Reinforcing Each Other: How the Combination of European and Domestic Reforms Increased Competition in Liberalized Industries

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May 2022

Abstract

This paper asks: how does the interplay between European and domestic dimensions of liberalization impact competition? It answers this question by carving out a space between two dominant accounts: the "EU-centric" and "strategic liberalization" views. These accounts differ in the relevance attributed to the Commission and governments and characterize these actors with contrasting goals. In contrast to both views, this paper claims there is an alignment of interests that produces a mutually reinforcing effect on competition. Using a staggered differences-in-differences design, I show that when European directives are combined with domestic pro-competition policies, we observe the most significant reduction in firm-level markups. Instead, when considered in isolation, these reforms do not increase competition. These results reveal the simultaneous importance of European authorities and domestic institutions for competition.

Several scholars claim that a revolution happened in European competition policy (Wilks and McGowan 1996; Wilks 2005, 2007). Oligopolies and entry barriers, once the norm, have been replaced by a strict European competition regime, a system that many consider the most pro-competition worldwide (Hylton and Deng 2007; Alemani et al. 2013). These institutional changes appear to be matched by increasing competition. European economies, since the 1990s, for a long time characterized by low competition (Alesina and Giavazzi 2008), experienced a period of profound transformation. Industries have become less concentrated (Kalemli-Ozcan et al. 2015; Gutierrez and Philippon 2018; Philippon 2019), and powerful incumbents have seen their market power diminishing (Badinger et al. 2007, Holland 2009, Weyerstrass and Jaenicke 2011).

The liberalization reforms that started in the 1980s and radically transformed European economies arguably represent an important competition policy contributing to this change. From a theoretical standpoint, liberalization can foster competition by eroding established monopolies via the reduction in entry barriers. Lower barriers stimulate firm entry (Blanchard and Giavazzi 2003) and, consequently, diminish the monopolistic power of large domestic incumbents (Levinsohn 1993; Tybout 2003). Levinsohn (1993) and Lu and Yu (2015) show the effectiveness of these reforms in decreasing firms' market power in Turkey

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and China, respectively. Similarly, Gutierrez and Philippon (2018) and Griffith et al. (2010) find analogous results using sectoral data on European industries.

Given this theoretical and empirical relevance, this paper focuses on the liberalization of European network industries of the 1990s and the early 2000s. The need for fixed infrastructure and increasing returns make entry and competition more difficult in these sectors. Consequently, governments have historically remedied this market failure through public management. However, technological progress and the need to increase the competitiveness of European firms vis-à-vis global competitors pressed for a radical transformation of these industries (Nicolaïdis and Vernon 1997, Foremen-Peck 2006). The Commission supervised this process of change, conceiving competition as a tool to increase efficiency. The removal of barriers was intended to widen markets and, consequently, increase firms' competitiveness and capacity to compete against extra-European rivals. Liberalization, however, also served the Commission's goal to integrate previously shielded industries into the Single Market (Zeit 2009; Pollak and Slominski 2011).

Existing accounts on liberalization, however, disagree on the main actors driving these reforms. Several scholars consider the Commission the leading agent behind liberalization while leaving limited space for domestic institutions. In this account, competition increases because the Commission successfully exploited its powers to win governments' resistance and increase competition in network industries (Sandholtz 1998; Pollak and Slominski 2011; Gutierrez and Philippon 2018). By contrast, comparative studies take governments more seriously, recognizing their importance in generating highly heterogenous reform trajectories (Jordana et al. 2006; Bulfone, 2019, 2020). Yet, they disagree with the prominence reserved for European institutions by the EU-centric studies. For this literature, MSs are the relevant actors which use liberalization strategically to increase their economic weight in the Single Market. Therefore, these two views see the Commission's and Member States' (MSs) interests as inherently contrasting. On the one hand, the Commission's pursuit of competition and a level playing field between MSs, and on the other, governments' expansionary ambitions.

