) months and imputing them through linear interpolation. Next, we compare the original CPI time series with the interpolated one and compute measurement errors for imputed observations. Figure B.4 compares the original and linearly interpolated time series of the CPI obtained by imputing observations in odd months for Chicago-Naperville-Elgin and Los Angeles-Long Beach-Anaheim from February 2020 to September 2022. The interpolated series matches closely the original ones for both MSAs. Provided that measurement error is not systematically different for imputed observations in Chicago-Naperville-Elgin, Los Angeles-Long Beach-Anaheim, and New York-Newark-Jersey City, imputed observations for all other MSAs should closely approximate true values.

We can construct an interpolated inflation series by computing the 12-month percent difference in the interpolated CPI series. The interpolated inflation series for all MSAs are also characterized by measurement error with respect to the original ones. However, to the extent that such error terms are not systematically correlated with the exogenous instrument z_{it}^{x}, our estimates of ψ should not be biased. Figure B.5 shows the pooled distribution of inflation interpolation errors computed in odd and even months for Chicago-Naperville-Elgin, Los Angeles-Long Beach-Anaheim, and New York-Newark-Jersey City (left panel) and plots them against the shift-share instrument z_{it}^{x} (right panel). As the right panel shows, there is no correlation between inflation interpolation errors and our instrument. Therefore, linearly interpolating our outcome variable allows us to exploit to the full extent the exogenous variation stemming from our instrumental variable.

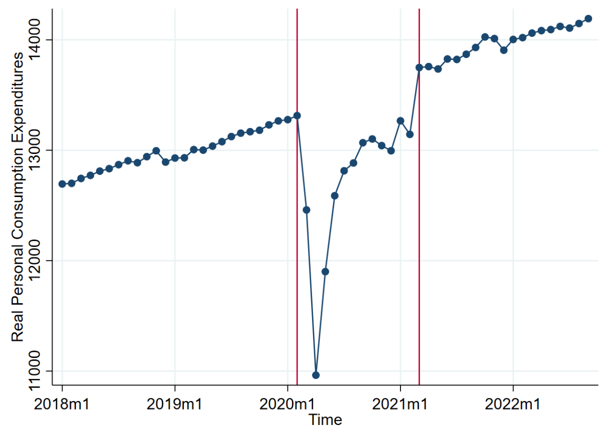
B.3 Empirical Strategy: Model-Based Impulse Response Functions

In this Appendix section, we illustrate through the model the mechanisms at the basis of our identification strategy. Figure B.6 shows the impulse response functions of the main endogenous variables in our model to an intermediate-input sector productivity shock in region H.

As a result of this shock, the production of intermediate input significantly increases in H and decreases to a lesser extent in F. Higher productivity in the intermediate-input sector in H increases labor demand, thus raising equilibrium employment and nominal wages in H. Higher nominal wages cause final-goods sectors' marginal costs to increase, inducing final-goods firms in H to raise prices and boost inflation. The opposite mechanism takes place with less intensity in F, thus generating the identifying cross-sectional variation we capture with our instrument z_{it}^{x}.

B.4 Appendix B: Figures and Tables

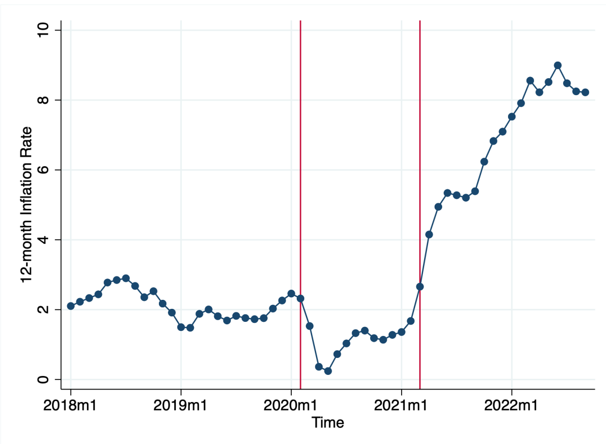
Figure B.1: Real Personal Consumption Expenditures (Jan 2018-Sep 2022)



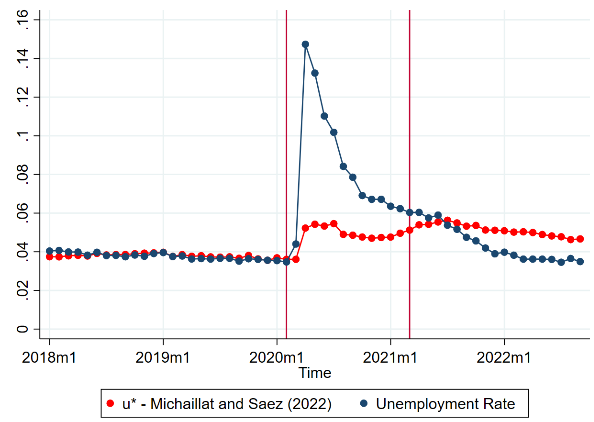
Notes. The figure shows the time series of US real personal consumption expenditures as made available by the BEA from January 2018 to September 2022. We set the start of the post-COVID period in March 2021, when the time series reverts to its pre-pandemic trend after the pandemic shock. The vertical red lines separate the pre-COVID, COVID, and post-COVID periods according to our definition.

Figure B.2: Inflation Rate, Unemployment Rate, and Efficient Unemployment Rate (Jan 2018-Sep 2022)

(a) Inflation Rate



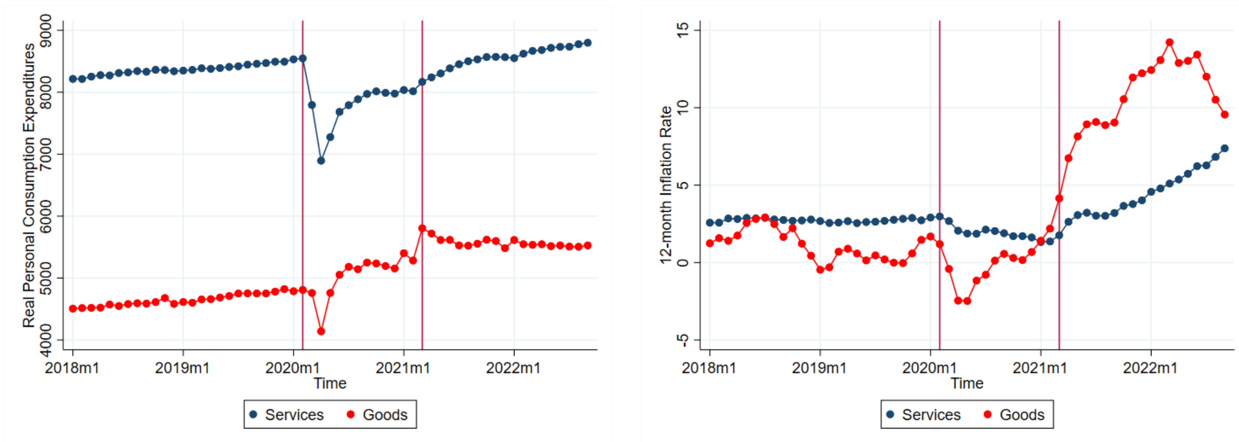
(b) Unemployment Rates



Notes. The figure shows the time series of US 12-month inflation rate (B.2a) from the BLS, the US unemployment rate (B.2b, blue line) from the BLS, and the efficient unemployment rate (B.2b, red line) computed following Michaillat and Saez (2022) from January 2018 to September 2022. The vertical red lines separate the pre-COVID, COVID, and post-COVID periods according to our definition.

Figure B.3: Real Consumption Expenditures and Inflation, Goods vs. Services (Jan 2018-Sep 2022)

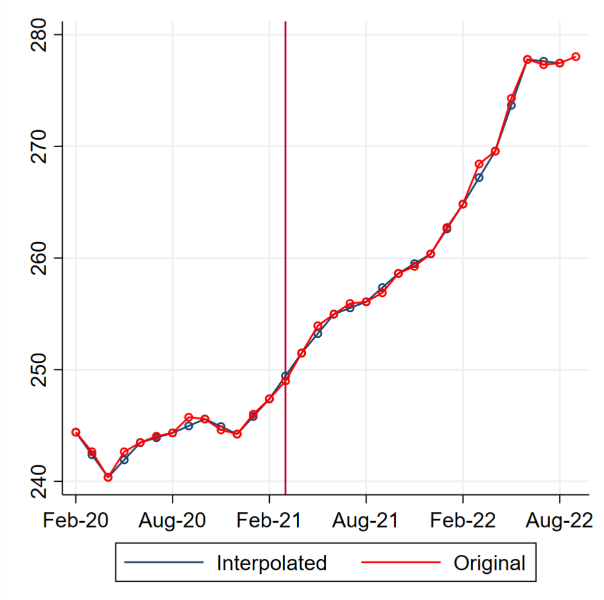
(a) Real Consumption Expenditures (b) Inflation Rates



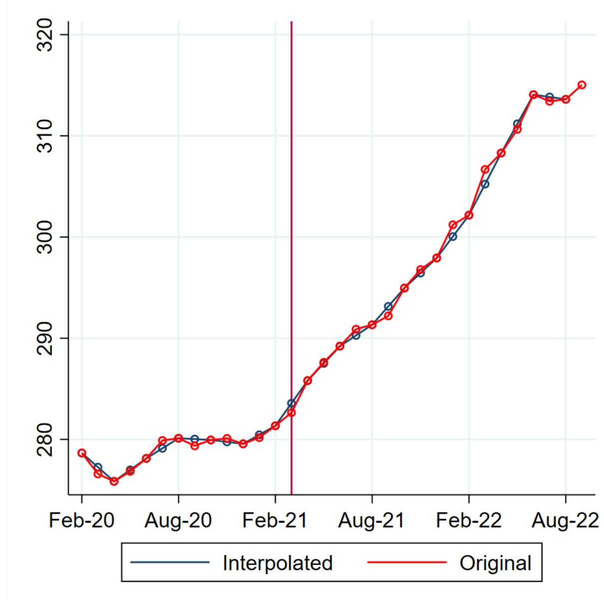
Notes. The figure shows the time series of US real consumption expenditures for goods vs. services (B.3a) from the BEA and the 12-month inflation rate for goods vs. services (B.3b) made available by the BLS from January 2018 to September 2022. We set the start of the post-COVID period in March 2021, when the time series of real personal consumption expenditures reverts to its pre-pandemic trend after the pandemic shock. The vertical red lines separate the pre-COVID, COVID, and post-COVID periods according to our definition.

Figure B.4: Original vs. Interpolated CPI series, Chicago and Los Angeles (Feb 2020-Sep 2022)

(a) Chicago-Naperville-Elgin (IL-IN-WI)



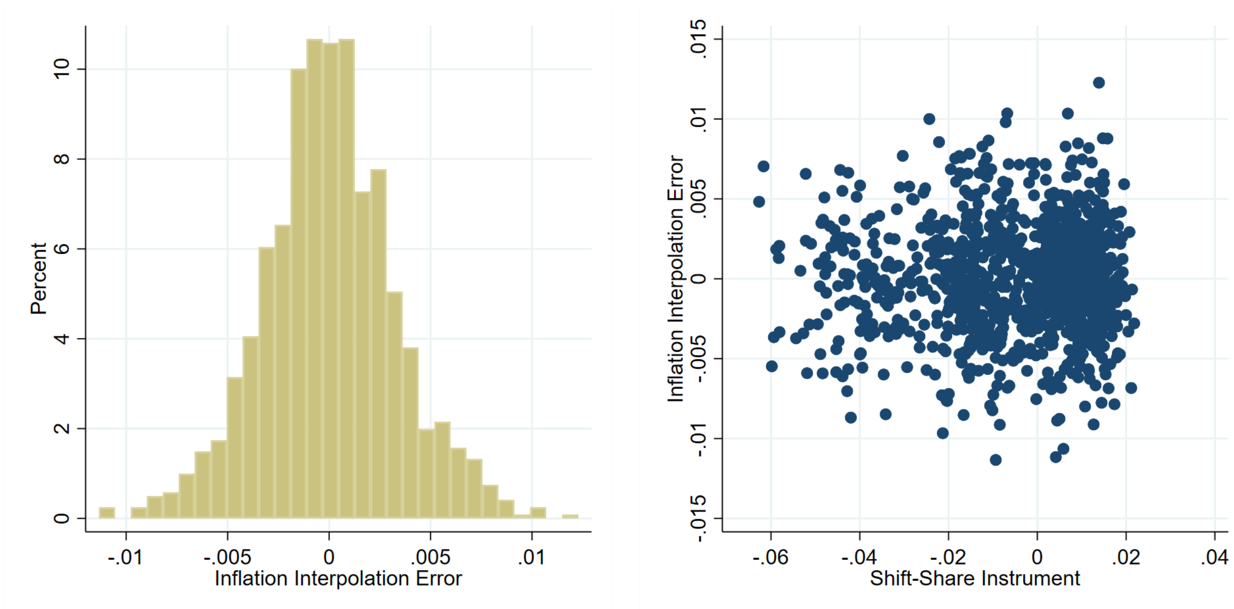
(b) Los Angeles-Long Beach-Anaheim (CA)



Notes. The figure compares original and linearly interpolated time series of the all-items CPI for Chicago-Naperville-Elgin (B.4a) and Los Angeles-Long Beach-Anaheim (B.4b) from February 2020 to September 2022. The blue line denotes the interpolated time series, while the red line denotes the original time series. The interpolated series are produced by imputing odd months observations. The vertical line indicates the beginning of the post-COVID period, starting in March 2021.

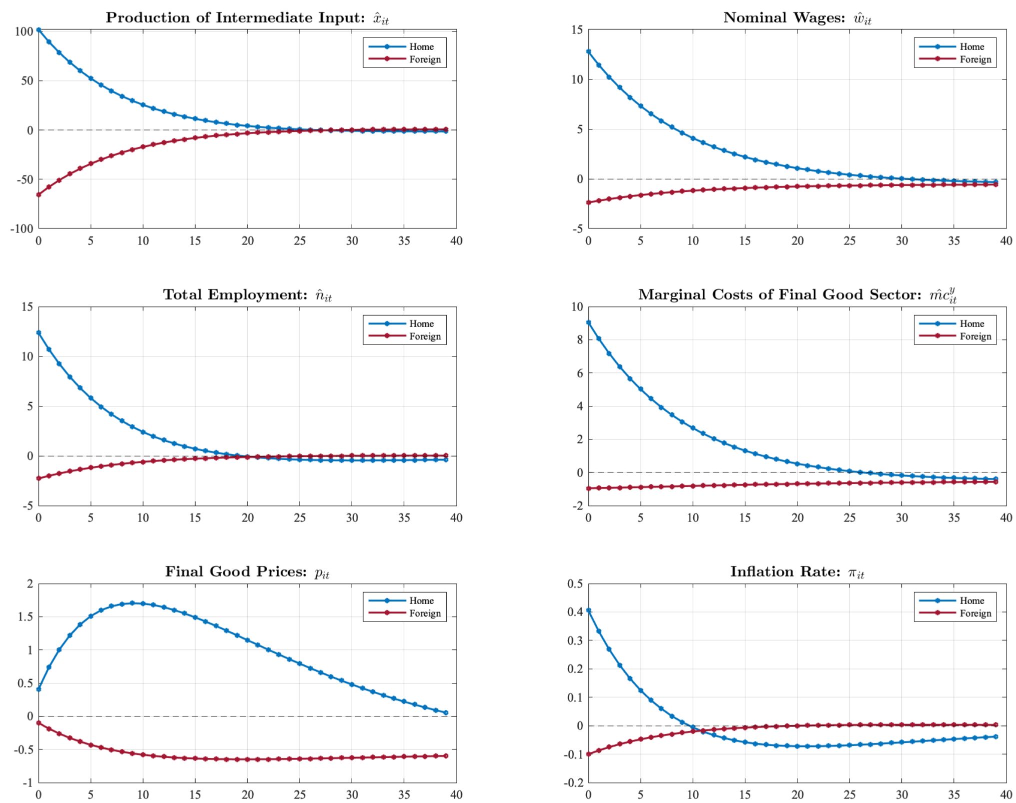
Figure B.5: Inflation Interpolation Error Distribution and Correlation with z^x_{it}

(a) Inflation Interpolation Error Distribution (b) Inflation Intepolation Errors vs. z^x_{it}



Notes. The figure shows the distribution of inflation interpolation errors in for Chicago-Naperville-Elgin, Los Angeles-Long Beach-Anaheim, and New York-Newark-Jersey City (B.5a) and plots them against the shift-share variable z^x_{it} that we use to instrument u_{it} in Equation 2.5 (B.5b). The distribution of inflation interpolation errors pools errors obtained by imputing odd- and even-month observations in the three aforementioned MSAs. The coefficient of a linear regression with inflation interpolation errors as an outcome variable and the shift-share instrument as regressor is 0.002 (0.005).

Figure B.6: Impulse Response Functions to Intermediate Sector Productivity Shock



Notes. The figure shows the impulse response functions over 40 periods of the main endogenous variables in our model to an intermediate-input sector productivity shock in region H. The blue lines refer to IRFs in region H, while the red lines refer to IRFs in region F. From the upper-left panel to the lower-right panel, the figure displays the IRFs of (i) production of intermediate input, (ii) nominal wages, (iii) total employment, (iv) final-goods sector's marginal costs, (v) final-goods prices, (vi) inflation rate.

Table B.1: Descriptive Statistics

	All-Items Inflation			Unemployment Rate			Start of
City	Pre	COVID	Post	Pre	COVID	Post	CPI data
Atlanta	2.00	1.10	8.93	5.50	7.07	3.33	12/1997
	(0.016)	(0.007)	(0.024)	(0.021)	(0.026)	(0.006)	
Baltimore	2.45	0.87	7.14	5.40	6.82	4.75	01/1988
	(0.015)	(0.004)	(0.028)	(0.014)	(0.018)	(0.008)	
Boston	2.58	0.77	5.47	4.97	9.66	4.19	01/1988
	(0.014)	(0.004)	(0.020)	(0.016)	(0.040)	(0.011)	
Chicago	2.23	0.90	6.29	6.42	10.11	5.31	01/1988
	(0.015)	(0.003)	(0.018)	(0.019)	(0.036)	(0.011)	
Dallas	2.32	0.62	7.38	5.18	7.65	4.26	01/1988
	(0.015)	(0.008)	(0.018)	(0.015)	(0.024)	(0.008)	
Denver	3.25	1.47	6.14	4.70	7.77	4.41	11/2017
	(0.004)	(0.010)	(0.025)	(0.019)	(0.025)	(0.012)	
Detroit	2.21	0.64	6.70	7.03	12.09	5.23	02/1988
	(0.014)	(0.007)	(0.019)	(0.028)	(0.066)	(0.013)	
Houston	2.35	0.02	6.86	5.69	9.34	5.44	02/1988
	(0.015)	(0.006)	(0.022)	(0.013)	(0.024)	(0.010)	
Los Angeles	2.58	1.25	6.06	6.87	12.65	6.28	01/1988
	(0.015)	(0.005)	(0.020)	(0.023)	(0.039)	(0.021)	
Miami	2.70	0.85	7.36	5.95	9.12	3.70	01/1988
	(0.015)	(0.006)	(0.028)	(0.023)	(0.032)	(0.012)	
Minneapolis	2.66	1.09	6.69	4.07	6.68	2.67	11/2017
	(0.004)	(0.009)	(0.016)	(0.014)	(0.026)	(0.008)	
New York	2.56	1.52	4.85	6.17	11.28	5.79	01/1988
	(0.013)	(0.003)	(0.014)	(0.017)	(0.037)	(0.015)	
Philadelphia	2.35	0.61	6.32	5.70	9.62	5.25	01/1988
	(0.015)	(0.005)	(0.019)	(0.015)	(0.031)	(0.012)	
Phoenix	3.90	1.39	8.72	5.04	7.49	3.68	12/2017
	(0.005)	(0.007)	(0.033)	(0.018)	(0.023)	(0.010)	
Riverside	2.88	1.85	7.73	7.70	10.82	5.76	12/2017
	(0.002)	(0.005)	(0.018)	(0.029)	(0.030)	(0.018)	
San Diego	2.35	1.54	6.86	5.78	10.36	4.84	11/2017
	(0.002)	(0.006)	(0.013)	(0.022)	(0.035)	(0.017)	
San Francisco	2.87	1.55	4.55	5.27	9.12	4.12	01/1988
	(0.012)	(0.003)	(0.012)	(0.020)	(0.030)	(0.015)	
Seattle	2.41	1.55	7.08	5.10	8.88	3.95	12/1997
	(0.012)	(0.004)	(0.022)	(0.015)	(0.037)	(0.009)	
St. Louis	2.16	0.74	7.20	5.57	7.17	3.87	01/1988
	(0.008)	(0.006)	(0.013)	(0.017)	(0.026)	(0.009)	
Tampa	2.91	2.55	8.49	5.33	8.22	3.38	11/2017
	(0.007)	(0.010)	(0.023)	(0.022)	(0.031)	(0.009)	
Washington	2.46	0.97	5.65	4.15	6.78	4.17	01/1988
	(0.013)	(0.006)	(0.016)	(0.011)	(0.019)	(0.009)	

Notes. This table presents pre-COVID, COVID and post-COVID averages with standard error in parentheses of 12-month, all-items inflation rate and unemployment rate for the 21 Metropolitan Statistical Areas in our sample. In the last column, we report the starting collection date of CPI data.