In contrast to both views, I argue that – although apparently contrasting – these interests are instead aligned and can produce a mutually reinforcing effect on competition. The Commission in promoting competition requires MSs' information and knowledge to tailor European directives to the domestic industries' characteristics. At the same time, MSs' international ambitions require the Commission's support. Precisely, governments need that markets are opened reciprocally between MSs to promote their firms internationally; thus, they can ally with the Commission to ensure liberalization in reluctant MSs. Consequently, this reciprocal liberalization fosters competition by increasing the market size. National reforms, however, can encounter the resistance of powerful interest groups. Therefore, pro-liberalization governments can use the Commission as leverage to win domestic opposition and impose their agenda.

Nevertheless, this analysis goes with a necessary clarification: liberalization is evaluated only according to its effect on competition. This policy could also have serious undesirable effects, such as increasing poverty and job insecurity (Topalova 2010; Emmenegger et al. 2012).

With this in mind, however, I consider European network industries as the ideal laboratory to test my argument. Firstly, domestic reforms accompanied European directives in network industries, whereby it is possible to analyze the interplay of both dimensions. Secondly, European competition law gives the Commission strong constitutional powers over liberalization, making liberalized industries a "least likely scenario" to show the relevance of national institutions in contrast to the EU-Centric view. Thirdly, network industries went through a process of privatization which anticipated European directives of almost a decade. MSs can combine both elements of privatization and liberalization when implementing domestic pro-competition re-

forms. Domestic liberalization can favor entry into oligopolistic industries by licensing third-party operators and granting them access to public networks. Instead, privatization requires governments to sell their participation in previously state-owned companies. Nevertheless, privatization is an inherently national policy since the Commission cannot decide on the type of ownership (Clifton et al. 2006, Article 220 of the EC Treaty). Therefore, showing that privatization favored European directives would further strengthen my argument, given its intrinsic national dimension.

I test the paper's predictions using a staggered difference-in-differences methodology. The leading dependent variable operationalizes competition at the firm level through markups, a proxy of market power. Consequently, larger markups are symptoms of less competition. The effect of reforms on competition is assessed by comparing markups in firms belonging to liberalized industries (treatment group) vis-à-vis the rest of the economy (control group). Since liberalization directives occur at different periods, this technique is called "staggered." Markups and other firm-level financial information are obtained from unconsolidated Orbis Historical data. Finally, I interact the treatment variable (i.e., European liberalization) with an OECD index measuring the intensity of domestic pro-competition reforms to capture the combined effect of both dimensions.

This empirical analysis reveals that only the interaction of European and domestic reforms reduces market power in liberalized industries. In line with my argument, this effect is stronger in sectors where governments are more willing to cooperate with European institutions, highlighting the importance of information sharing. Furthermore, the joint impact of European reforms seems larger in industries characterized by initial low barriers to entry. This result indicates that the Common European framework may generate divergent dynamics and reveals the importance of early reforms. Finally, privatization can diminish market power but only when combined with liberalization at the European and national levels. This finding has two main implications. Firstly, privatization alone is not conducive to more competition. Secondly, it further supports the hypothesis that is anchored in comparative political economy that domestic reforms are critical to promoting institutional change.

The rest of the paper is organized as follows. Section 1 reviews the literature on European liberalization, while section 2 introduces the theoretical framework and formulates the hypotheses. Section 3 explains the data and variables. The empirical strategy and the results are discussed in section 4. Finally, section 5 concludes. A separate appendix contains additional information on data and variables and conducts further robustness checks.

1 Different Views on Liberalization

The radical change brought forward by European liberalization in the state-market relationship characterizing network industries has generated a vibrant scholarship. This literature can be broadly divided into two dominant accounts: the "EU-centric" and the "strategic liberalization" views. Sandholtz (1998) stands with the former when he claims that the Commission obtained complete control over the telecom industry liberalization by exploiting transnational interest groups and competition rules. For Sandholtz (1998), Article 86 of the Rome treaty generates a highly asymmetric distribution of power between the Commission and MSs since it gives the former the right to liberalize state-owned industries through directives. Pollak and Slominski (2011) share the same rationale when claiming that the Commission exploited the Treaty to overcome the MSs' resistance concerning the energy sector liberalization – a sector for a long time jealously shielded given its strategic importance for national security. Surely constitutional rules gave the Commission

a critical advantage during negotiations. Yet, as Eising and Jabko (2001) and Eising (2002) suggest, power can assume subtler forms. In this respect, they argue that the Commission was also able to persuade governments and shape their preferences over liberalization according to its preferred outcomes.