Table B.2: IV Estimates of ψ from Equation (2.5) with different inflation measures

	(1) All Items	(2) No Energy	(3) Core	(4) No Shelter
Pre-COVID				
u_{it}	-0.25	-0.27	-0.25	0.06
	(0.15)	(0.17)	(0.19)	(0.11)
$\hat{p}^x_{it}$	0.06**	0.05*	0.05**	0.13**
	(0.03)	(0.02)	(0.02)	(0.05)
z^y_{it}	0.13	0.14	0.13	0.04
	(0.09)	(0.11)	(0.15)	(0.05)
COVID				
u_{it}	0.02	0.02	0.03	-0.05
	(0.07)	(0.06)	(0.06)	(0.08)
$\hat{p}^x_{it}$	0.33***	0.09	0.13*	0.32**
	(0.08)	(0.06)	(0.07)	(0.14)
z^y_{it}	0.01	0.03	0.01	0.04
	(0.04)	(0.03)	(0.03)	(0.06)
Post-COVID				
u_{it}	-0.85**	-0.81***	-0.71**	-1.11**
	(0.34)	(0.25)	(0.25)	(0.40)
$\hat{p}^x_{it}$	0.20***	0.05	0.02	0.33***
	(0.04)	(0.03)	(0.03)	(0.04)
z^y_{it}	-0.14	-0.29**	-0.23	-0.47**
	(0.16)	(0.12)	(0.15)	(0.19)
Observations	5862	5862	5862	5862
MSA-Period FE	✓	✓	✓	✓
Year-Quarter FE	✓	✓	✓	✓

Notes. This table presents estimates of ψ from Equation (2.5) for the pre-COVID (i.e., from January 1990 to February 2020), COVID (i.e., from March 2020 to February 2021), and post-COVID (i.e., from March 2021) periods. Column (1) uses 12-month, all-items inflation rate as dependent variable - our benchmark outcome variable. Column (2) uses 12-month all-items excluding energy inflation rate as dependent variable. Column (3) uses 12-month, core inflation rate (i.e., all items excluding food and energy) as dependent variable. Column (4) uses 12-month, all-items excluding shelter inflation rate as dependent variable. All specifications control for MSA fixed effects (allowed to shift across the pre-COVID, COVID, and post-COVID periods), year-quarter fixed effects, intermediate-input prices relative to the corresponding CPI category, and the shift-share control variable z^y_{it}. All columns display IV estimates of ψ obtained by instrumenting u_{it} with the shift-share instrument z^x_{it} and $\hat{p}^x_{it}$ with $\hat{p}^{x*}_{it}$. Standard errors in parentheses are clustered at the MSA level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.3: IV Estimates of ψ from Equation (2.5) with v_{it} (V/U) measuring labor market tightness

	(1) All Items	(2) No Energy	(3) Core	(4) No Shelter
COVID				
v_{it}	-0.014	-0.017	-0.021	0.044
	(0.057)	(0.037)	(0.040)	(0.064)
$\hat{p}_{it}^{x}$	0.355	0.093	0.135	0.103
	(0.237)	(0.097)	(0.107)	(0.352)
z_{it}^{y}	-0.034	0.017	-0.012	0.009
	(0.063)	(0.040)	(0.051)	(0.068)
Post-COVID				
v_{it}	0.041***	0.041***	0.038***	0.052***
	(0.013)	(0.011)	(0.013)	(0.014)
$\hat{p}_{it}^{x}$	0.004	-0.113	-0.129	-0.004
	(0.104)	(0.075)	(0.082)	(0.128)
z_{it}^{y}	-0.147	-0.286**	-0.252**	-0.410**
	(0.123)	(0.103)	(0.114)	(0.158)
Observations	630	630	630	630
MSA-Period FE	✓	✓	✓	✓
Year-Quarter FE	✓	✓	✓	✓

Notes. This table presents estimates of ψ from Equation (2.5) using v_{it} (i.e., the vacancy-to-unemployment ratio) as a measure of labor market tightness for the COVID (i.e., from March 2020 to February 2021) and post-COVID (i.e., from March 2021) periods. Columns (1) to (4) use the 12-month, all-items inflation rate, all-items excluding energy inflation rate, core inflation rate (i.e., all items excluding food and energy), and all-items excluding shelter inflation rate as dependent variables, respectively. All specifications control for MSA fixed effects (allowed to shift across the COVID and post-COVID periods), year-quarter fixed effects, relative intermediate-input prices, and the shift-share control variable z_{it}^{y}. All columns display IV estimates of ψ obtained by instrumenting v_{it} with the shift-share instrument z_{it}^{x} and $\hat{p}_{it}^{x}$ with $\hat{p}_{it}^{x*}$. Standard errors in parentheses are clustered at the MSA level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.4: IV Estimates of ψ from Equation (2.5) for different pre-COVID periods

	(1) From 1990	(2) From 2000	(3) From 2010	(4) COVID	(5) Post-COVID
u_{it}	-0.25	-0.10	-0.18	0.02	-0.85**
	(0.15)	(0.26)	(0.14)	(0.07)	(0.34)
$\hat{p}^x_{it}$	0.06**	0.12***	0.01	0.33***	0.20***
	(0.03)	(0.03)	(0.04)	(0.08)	(0.04)
z^y_{it}	0.13	0.24*	0.22*	0.01	-0.14
	(0.09)	(0.12)	(0.10)	(0.04)	(0.16)
Observations	5211	3652	1852	252	399
MSA-Period FE	✓	✓	✓	✓	✓
Year-Quarter FE	✓	✓	✓	✓	✓

Notes. This table presents estimates of ψ from Equation (2.5) for the pre-COVID, COVID (i.e., from March 2020 to February 2021), and post-COVID (i.e., from March 2021) periods and assesses the sensitivity of the estimates to differences in the definition of the pre-COVID period. Column (1) defines the pre-COVID sample period from January 1990 to February 2020. Column (2) defines the pre-COVID sample period from January 2000 to February 2020. Column (3) defines the pre-COVID sample period from January 2010 to February 2020. Columns (4) and (5) report our preferred estimates from Table 2.2, column (5), for COVID and post-COVID periods. All specifications use 12-month, all-items inflation as the outcome variable and control for MSA fixed effects (allowed to shift across the pre-COVID, COVID, and post-COVID periods), year-quarter fixed effects, relative intermediate-input prices, and the shift-share control variable z^y_{it}. All columns display IV estimates of ψ obtained by instrumenting u_{it} with the shift-share instrument z^x_{it} and $\hat{p}^x_{it}$ with $\hat{p}^{x*}_{it}$. Standard errors in parentheses are clustered at the MSA level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.5: IV Estimates of ψ from Equation (2.5) with proxy of local inflation expectations

	(1) All Items	(2) No Energy	(3) Core	(4) No Shelter
Pre-COVID				
u_{it}	-0.20	-0.23	-0.21	0.07
	(0.14)	(0.15)	(0.18)	(0.09)
$\hat{p}^x_{it}$	0.04*	0.05*	0.05**	0.06*
	(0.02)	(0.02)	(0.02)	(0.03)
π^{gas}_{it}	0.04***	-0.00	-0.00	0.06***
	(0.00)	(0.00)	(0.00)	(0.00)
z^y_{it}	0.13	0.14	0.13	0.05
	(0.09)	(0.12)	(0.16)	(0.05)
COVID				
u_{it}	0.05	0.04	0.05	-0.00
	(0.06)	(0.06)	(0.06)	(0.07)
$\hat{p}^x_{it}$	0.03	-0.02	0.00	0.04
	(0.07)	(0.06)	(0.07)	(0.11)
π^{gas}_{it}	0.05***	0.02**	0.03**	0.06***
	(0.01)	(0.01)	(0.01)	(0.01)
z^y_{it}	0.00	0.02	0.00	0.03
	(0.04)	(0.03)	(0.03)	(0.06)
Post-COVID				
u_{it}	-0.84**	-0.76**	-0.66**	-1.11**
	(0.35)	(0.27)	(0.28)	(0.39)
$\hat{p}^x_{it}$	0.21**	0.10	0.06	0.30***
	(0.09)	(0.07)	(0.07)	(0.06)
π^{gas}_{it}	-0.00	-0.02	-0.02	0.01
	(0.02)	(0.02)	(0.02)	(0.01)
z^y_{it}	-0.12	-0.16	-0.13	-0.51**
	(0.20)	(0.17)	(0.19)	(0.21)
Observations	5646	5646	5646	5646
MSA-Period FE	✓	✓	✓	✓
Year-Quarter FE	✓	✓	✓	✓

Notes. This table presents estimates of ψ from Equation (2.5) for the pre-COVID (i.e., from January 1990 to February 2020), COVID (i.e., from March 2020 to February 2021), and post-COVID (i.e., from March 2021) periods, controlling for a proxy of local inflation expectations. Column (1) uses 12-month, all items inflation rate as dependent variable - our benchmark outcome variable. Column (2) uses 12-month all-items excluding energy inflation rate as dependent variable. Column (3) uses 12-month, core inflation rate (i.e., all-items excluding food and energy) as dependent variable. Column (4) uses 12-month, all-items excluding shelter inflation rate as dependent variable. All specifications control for MSA fixed effects (allowed to shift across the pre-COVID, COVID, and post-COVID periods), year-quarter fixed effects, intermediate-input prices relative to the corresponding CPI category, gasoline inflation in MSA i in period t that proxies for local inflation expectations, and the shift-share control variable z^y_{it}. All columns display IV estimates of ψ obtained by instrumenting u_{it} with the shift-share instrument z^x_{it} and $\hat{p}^x_{it}$ with $\hat{p}^{x*}_{it}$. Standard errors in parentheses are clustered at the MSA level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.6: IV Estimates of ψ from Equation (2.5) with more stringent MSA-time FE