This EU-centrism, however, is not liberalization-specific, but it permeates a broader literature studying competition policy at large. This literature sees the Commission's extensive powers resulting from the convergence and centralization of competition law. On the one hand, the architecture of European competition policy and the Commission's activism have pushed MSs to conform their statutes to European standards (Dumez and Jeunemaitre 1996; Gerber 1998; Wardeen and Drahos 2002). On the other hand, the various amendments to the European competition law have increasingly centralized powers in the Commission (Warlouzet 2016). These trends have led many scholars to consider the Commission as the "hegemonic leader" in a competition constellation where MSs have a peripheral role (Wilks 2005, 2007).

EU-centric studies on liberalization, however, mainly focus on the design of policies while neglecting their economic effects. In this respect, the groundbreaking contributions of Gutierrez and Philippon (2018) and Philippon (2019) are among the few studies examining the economic consequences of the Commission's hegemonic powers. These authors argue that increasing competition in Europe results from MSs' decision to fully delegate powers to the Commission. In this account, the Commission acts as a powerful and independent regulator which promotes competition in the Single Market by opposing MSs' domestically circumscribed interests. While in Gutierrez and Philippon (2018) the MSs' role ends with this act of delegation, Philippon (2019: 136) acknowledges a greater role for governments in implementing the liberalization goals set in the Lisbon strategy. However, again, it is the Commission that ensures the compliance to these goals using "name and shame" and sanctions, hinting that liberalization would have been low otherwise.

In short, this EU-centric account elevates European institutions as the decisive factor in increasing competition in network industries. Yet, it is difficult to imagine that MSs' accepted an almost completely centralized control of network industries, given their current and historical strategic relevance. In this regard, comparative studies reveal that MSs have undertaken distinct reform trajectories. For instance, the Netherlands and Italy responded very differently to European demand for road haulage liberalization (Hèritier 1997). Because of its highly competitive firms, the Dutch government espoused the European requests enthusiastically and consequently designed fully-fledged liberalization reforms. Instead, in Italy, powerful sectoral interests saw liberalization as a perilous threat, whereby they forced policymakers to implement only partial reforms. A similar path is observed in the electricity sector, where France removed barriers to the minimum requirements prescribed by European directives while Germany adapted these reforms to its industrial context (Humphreys and Padgett 2006).

Although it is hard to deny the importance of the Commission for liberalization and competition policy more broadly, these comparative studies question the conclusions of the EU-centric literature. If only the Commission matters, why do we observe countries that have highly liberalized their industries and others that have maintained some barriers? Why did a so powerful and hegemonic leader not correct reluctant MSs?

The empirical evidence reveals some inconsistencies in the EU-centric explanation also concerning the economic effects of liberalization. If competition increases because of the Commission's initiatives, why did competition statistics evolve so unevenly across MSs, as shown by several studies (Christopoulou and Vermeulen 2012; Cook 2011; De Loecker and Eeckhout 2018)?

The second dominant view takes an opposite perspective concerning liberalization. The Commission has only a background role in domestic liberalization, or it does not even matter. In this respect, Jordana

et al. (2006) argue that Europe was not the motor for the electricity and telecom liberalization in Portugal and Spain. By contrast, these reforms were the strategic responses of governments to the changing global economic landscape. A similar conclusion is made by Levi-Faur (2003, 2004) for a broader set of countries. The common rationale of these studies is that governments strategically design liberalization to advance their firms in international markets (Clifton et al. 2006; Bulfone 2019, 2020). Thus, liberalization will allow the most successful firms to increase market shares in foreign sectors. Consequently, this literature predicts that only a few European champions will dominate network industries (Thomas 2003).

This literature enriches our theoretical understanding of liberalization by acknowledging the MSs' importance. However, the passive – and often neglected – role of European institutions contrasts with the Commission's mission of ensuring a level playing field and capacity to mediate MSs' interests. For example, this was the case in the energy sector when the Commission won the resistance of France and Germany by threatening to investigate EDF and EnBW, two of the most prominent players in these countries (Schumann 2003 in Pollak and Slominski 2011). Furthermore, the formation of "Gargantuan" European champions contrasts with the declining concentration (Gutierrez and Philippon 2018) and stable or decreasing markups (Cavalleri et al. 2019) in network industries.