	(1)	(2)	(3)	(4)
Pre-COVID				
u_{it}	-0.25	-0.02	-0.12	-0.16
	(0.15)	(0.20)	(0.13)	(0.10)
$\hat{p}^x_{it}$	0.06**	0.31***	0.29***	0.33***
	(0.03)	(0.03)	(0.03)	(0.03)
z^y_{it}	0.13	0.05	0.02	0.02
	(0.09)	(0.03)	(0.02)	(0.02)
COVID				
u_{it}	0.02	-0.00	-0.04	-0.04
	(0.07)	(0.06)	(0.05)	(0.06)
$\hat{p}^x_{it}$	0.33***	0.30***	0.22***	0.15***
	(0.08)	(0.06)	(0.05)	(0.04)
z^y_{it}	0.01	0.00	-0.00	0.01
	(0.04)	(0.04)	(0.03)	(0.03)
Post-COVID				
u_{it}	-0.85**	-1.01***	-1.33***	-1.33***
	(0.34)	(0.29)	(0.29)	(0.25)
$\hat{p}^x_{it}$	0.20***	0.19***	0.17***	0.16***
	(0.04)	(0.04)	(0.03)	(0.03)
z^y_{it}	-0.14	-0.36***	-0.40***	-0.41***
	(0.16)	(0.08)	(0.10)	(0.11)
Observations	5862	5858	5858	5816
MSA-Period FE	✓			
MSA-Year FE		✓		
MSA-Year-Semester FE			✓	
MSA-Year-Quarter FE				✓
Year-Quarter FE	✓	✓	✓	

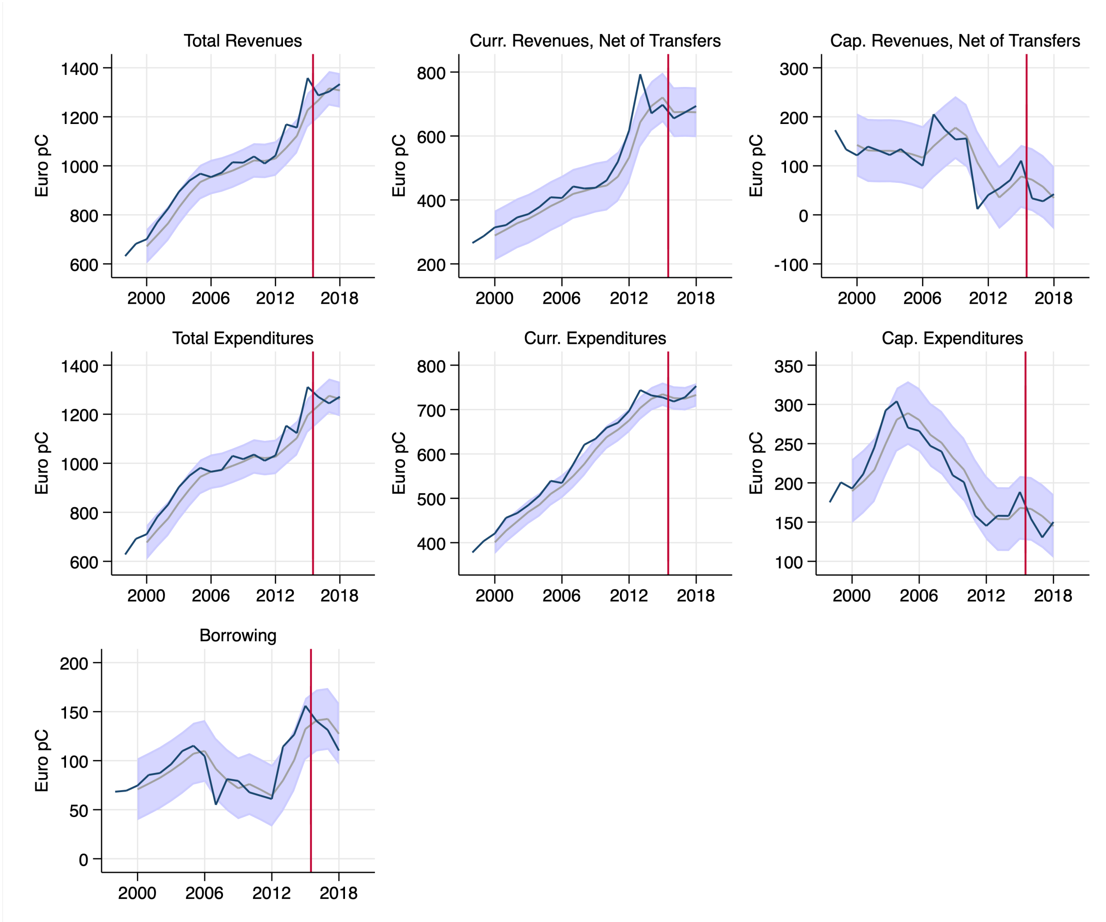
Notes. This table presents estimates of ψ from Equation (2.5) for the pre-COVID (i.e., from January 1990 to February 2020), COVID (i.e., from March 2020 to February 2021), and post-COVID (i.e., from March 2021) periods to assess the sensitivity of the estimates to the inclusion of more stringent MSA-time fixed effects. Column (1) reports our preferred estimates from Table 2.2, column (4), with MSA fixed effects (allowed to shift across the pre-COVID, COVID, and post-COVID periods). Column (2) controls for MSA-year fixed effects. Column (3) controls for MSA-year-semester fixed effects. Column (4) controls for MSA-year-quarter fixed effects. All specifications use 12-month, all-items inflation as the outcome variable and control for relative intermediate-input prices and the shift-share control variable z^y_{it}. All columns display IV estimates of ψ obtained by instrumenting u_{it} with the shift-share instrument z^x_{it} and $\hat{p}^x_{it}$ with $\hat{p}^{x*}_{it}$. Standard errors in parentheses are clustered at the MSA level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix C

Appendix of Balanced Budget Requirements and Local Austerity Multipliers

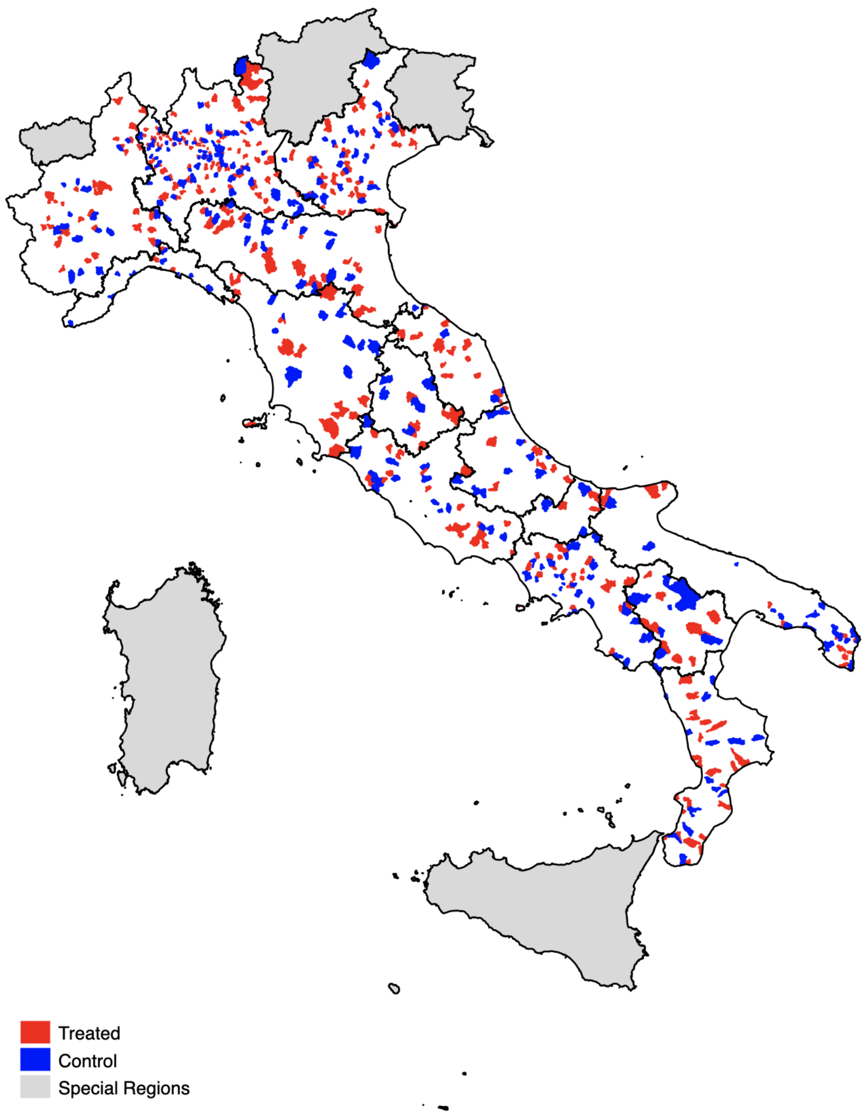
C.1 Appendix C: Figures and Tables

Figure C.1: Stability in Match Between Balance-Sheet Variables in the Pre-2015 Model (CCOU) and Post-2015 Model (CCOX)



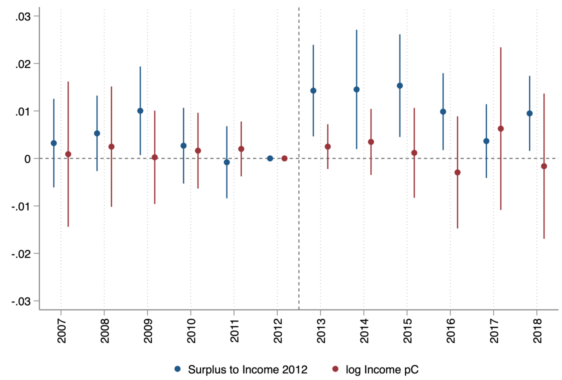
Notes. The figure reports time series (blue lines) and MA3 trends (gray lines) with 95% confidence intervals of the per-capita mean of our main balance-sheet variables of interest. Values are in 2012 Euros. The vertical red line between 2015 and 2016 highlights the year in which the format of municipal balance sheets changed. No clear discontinuity is visible between 2015 and 2016 in any of the variables considered.