2 Theoretical Framework & Hypotheses

2.1 Mutually Reinforcing Reforms

The empirical inconsistencies affecting both views reveal a challenging task afflicting scholars studying the political economy of European liberalization: the difficulty of reconciling domestic heterogeneity with the Commission's centrality within the multilevel competition system. In this paper, I try to reconcile both dimensions by advancing an alternative explanation conceiving the European and domestic institutions as mutually interdependent for the success of liberalization.

Heritièr (1997) and Heritièr and Knill (2000) identify the conditions under which MSs fully implement and even reinforce European directives. Precisely, governments' preferences must align with those of the Commission. Such an alignment depends on institutional (e.g., the importance of sectoral interest groups in decision-making) and economic (e.g., the chances of domestic firms to expand abroad) factors. In that, these authors can provide an explanation that takes account of domestic heterogeneity in liberalization. Heritièr (1997) and Heritièr and Knill (2000), however, do not explain how the alignment of interests should increase competition in liberalized industries.

I claim that when there is a commonality of interests between the European and domestic dimensions, liberalization has the greatest effect on competition. When designing directives, the Commission operates in an asymmetric information context: it lacks the sector-specific knowledge that national policymakers and officials can have. Therefore, domestic reforms are fundamental to adapting the general principles of European directives to the domestic economic and institutional ecosystem, thereby increasing their effectiveness. In contrast to Gutierrez and Philippon (2018), I depart from the Commission's super-imposition of a one-size-fits-all policy. Paradoxically, such a policy could not produce the desired effect on competition since it neglects the specific domestic industrial and institutional setting. In this respect, the adoption of the "Washington Consensus" in Latin America constitutes an example of standardized policies that disregard domestic specificities and produce undesirable consequences (Mukand and Rodrik 2005). Following the consensus, these countries massively liberalized their industries; yet, market power has evolved very unevenly,

decreasing in Chile while increasing in Argentina and Colombia (De Loecker and Eeckhout 2018). In light of Mukand and Rodrik (2005), the reason can be that standardized liberalization packages, while opening markets, did not change the formal and informal institutions shaping firms' interactions.

At the same time, supranational ambitions and domestic opposition can motivate governments' cooperation. Governments that open industries to favor the international expansion of successful firms need that other MSs liberalize their markets. Otherwise, the lack of reciprocal liberalization will constrain firms' international ambitions. Thus, governments need the Commission as a "liberalization arbiter," ensuring that liberalization is reciprocal between MSs. For this reason, governments can ally with the Commission and push together more reluctant MSs to liberalize their industries, thereby reinforcing the Commission's ambition of market integration. This mechanism can explain why countries that autonomously liberalized their industries, such as the Netherlands and UK, were also fierce supporters of European directives (Hèritier 1997, Eising 2002). Thus, these nationalistic motivations can promote competition because reciprocal liberalization ensures that firms compete on a larger scale.

On the domestic side, governments may be willing to reduce economic barriers significantly, but they could not possess the strength to win resistant vested interests without the Commission's support. Therefore, the European dimension can be leveraged to pursue domestic pro-competition goals. This leveraging on the Commission can take the form of genuine willingness to conform to European standards; alternatively, European authorities can be strategically used as an external "alibi" to impose the government's agenda (Börzel 1999).

The dependence of liberalization success on government cooperation can answer an implicit question in Héritier (1997) and Héritier and Knill (2000), but which is not addressed in their works: Why the Commission – with its preferences for liberalization and substantial formal powers - is willing to tolerate different levels of liberalization? In game-theoretic terms, the mutual need can explain why agreements between the Commission and governments can be sustained in equilibrium. The former finds the superimposition of full liberalization ineffective while increasing opportunities for domestic firms or the threats of sanctions lower governments' incentives to renege from agreements.

The following hypothesis summarizes the core prediction of the present framework.

Hypothesis 1: *European and domestic reforms produce a mutually reinforcing effect on competition. By contrast, their pro-competition effect is limited when these dimensions are considered autonomously.*

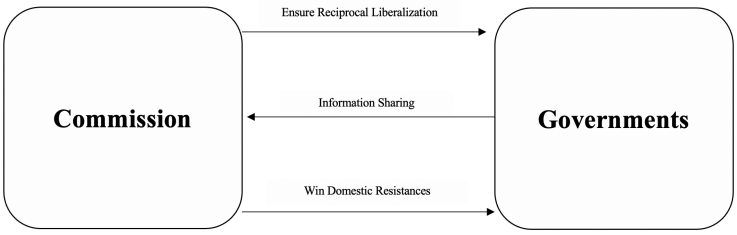

Figure 1 Mutual Reinforcement

Note: The arrow's origin indicates the actor driving the mechanism, while the arrowhead indicates the beneficiary.

The validity of hypothesis 1, however, is conditional on the rejection of the "null hypothesis" that

liberalization reforms do not matter and competition varies because of market dynamics. In other words, I need to make sure that the effect of these reforms, if any, is not confounded by some contingent economic force. A key concern is the impact of technological progress on firms' productivity. More productive firms tend to expand their size and gain market shares, thereby affecting industry competition (Autor et al. 2020). Another concern is the effect of liberalization on firms' cost structures. De Loecker et al. (2016) show that liberalization decreased the cost of accessing foreign technology for domestic firms in India. However, these lower costs were not perfectly "pass-through" lower prices, thereby increasing firms' capacity to gain excess profits. Thus, different productivity and cost-structure dynamics are alternative explanations that can impact the evolution of competition in liberalized industries, potentially confounding the effects of domestic and European reforms.

Finally, the increasing openness resulting from the Single Market launch can also generate paradoxical consequences affecting the validity of hypothesis 1. Increasing external competition reduces the marginal cost "cut-off" in a specific industry, and only firms which can produce at lower costs will make profits (Arkolakis et al. 2019). These are the most technologically advanced and productive enterprises known as "superstar firms." The superior productivity of superstar firms derives from labor-substituting technology, which allows them to reduce costs (Karabarbounis and Neiman 2014). For this reason, Autor et al. (2020) explain the secular decline in labor share with the rise of superstar firms. The political economy side of the superstar firms story is that corporations' capacity to relocate decreases employment security and the workers' bargaining power, contributing to the lower labor share (Scheve and Slaughter 2004; Shadmehr 2019).

In the long run, however, competition will deteriorate as low-productivity businesses will exit the market, and superstar firms will acquire increasing market shares. Nonetheless, this phenomenon tends to be more prevalent in tradable sectors given their natural exposition to fiercer foreign competition than network industries. Therefore, any potential relative increase in competition found in liberalized industries might be due to decreasing competition in other sectors of the economy. For this reason, ruling out these alternative mechanisms is a necessary condition for the validity of the central thesis of this paper.

2.2 Industry & Country Heterogeneity

A corollary of my argument is that the combined effects of European and domestic reforms should be greater in industries where governments were more willing to cooperate with European institutions. Higher cooperation results in greater information sharing and willingness to complement European directives with national reforms. Thus, industry heterogeneity can be exploited to further test the central claim of this paper by investigating the "information sharing mechanism." Comparative political-economic studies often engage in a two-sector analysis to evaluate liberalization. Therefore, they represent an excellent source for understanding the different levels of cooperation between the EU and governments. In most cases, however, these studies focus on comparing the telecom and electricity sectors. Thus, extending a potential cooperation scale to other industries is difficult. For this reason, I will limit my attention to the telecom and electricity industry.

In contrast to the electricity sector, there was a relatively widespread agreement that the telecom industry should be liberalized (Eising 2002: 104). Technological innovation and increasing economic opportunities increased the MSs' willingness to liberalize this industry (Bartle 2002; Humphreys and Padgett 2006). On the other hand, the risk of employment losses and national security considerations made MSs more reluctant to make concessions during the energy sector liberalization (Levi-Faur 1999, Pollak and Slominski 2011). Yet,

although less, some cooperation took place in the electricity sector. Therefore, I expect multilevel reforms to increase competition in this industry, although less than in the telecom sector.