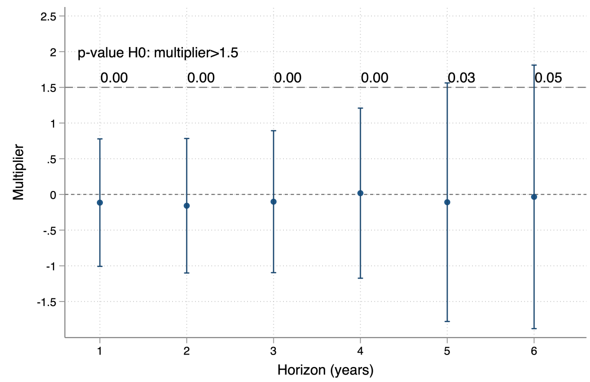
Figure C.2: Treated and Control Municipalities



Notes. The figure shows treated and control municipalities used in the main analysis. Treated municipalities are the ones with a population between 4,000 and 5,000 in 2011. Control municipalities are the ones with a population between 5,001 and 6,000 in 2011. Municipalities in the 5 autonomous regions with a special statute, as well as municipalities that merged at some point in the period of the analysis, are excluded.

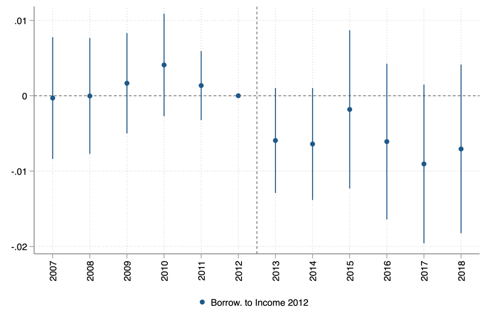
Figure C.3: Dynamic Effects of DSP on Net Budget Surplus and Local Income (Including Sardinia and Sicily)



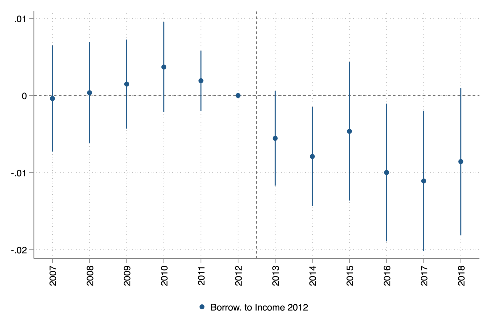


Notes. The left-hand panel of the figure displays difference-in-discontinuities estimates of the effect of the extension of the Domestic Stability Pact (DSP) to Italian Municipalities below 5,000 residents from 2013 on their net budget surplus and the log of municipal per-capita income. We expand the sample of our benchmark specification to include municipalities located in the autonomous regions of Sardinia and Sicily. The net budget surplus is scaled by 2012 total income of municipal residents. We report the estimated coefficients $\hat{\gamma}_t$ from specification (3.1) in its fully dynamic form. Standard errors are clustered at the municipality level. The multiplier estimate and its standard errors are displayed on the right-hand panel of the figure. They are the coefficients of an IV regression with log income per-capita as the dependent variable and net budget surplus as the main independent variable, instrumented by the DSP dummy, keeping observations only up to a specific horizon after the shock. The p-values displayed in the right-hand panel of the figure are obtained from one-sided tests for the multiplier being below 1.5.

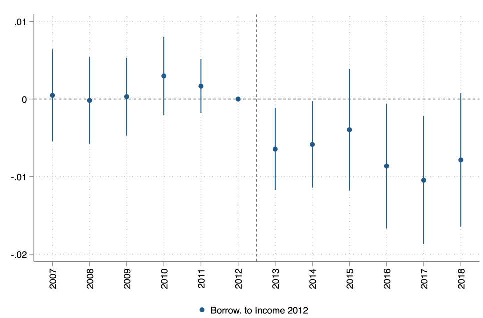
Figure C.4: Dynamic Effects of DSP on Municipal Borrowing



(a) Bandwidth: 750



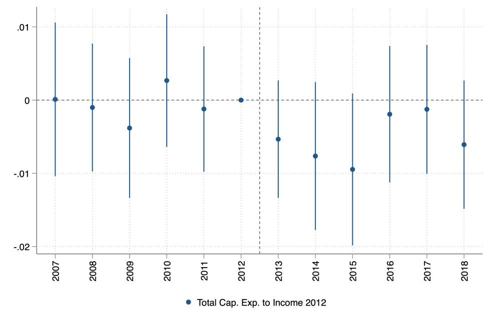
(b) Bandwidth: 1,000



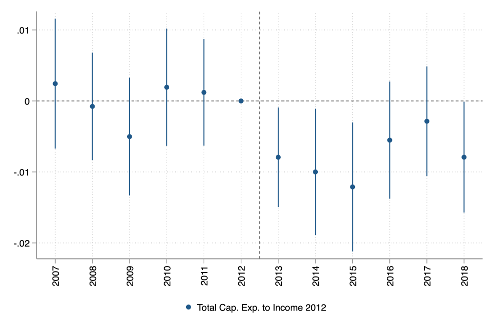
(c) Bandwidth: 1,250

Notes. The figure reports difference-in-discontinuities estimates of the effect of the extension of the Domestic Stability Pact (DSP) to Italian Municipalities below 5,000 residents from 2013 on municipal borrowings. Panels (a), (b), and (c) display the coefficients $\hat{\gamma}_t$ from the fully dynamic version of specification (3.1) with municipal borrowing scaled by 2012 total income of municipal residents as the outcome variable. Panel (a), (b), and (c) differ in the bandwidth around the 5,000 resident population threshold (i.e., 750, 1,000, and 1,250, respectively). Standard errors are clustered at the municipality level.

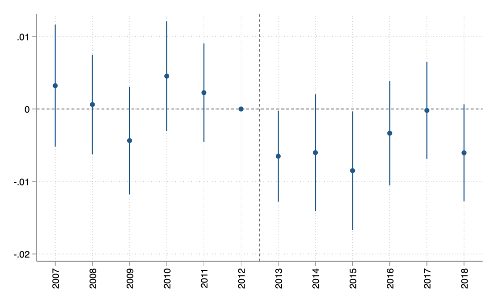
Figure C.5: Dynamic Effects of DSP on Capital Expenditures



(a) Bandwidth: 750



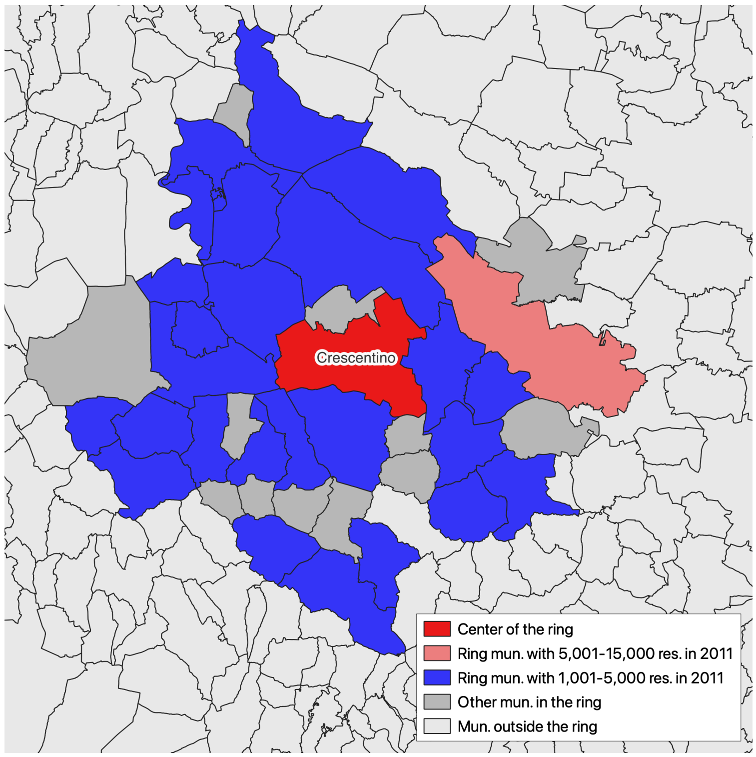
(b) Bandwidth: 1,000



(c) Bandwidth: 1,250

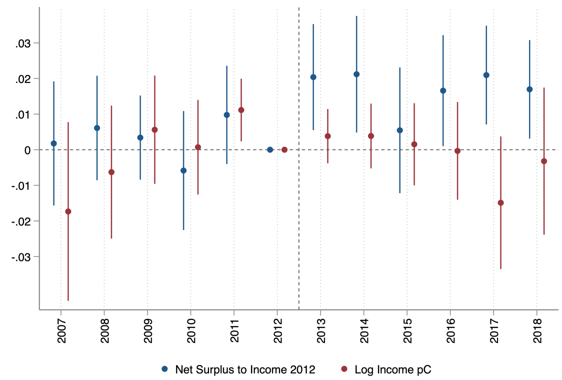
Notes. The figure reports difference-in-discontinuities estimates of the effect of the extension of the Domestic Stability Pact (DSP) to Italian Municipalities below 5,000 residents from 2013 on capital expenditures. Panels (a), (b), and (c) display the coefficients $\hat{\gamma}_t$ from the fully dynamic version of specification (3.1) with capital expenditures scaled by 2012 total income of municipal residents as the outcome variable. Panel (a), (b), and (c) differ in the bandwidth around the 5,000 resident population threshold (i.e., 750, 1,000, and 1,250, respectively). Standard errors are clustered at the municipality level.

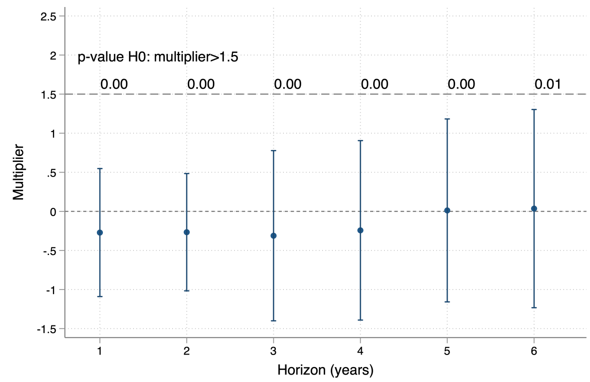
Figure C.6: Ring around the municipality of Crescentino



Notes. The figure shows an example of a neighborhood used to estimate spillover effects in our analysis. The units of observations for our benchmark analysis are municipalities with 2011 population between 5,001 and 15,000 residents, not subject to the 2013 DSP extension. Around these units of observations we define neighborhoods of 20-minute drive. The dark red municipality at the center of the neighborhood is one of our unit of observations (i.e., Crescentino). The blue municipalities around are the ones subject to the 2013 DSP extension within the neighborhood we defined. The light red municipality is another unit of observation of our analysis, whose neighborhood is not shown in this figure. Dark gray municipalities lie within the neighborhood defined, but are not subject to the 2013 DSP extension nor one of our units of observation. Light gray municipalities are outside the neighborhood defined. The logic of our analysis is the following. The share of income of the defined neighborhood accruing to the blue municipalities interacted with a dummy that takes value 1 for all years after 2012 is our instrument for the neighborhood-level fiscal consolidation shock that affects income in Crescentino. In our robustness checks, we vary the size of the neighborhood (i.e., 15, 25, 30-minute drive, as well as the 2011 population ranges to define our units of observations (i.e., 5,000-10,000 and 5,000-20,000).

Figure C.7: Dynamic Effects of DSP on Neighborhood Surplus and Spillovers on Local Income





Notes. The figure reports the impact of neighborhood-level exposure to the 2013 extensions of the Domestic Stability Pact (DSP) on neighborhood-level net budget surplus and municipal log per-capita income. The blue dots display the coefficients $\hat{\gamma}_t$ from specification (3.2) with neighborhood-level net budget surplus scaled by neighborhood level income in 2012 as the dependent variable (i.e., first-stage regression). The red dots display the coefficients $\hat{\gamma}_t$ from specification (3.2) with municipal log per-capita income as the dependent variable (i.e., reduced-form regression). The specifications include controls for region-specific time trends. Standard errors are clustered at the municipality level. The multiplier estimate and its standard errors are displayed on the right-hand panel of the figure. They are the coefficients of an IV regression with log income per-capita as the dependent variable and neighborhood-level net budget surplus as the main independent variable, instrumented by the share of 2012 income subject to the DSP from 2013 interacted with a dummy taking value 1 for all years after 2012, keeping observations only up to a specific horizon after the shock. The p-values displayed in the right-hand panel of the figure are obtained from one-sided tests for the multiplier being below 1.5.

Table C.1: Evolution of DSP Rules for Italian Municipalities

Year	Target Municipalities	Deficit Rule	Accounting Criteria	Others
1999	All	Zero growth	Cash	Initial sanctions: cut in transfers, ban on hires, cut on non-absenteeism bonuses
2000	All	Zero growth	Cash	
2001	>5,000 resid.	Max 3% growth	Cash	
2002	>5,000 resid.	Max 2.5% growth	Cash	Limit to current expenditure
2003	>5,000 resid.	Zero growth	Cash+Accrual	
2004	>5,000 resid.	Zero growth	Cash+Accrual	
2005	>5,000 resid.		Cash+Accrual	Current+capital expenditure cannot grow more than personalized threshold (up to 10%)
2006	>5,000 resid.		Cash+Accrual	Current must be reduced, capital can grow within personalized threshold
2007	>5,000 resid.	Zero growth	Cash+Accrual	
2008	>5,000 resid.	Zero growth	"Mixed"	
2009	>5,000 resid.	Pers. red. goal (*)	"Mixed"	Additional sanctions: limits to borrowing, limits to current expenditure, larger cut to transfers and administrators' wages
2010	>5,000 resid.	Pers. red. goal (*)	"Mixed"	
2011	>5,000 resid.	Zero-deficit	"Mixed"	
2012	>5,000 resid.	Zero-deficit	"Mixed"	Cut to transfers to municipalities >5,000 residents
2013	>1,000 resid.	Zero-deficit	"Mixed"	
2014	>1,000 resid.	Zero-deficit	"Mixed"	
2015	>1,000 resid.	Zero-deficit	"Mixed"	
2016	All	Zero-deficit	Accrual	
2017	All	Zero-deficit	Accrual	
2018	All	Zero-deficit	Accrual	
2019	All	Zero-deficit	Accrual	

Notes. (*) Specifically, according to art.77 of L. 203/2008, municipalities are required to improve the 2007 balance, calculated on a "mixed" basis, a) If the municipality fulfilled the DSP and reported a deficit in 2007, 48% in 2009, 97% in 2010 and 165% for 2011; b) If the municipality fulfilled the DSP and reported a surplus in 2007, 10% in 2009, 10% in 2010 and 0% for 2011; c) If the municipality did not fulfill the DSP and reported a deficit in 2007, 70% in 2009, 110% in 2010 and 180% for 2011; and d) If the municipality did not fulfill the DSP and reported a surplus in 2007, 0% in 2009, 0% in 2010 and 0% for 2011. Requirements for 2011 were then modified by art.1 of L.220/2010.

Table C.2: Descriptive Statistics (Full Sample)

	Count	Mean	St. Dev.	P10	P90	Total
Period: 2007-2012						
Total budget	33,610	11,630	96,198	1,148	17,127	390,899,548
Fiscal revenues	33,610	3,697	30,679	245	5,995	124,240,142
Non-fiscal revenues	33,610	1,735	21,792	111	2,606	58,301,638
Revenues from capital transfers	33,610	1,932	24,300	87	3,010	64,942,761
Curr. expenditures	33,610	7,241	60,659	688	10,887	243,374,757
Capital expenditures	33,610	2,385	28,238	138	3,692	80,154,161
Total income declared	33,610	108,979,715	751,632,018	8,388,570	187,807,880	3,662,808,221,310
Labor income declared	33,610	57,323,343	390,931,134	4,173,840	101,863,825	1,926,637,549,403
Self-entrepreneurship income decl.	33,610	5,267,338	51,751,893	188,625	8,077,614	177,035,221,810
Capital income decl.	33,610	9,543,522	68,938,787	442,013	17,033,919	320,757,765,551
Freq. income 0-15,000	33,610	2,791	12,482	320	5,198	93,806,411
Freq. income 15,000-26,000	33,610	1,815	8,689	162	3,457	61,007,551
Freq. income > 26,000	33,610	1,259	9,823	69	2,092	42,320,490
Period: 2013-2015						
Total budget	20,807	13,181	112,800	1,087	19,104	274,248,891
Fiscal revenues	20,807	5,914	44,239	445	9,740	123,046,764
Non-fiscal revenues	20,807	1,848	23,517	108	2,702	38,459,827
Revenues from capital transfers	20,807	1,628	14,757	50	2,860	33,865,694
Curr. expenditures	20,807	7,870	73,755	673	12,075	163,752,076
Capital expenditures	20,807	1,893	17,268	65	3,238	39,381,256
Total income declared	20,807	116,662,353	814,699,602	8,026,806	204,846,224	2,427,393,580,244
Labor income declared	20,807	60,414,856	415,776,245	3,844,104	110,905,303	1,257,051,910,210
Self-entrepreneurship income decl.	20,807	9,327,854	71,085,682	551,107	15,580,202	194,084,664,348
Capital income decl.	20,807	8,816,965	63,791,444	389,385	15,982,554	183,454,597,923
Freq. income 0-15,000	20,807	2,512	11,904	253	4,758	52,257,752
Freq. income 15,000-26,000	20,807	1,715	8,126	144	3,324	35,694,259
Freq. income > 26,000	20,807	1,372	10,332	71	2,362	28,546,566
Period: 2016-2018						
Total budget	20,350	14,626	137,578	1,165	20,765	297,644,450
Fiscal revenues	20,350	5,797	45,662	448	9,361	117,971,380
Non-fiscal revenues	20,350	1,986	23,112	111	2,954	40,416,460
Revenues from capital transfers	20,350	1,389	11,209	48	2,379	28,256,850
Curr. expenditures	20,350	7,987	70,376	697	12,330	162,526,020
Capital expenditures	20,350	1,504	9,001	86	2,716	30,614,588
Total income declared	20,350	126,424,075	870,454,630	9,073,736	222,636,320	2,572,729,934,498
Labor income declared	20,350	66,687,281	449,912,872	4,475,370	122,782,228	1,357,086,162,725
Self-entrepreneurship income decl.	20,350	9,413,462	73,484,128	535,052	15,562,734	191,563,960,339
Capital income decl.	20,350	9,054,309	65,986,535	414,293	16,264,128	184,255,189,129
Freq. income 0-15,000	20,350	2,502	11,976	254	4,680	50,925,405
Freq. income 15,000-26,000	20,350	1,755	7,970	157	3,430	35,707,585
Freq. income > 26,000	20,350	1,521	10,984	86	2,636	30,947,511

Notes. The table reports descriptive statistics of the sample after dropping municipalities that were merged, and restricting to municipalities with no missing information between 2007 and 2018. Monetary values are in 2012 Euros.