Hypothesis 2: *The combined effect of European and domestic reforms on competition should be lower in the electricity sector than in telecom but still positive.*

The above hypothesis contrasts with the EU-centric view. If competition increases mainly because of European institutions, we should instead observe a homogenous effect across industries once sectoral economic characteristics are considered.

Besides industries, countries represent an important dimension of heterogeneity. In this respect, it seems legit to ask whether there has been a certain degree of convergence across MSs. After all, European countries belong to the same competition system. The centrality of mutually reinforcing reforms, however, generates two rival implications concerning convergence. These implications can help understand which mechanism is more prevalent between Commission's role as liberalization arbiter or as external leverage against resistant domestic interest.

On the one hand, European institutions may increase competition by helping MSs to win domestic interests opposing liberalization. If this is the case, we should observe a stronger effect in MSs with higher ex-ante restrictions, thereby generating a process of convergence. This explanation aligns with Hèritier (1999), who affirms that the pressure for convergence is greater the larger the distance from European standards.

On the other hand, European directives may have amplified the existing differences between MSs. Countries with more successful enterprises liberalized industries before European directives and might have pressured the Commission to induce liberalization in other MSs. Yet, European directives could have been more effective in countries with initially low barriers, given the easier entry from foreign firms. In other words, these early initiatives may have created a fertile ground for European directives.

Hypothesis 3a: *European institutions have a stronger pro-competition effect in MSs with initially higher barriers.*

Hypothesis 3b: *European institutions have a stronger pro-competition effect in MSs with initially lower barriers.*

2.3 Disentangling Domestic Pro-Competition Policies

Until now, I have used the term pro-competition reforms rather than liberalization to refer to national policies. This semantic choice is motivated by the fact that domestic reforms can involve both liberalization and privatization. Indeed, these two terms are often used interchangeably since these policies tend to be highly correlated (Belloc et al. 2014). Nonetheless, a neat distinction between liberalization and privatization is highly relevant to test the core argument of this paper.

Indeed, European directives aimed at reducing entry barriers without any element of privatization. The reason is that the EU must be ownership neutral: its role is limited to ensuring that effective competition is achieved in a specific sector (Clifton et al. 2006, Article 220 of the EC Treaty). However, MSs, when implementing domestic reforms, can combine both policies. Although these policies have often been paired, their mix varies substantially across countries. For example, countries like Ireland and the United Kingdom privatize their industries significantly more than France and Germany (Clifton et al. 2006).

The timing and inherently domestic nature of privatization, however, can represent a further check for the robustness of my framework. Privatization, on average, started one decade in advance of European liberalization. As Clifton et al. (2006) argue, some MSs autonomously privatized their industries to facilitate the reception of European liberalization directives. Therefore, showing that privatization increased competition - when considered in conjunction with liberalization directives - would further corroborate the claim that domestic and European reforms are mutually reinforcing. Furthermore, the inherently national nature of privatization can defend the analysis from the potential critique that domestic reforms – despite their heterogeneity – are simply the result of the Commission imposing its will on MSs, which, otherwise, would not have implemented those policies.

It is necessary to understand, however, how privatization can increase competition. Privatization alone means that state-owned enterprises become private, but it does not require reducing entry barriers to competition. Therefore, privatization per se is not conducive to more competition, but it can simply transform a public into a private monopoly (Belloc et al. 2014). On the other hand, liberalization can increase competition by decreasing entry barriers without explicitly requiring a transfer of ownership from public to private.

Nevertheless, I argue that privatization may enhance the pro-competition effects of liberalization. It is true that high fixed costs characterizing network industries reduced business dynamism and motivated government ownership (Sappington and Stiglitz 1987). Yet, this rationale may be less relevant in the present European economy, where firms from different countries can compete more easily. By contrast, foreign firms might be discouraged from investing in countries where powerful incumbents are publicly owned since they could feel a lack of a level playing field. Indeed, Governments tend to support more state-owned firms, which also have higher access to insider information (Sarkar et al. 1999, Bonardi et al. 2004). In this light, the Commission might see favorably privatization initiatives that complement liberalization directives since they can promote cross-country investments, in line with its goal of fostering economic integration. Finally, besides theoretical motivations, there is a vast empirical literature on reform sequencing in developing countries, showing the positive effect of privatization on competition when implemented together with liberalization (see Bagdadioglu and Cetinkaya 2010 for a review).