Table C.3: Descriptive Statistics (Selected Sample)

	Count	Mean	St. Dev.	P10	P90	Total
Period: 2007-2012						
Total budget	5,074	5,205	2,853	3,071	8,030	26,411,463
Fiscal revenues	5,074	1,860	1,099	924	2,912	9,439,201
Non-fiscal revenues	5,074	756	798	279	1,301	3,837,040
Revenues from capital transfers	5,074	926	1,709	161	1,892	4,698,411
Curr. expenditures	5,074	3,132	1,271	2,047	4,420	15,892,447
Capital expenditures	5,074	1,209	1,839	227	2,489	6,131,931
Total income declared	5,074	55,988,832	17,736,808	33,068,016	80,695,240	284,087,333,640
Labor income declared	5,074	30,073,178	10,230,294	17,415,668	44,310,776	152,591,306,996
Self-entrepreneurship income decl.	5,074	2,159,301	1,843,604	692,005	4,822,989	10,956,292,219
Capital income decl.	5,074	4,816,191	2,794,988	1,547,302	8,160,609	24,437,352,668
Freq. income 0-15,000	5,074	1,674	453	1,133	2,281	8,494,818
Freq. income 15,000-26,000	5,074	1,087	362	575	1,574	5,513,387
Freq. income > 26,000	5,074	580	252	274	927	2,941,435
Period: 2013-2015						
Total budget	3,047	5,816	3,678	3,054	9,639	17,722,571
Fiscal revenues	3,047	2,919	1,846	1,732	4,449	8,893,951
Non-fiscal revenues	3,047	798	692	293	1,412	2,431,133
Revenues from capital transfers	3,047	915	1,810	107	1,988	2,787,579
Curr. expenditures	3,047	3,429	1,601	2,157	4,862	10,448,394
Capital expenditures	3,047	1,021	1,948	115	2,325	3,109,579
Total income declared	3,047	62,221,824	19,948,740	36,335,092	90,227,440	189,589,898,978
Labor income declared	3,047	33,062,563	11,753,872	18,332,301	49,504,198	100,741,630,194
Self-entrepreneurship income decl.	3,047	4,773,777	1,979,808	2,626,048	7,305,872	14,545,697,309
Capital income decl.	3,047	4,640,220	2,665,786	1,627,681	7,649,623	14,138,749,820
Freq. income 0-15,000	3,047	1,524	424	1,030	2,112	4,644,223
Freq. income 15,000-26,000	3,047	1,068	343	594	1,529	3,254,595
Freq. income > 26,000	3,047	682	295	313	1,095	2,077,633
Period: 2016-2018						
Total budget	3,046	5,963	3,474	3,164	9,975	18,164,654
Fiscal revenues	3,046	2,693	1,311	1,727	3,742	8,204,096
Non-fiscal revenues	3,046	814	817	290	1,474	2,477,991
Revenues from capital transfers	3,046	706	1,003	106	1,566	2,151,743
Curr. expenditures	3,046	3,425	1,649	2,143	4,876	10,432,212
Capital expenditures	3,046	847	940	172	1,751	2,578,665
Total income declared	3,046	65,831,931	21,902,948	37,730,180	96,559,328	200,524,062,294
Labor income declared	3,046	35,645,755	12,828,313	19,726,300	53,820,543	108,576,970,776
Self-entrepreneurship income decl.	3,046	4,568,505	2,081,158	2,286,246	7,185,275	13,915,665,973
Capital income decl.	3,046	4,693,262	2,902,008	1,598,401	7,863,386	14,295,675,769
Freq. income 0-15,000	3,046	1,458	402	989	2,013	4,441,749
Freq. income 15,000-26,000	3,046	1,071	336	612	1,527	3,262,925
Freq. income > 26,000	3,046	752	321	351	1,196	2,289,237

Notes. The table reports descriptive statistics of the sample after dropping municipalities from autonomous regions with special statute, municipalities that were merged, and restrict to municipalities with no missing information between 2007 and 2018, having a number of inhabitants between 3,500 and 6,500 in the 2011 census, which comprise all the municipalities in the different bandwidths around the threshold of 5,000 we are going to use. Monetary values are in 2012 Euros.

Table C.4: Effect of DSP on Surplus and Local Income (Different Bandwidth)

	(1) Surplus to 2012 Income	(2) Log Per-Capita Income	(3) Surplus to 2012 Income	(4) Log Per-Capita Income	(5) Surplus to 2012 Income	(6) Log Per-Capita Income
$D_i \times T_t$	0.00622*	-0.00361	0.00699**	-0.00247	0.00608**	-0.00138
	(0.00274)	(0.00798)	(0.00222)	(0.00612)	(0.00203)	(0.00557)
Observations	6021	6021	10076	10076	12185	12185
R-squared	0.516	0.988	0.515	0.988	0.517	0.987
Bandwidth	750	750	1250	1250	1500	1500
Years	2007-2018	2007-2018	2007-2018	2007-2018	2007-2018	2007-2018
Mean in 2012	-0.02513	9.38875	-0.02535	9.39733	-0.02564	9.39345
Specification		Diff-in-disc		Diff-in-disc		Diff-in-disc
Multiplier		.58		.352		.226
		[1.29]		[.880]		[.919]
H0: Multiplier $\geq$ 1.5		.239		.096		.083

Notes. The table reports difference-in-discontinuities estimates of the effect of the 2013 extension of the Domestic Stability Pact (DSP) to Italian Municipalities between 1,000 and 5,000 residents on their net budget surplus and the log of municipal per-capita income. We report the estimated coefficient $\hat{\gamma}$ from specification (3.1). The table presents results from our benchmark specification with several population bandwidth (i.e., 750, 1,250, and 1,500 residents around the threshold of 5,000 residents). Standard errors are clustered at the municipality level. The multiplier estimate and its standard errors are obtained from an IV regression with log income per-capita as the dependent variable and net budget surplus as the main independent variable, instrumented by the DSP dummy. The last row reports the p-values obtained from one-sided tests for the multiplier being below 1.5. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.5: Effect of DSP on Surplus and Local Income (Restricted Sample)

	(1) Surplus to 2012 Income	(2) Log Per-Capita Income	(3) Surplus to 2012 Income	(4) Log Per-Capita Income	(5) Surplus to 2012 Income	(6) Log Per-Capita Income	(7) Surplus to 2012 Income	(8) Log Per-Capita Income
$D_i \times T_t$	0.00839*	-0.00124	0.01323***	-0.00130	0.00982**	-0.00141	0.00886**	0.00001
	(0.00390)	(0.00629)	(0.00367)	(0.00553)	(0.00314)	(0.00479)	(0.00283)	(0.00439)
Observations	4516	4516	6036	6036	7557	7557	9139	9139
R-squared	0.514	0.991	0.502	0.991	0.509	0.991	0.511	0.990
Bandwidth	750	750	1000	1000	1250	1250	1500	1500
Years	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015
Mean in 2012	-0.02513	9.38875	-0.02552	9.39266	-0.02535	9.39733	-0.02564	9.39345
Specification		Diff-in-disc		Diff-in-disc		Diff-in-disc		Diff-in-disc
Multiplier		.148		.098		.143		0
		[.744]		[.416]		[.487]		[.495]
H0: Multiplier ≥ 1.5		.035		0		.003		.001

Notes. The table reports difference-in-discontinuities estimates of the effect of the 2013 extension of the Domestic Stability Pact (DSP) to Italian Municipalities between 1,000 and 5,000 residents on their net budget surplus and the log of municipal per-capita income. We report the estimated coefficient $\hat{\gamma}$ from specification (3.1), limiting the time frame to the 2007-2015 period, before the municipal balance sheet format changes. The table presents results from our benchmark specification with several population bandwidth (i.e., 750, 1,000, 1,250, and 1,500 residents around the threshold of 5,000 residents). Standard errors are clustered at the municipality level. The multiplier estimate and its standard errors are obtained from an IV regression with log income per-capita as the dependent variable and net budget surplus as the main independent variable, instrumented by the DSP dummy. The last row reports the p-values obtained from one-sided tests for the multiplier being below 1.5. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.6: Effect of DSP on Surplus and Local Income (Including Sardinia and Sicily)

	(1) Surplus to 2012 Income	(2) Log Per-Capita Income	(3) Surplus to 2012 Income	(4) Log Per-Capita Income	(5) Surplus to 2012 Income	(6) Log Per-Capita Income	(7) Surplus to 2012 Income	(8) Log Per-Capita Income
$D_i \times T_t$	0.00368	-0.00311	0.00778**	0.00026	0.00508*	0.00045	0.00468*	-0.00019
	(0.00292)	(0.00850)	(0.00270)	(0.00731)	(0.00236)	(0.00644)	(0.00214)	(0.00580)
Observations	6441	6441	8708	8708	10892	10892	13229	13229
R-squared	0.572	0.988	0.572	0.988	0.584	0.988	0.590	0.987
Bandwidth	750	750	1000	1000	1250	1250	1500	1500
Years	2007-2018	2007-2018	2007-2018	2007-2018	2007-2018	2007-2018	2007-2018	2007-2018
Mean in 2012	-0.02890	9.36215	-0.02992	9.36326	-0.03024	9.36659	-0.03083	9.36057
Specification		Diff-in-disc		Diff-in-disc		Diff-in-disc		Diff-in-disc
Multiplier		.843		-.034		-.088		.041
		[2.30]		[.940]		[1.27]		[1.23]
H0: Multiplier $\geq$ 1.5		.388		.052		.106		.12

Notes. The table reports difference-in-discontinuities estimates of the effect of the 2013 extension of the Domestic Stability Pact (DSP) to Italian Municipalities between 1,000 and 5,000 residents on their net budget surplus and the log of municipal per-capita income. We report the estimated coefficient $\hat{\gamma}$ from specification (3.1), expanding the sample of our benchmark specification to include municipalities in the autonomous regions of Sardinia and Sicily. The table presents results from our benchmark specification with several population bandwidth (i.e., 750, 1,000, 1,250, and 1,500 residents around the threshold of 5,000 residents). Standard errors are clustered at the municipality level. The multiplier estimate and its standard errors are obtained from an IV regression with log income per-capita as the dependent variable and net budget surplus as the main independent variable, instrumented by the DSP dummy. The last row reports the p-values obtained from one-sided tests for the multiplier being below 1.5. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.7: Composition of the Municipal Budget Shock Induced by DSP Extension (Restricted Panel)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$D_i \times T_t$	0.00069	0.01110***	-0.00022	-0.00123	0.00014	-0.01099***	-0.00845**
	(0.00147)	(0.00264)	(0.00192)	(0.00125)	(0.00067)	(0.00276)	(0.00271)
Observations	6036	6036	6036	6036	6036	6036	6036
R-squared	0.634	0.382	0.804	0.921	0.259	0.387	0.556
Bandwidth	1000	1000	1000	1000	1000	1000	1000
Years	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015
Mean in 2012	-0.01076	-0.01513	0.04832	0.05928	0.00542	0.01673	0.00727

Notes. The table reports difference-in-discontinuities estimates of the effect of the 2013 extension of the Domestic Stability Pact (DSP) to Italian Municipalities between 1,000 and 5,000 residents on the different components of their net budget surplus. We report the estimated coefficient $\hat{\gamma}$ from specification (3.1) for several different outcome variables, limiting the time frame to the 2007-2015 period, before the municipal balance sheet format changes. Specifically, columns (1) and (2) report the impact on current and capital surplus, respectively. Columns (3) to (6) report the impact on current revenues, current expenditures, capital revenues, and capital expenditures, respectively. Finally, column (7) reports the impact on municipal borrowings. All outcome variables are scaled by 2012 total income of municipal residents. The table presents results from our benchmark specification with a population bandwidth of 1,000 residents around the threshold of 5,000 residents. Standard errors are clustered at the municipality level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.8: Composition of the Change in Expenditures Induced by DSP Extension

	(1) Cur. Exp. to 2012 Income	(2) Mean DV	(3) Cap. Exp. to 2012 Income	(4) Mean DV
Administration	-0.00147	0.01996	-0.00095	0.00319
	(0.00126)		(0.00199)	
Culture	-0.00013	0.00116	-0.00066	0.00073
	(0.00011)		(0.00057)	
Justice	-0.00001	0.00005	-0.00002	0.00002
	(0.00001)		(0.00002)	
School	0.00011	0.00579	-0.00087	0.00252
	(0.00016)		(0.00076)	
Police	-0.00004	0.00274	0.00000	0.00009
	(0.00016)		(0.00003)	
Utilities	-0.00004	0.00050	-0.00039	0.00042
	(0.00010)		(0.00037)	
Social services	-0.00129	0.00682	0.00048	0.00164
	(0.00097)		(0.00065)	
Sport Facilities	0.00014*	0.00094	-0.00123***	0.00101
	(0.00008)		(0.00039)	
Economic Development	0.00008	0.00025	0.00024	0.00043
	(0.00006)		(0.00078)	
Urban Planning	-0.00023	0.01300	-0.00380*	0.00696
	(0.00091)		(0.00220)	
Tourism	0.00001	0.00043	0.00013	0.00049
	(0.00012)		(0.00034)	
Roads and Transp.	-0.00000	0.00529	-0.00095	0.00488
	(0.00025)		(0.00086)	

Notes. The table reports difference-in-discontinuities estimates of the effect of the 2013 extension of the Domestic Stability Pact (DSP) to Italian Municipalities between 1,000 and 5,000 residents on the different components of their net budget surplus. We report the estimated coefficient $\hat{\gamma}$ from specification (3.1) for current and capital expenditures normalized by the 2012 total income of municipal residents. The table presents results from our benchmark specification with a population bandwidth of 1,000 residents around the threshold of 5,000 residents. Standard errors are clustered at the municipality level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.9: Effect of DSP on Neighborhood Surplus and Spillover on Local Income (Upper Panel: Municipalities 5,000-20,000 residents; Lower Panel: 5,000-10,000 residents)

	(1) Surplus to 2012 Income	(2) Log Per-Capita Income	(3) Surplus to 2012 Income	(4) Log Per-Capita Income
% GDP of Neighb.	0.01489**	-0.00438	0.01563**	0.00368
under DSP	(0.00500)	(0.01124)	(0.00496)	(0.01047)
Observations	11532	11532	11532	11532
R-squared	0.813	0.990	0.856	0.991
Time trend	-	-	Region	Region
Mean in 2012	-0.02265	9.41965	-0.02265	9.41965
Multiplier		.294		-.235
		[.769]		[.667]
H0: Multiplier $\geq$ 1.5		.059		.005

	(1) Surplus to 2012 Income	(2) Log Per-Capita Income	(3) Surplus to 2012 Income	(4) Log Per-Capita Income
% GDP of Neighb.	0.01184**	-0.00855	0.01359***	-0.00489
under DSP	(0.00384)	(0.00951)	(0.00381)	(0.00893)
Observations	18336	18336	18336	18336
R-squared	0.809	0.990	0.853	0.992
Time trend	-	-	Region	Region
Mean in 2012	-0.02140	9.42168	-0.02140	9.42168
Multiplier		.721		.36
		[.849]		[.673]
H0: Multiplier $\geq$ 1.5		.18		.045

Notes. The table reports the impact of neighborhood-level exposure to the 2013 extensions of the Domestic Stability Pact (DSP) on neighborhood-level net budget surplus and municipal log per-capita income. Columns (1) and (3) report the coefficient $\hat{\gamma}$ from specification (3.2) with neighborhood-level net budget surplus scaled by neighborhood-level income in 2012 as the dependent variable (i.e., first-stage regression). Columns (2) and (4) report the coefficient $\hat{\gamma}$ from specification (3.2) with municipal log per-capita income as the dependent variable (i.e., reduced-form regression). Columns (3) and (4) include region-specific time fixed effects. Standard errors are clustered at the municipality level. The multiplier estimate and its standard errors are obtained from an IV regression with municipal log income per-capita as the dependent variable and the neighborhood-level net budget surplus as the main independent variable, instrumented by the neighborhood-level exposure to the 2013 DSP extension interacted with a dummy taking value 1 for all years after 2012. The last row reports the p-values obtained from one-sided tests for the multiplier being below 1.5. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.10: Effect of DSP on Neighborhood Surplus and Spillover on Local Income (Municipalities 5,000-15,000 residents)

	(1) Surplus to 2012 Income	(2) Log Per-Capita Income	(3) Surplus to 2012 Income	(4) Log Per-Capita Income	(5) Surplus to 2012 Income	(6) Log Per-Capita Income	(7) Surplus to 2012 Income	(8) Log Per-Capita Income
% GDP of Neighb.	0.00817*	-0.00863	0.01237**	-0.00560	0.01123**	-0.00908	0.00950*	0.01313
under DSP	(0.00355)	(0.00835)	(0.00412)	(0.00995)	(0.00354)	(0.01296)	(0.00446)	(0.02283)
Observations	15936	15936	16176	16176	16224	16224	16236	16236
R-squared	0.762	0.990	0.812	0.990	0.839	0.990	0.860	0.990
Radius	15	15	20	20	25	25	30	30
Time trend	-	-	-	-	-	-	-	-
Mean in 2012	-0.02265	9.42243	-0.02172	9.42025	-0.02128	9.41997	-0.02137	9.42013
Multiplier		1.056		.453		.808		-1.381
		[1.11]		[.827]		[1.16]		[2.73]
H0: Multiplier $\geq$ 1.5		.346		.103		.277		.146

	(1) Surplus to 2012 Income	(2) Log Per-Capita Income	(3) Surplus to 2012 Income	(4) Log Per-Capita Income	(5) Surplus to 2012 Income	(6) Log Per-Capita Income	(7) Surplus to 2012 Income	(8) Log Per-Capita Income
% GDP of Neighb.	0.00912**	-0.00222	0.01439***	-0.00051	0.01374***	-0.01528	0.01222**	0.01257
under DSP	(0.00339)	(0.00773)	(0.00408)	(0.00930)	(0.00321)	(0.01255)	(0.00421)	(0.02253)
Observations	15936	15936	16176	16176	16224	16224	16236	16236
R-squared	0.805	0.992	0.857	0.992	0.887	0.992	0.908	0.992
Radius	15	15	20	20	25	25	30	30
Time trend	Region	Region	Region	Region	Region	Region	Region	Region
Mean in 2012	-0.02265	9.42243	-0.02172	9.42025	-0.02128	9.41997	-0.02137	9.42013
Multiplier		.243		.035		1.111		-1.028
		[.853]		[.646]		[.978]		[2.00]
H0: Multiplier $\geq$ 1.5		.071		.012		.346		.104

Notes. The table reports the impact of neighborhood-level exposure to the 2013 extensions of the Domestic Stability Pact (DSP) on neighborhood-level net budget surplus and municipal log per-capita income. We vary the radius defining a neighborhood across pairs of columns. Specifically, columns (1) and (2) report results with a 15-minute radius, columns (3) and (4) with a 20-minute radius, columns (5) and (6) with a 25-minute radius, and columns (7) and (8) with a 30-minute radius. Columns (1), (3), (5), and (7) report the coefficient $\hat{\gamma}$ from specification (3.2) with neighborhood-level net budget surplus scaled by neighborhood-level income in 2012 as the dependent variable (i.e., first-stage regression). Columns (2), (4), (6), and (8) report the coefficient $\hat{\gamma}$ from specification (3.2) with municipal log per-capita income as the dependent variable (i.e., reduced-form regression). In the lower panel, all columns include region-specific time fixed effects. Standard errors are clustered at the municipality level. The multiplier estimate and its standard errors are obtained from an IV regression with municipal log income per-capita as the dependent variable and the neighborhood-level net budget surplus as the main independent variable, instrumented by the neighborhood-level exposure to the 2013 DSP extension interacted with a dummy taking value 1 for all years after 2012. The last row reports the p-values obtained from one-sided tests for the multiplier being below 1.5. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.11: Effect of the DSP Extension on Neighborhood Surplus and Spillovers on Local Income (Including Sardinia and Sicily)

	(1) Surplus to 2012 Income	(2) Log Per-Capita Income	(3) Surplus to 2012 Income	(4) Log Per-Capita Income
% GDP of Neighb. under DSP	0.01081** (0.00417)	-0.00238 (0.00958)	0.01431*** (0.00417)	-0.00505 (0.00888)
Observations	17868	17868	17868	17868
R-squared	0.837	0.990	0.873	0.993
Time trend	-	-	Region	Region
Mean in 2012	-0.02563	9.38138	-0.02563	9.38138
Multiplier		.219 [.893]		.352 [.638]
H0: Multiplier $\geq$ 1.5		.076		.036

Notes. The table reports the impact of neighborhood-level exposure to the 2013 extensions of the Domestic Stability Pact (DSP) on neighborhood-level net budget surplus and municipal log per-capita income. Columns (1) and (3) report the coefficient $\hat{\gamma}$ from specification (3.2) with neighborhood-level net budget surplus scaled by neighborhood-level income in 2012 as the dependent variable (i.e., first-stage regression). Columns (2) and (4) report the coefficient $\hat{\gamma}$ from specification (3.2) with municipal log per-capita income as the dependent variable (i.e., reduced-form regression). Columns (3) and (4) include region-specific time fixed effects. Standard errors are clustered at the municipality level. The multiplier estimate and its standard errors are obtained from an IV regression with municipal log income per-capita as the dependent variable and the neighborhood-level net budget surplus as the main independent variable, instrumented by the neighborhood-level exposure to the 2013 DSP extension interacted with a dummy taking value 1 for all years after 2012. The last row reports the p-values obtained from one-sided tests for the multiplier being below 1.5. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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