Hypothesis 4a: *Privatization amplifies the effect of European directives on competition.*

Hypothesis 4b: *Privatization alone does not increase competition.*

3 Data & Variables

The dataset used in the empirical analysis contains nearly 3 million firm-year observations for fourteen European countries between 1995 and 2013. The countries in the sample are the EU-15 nations minus Luxembourg, given its small economy. The analysis starts in 1995, when Austria, Denmark, and Finland joined the EU. Finally, since most liberalization reforms happened in the nineties and early 2000s, I have decided to exclude Eastern European countries because they were not EU members at the time. These data have an inherently multilevel nature. At the top, we have European directives affecting all countries in the same year. The second level of aggregation is countries within which we have industries. Finally, firms operating in each sector are the ultimate unit of analysis.

Markups. The primary dependent variable used is the firm-level markup μ. This variable is the ratio between price and marginal cost $\mu = \frac{p}{c}$. Firms charge a price equal to the marginal cost in perfectly competitive markets and make zero profits. Therefore, large markups indicate lower competition. Markups have been estimated using firm-level data from Orbis historical archives. Orbis dataset is provided by Bureau van Dijk and contains balance sheet information for European firms. These data have been used to implement a markup estimation technique based on the control function approach (Olley and Pakes 1996; Levinsohn and Petrin 2003; De Loecker and Warzynski 2012; De Loecker et al. 2016, 2020). This technique requires estimating a 2-digit industry production function and modeling the evolution of unobserved firms' productivity.[1] For most of the 14 in the sample, the control function is defined on material costs, as in De Loecker and Warzinsky (2012) and De Loecker et al. (2016). However, few Danish, Greek, Irish, and British firms in Orbis report materials expenditures. Thus, following De Loecker et al. (2020), I have defined the control function for these countries using the cost of goods sold to increase data coverage.

The industrial organization literature generally prefers markups to concentration indexes to measure market power. The main reason is that concentration accurately measures market power when firms engage in quantity competition à la Cournot (Breshahan 1989). Unfortunately, even in Cournot-type industries, concentration fails to adequately capture market power when products are differentiated (De Loecker and Eeckhout 2018).

Nonetheless, even firm-level markups also have limitations. For instance, unobserved firms' prices can cause an omitted variable bias. Fortunately, this bias neither affects the evolution of markups over time nor the correlation between markups and firm-level characteristics (De Loecker and Warzynski 2012). Thus, this bias does not seem particularly severe for this study. Another source of concern is that markups, especially for the energy sector, might be affected by input prices and so they cannot adequately capture market power. Nevertheless, my estimation technique allows me to circumvent this problem since markups are estimated in real terms. Finally, the top and bottom 5% of the markup distribution have been trimmed to avoid outliers that could bias the empirical analysis. However, the appendix shows that the main results are robust to different levels of trimming.

European Directives. European directives impacted six sectors: aviation, electricity, gas, postal services, and railways. Aviation, however, is excluded from the analysis since the liberalization of this industry started in 1987, a period where Orbis Historical has not sufficient data coverage. Moreover, another reason to exclude it is that Austria, Denmark, and Finland were not European Union countries when aviation was liberalized.

I have used the timing of liberalization directives to code a treatment variable (eu) that varies across liberalized industries. This variable takes the value of 1, the year of the deadline for the transposition of the first liberalization package.[2] Finally, table 1 assigns an industry NACE code to each liberalized industry following the mapping in Gutierrez and Philippon (2018: 26). However, unlike these authors, I adopt a more granular industry definition for electricity, gas, and railways using 3-digit instead of 2-digit codes. This choice allows me to assign financial information from Orbis to firms in these industries more precisely. Indeed, two-digit codes make it impossible to separate electricity and gas, although these industries were liberalized by two different directives.

[1] I have used a Cobb-Douglas production function with unobserved productivity following a Gauss-Markov process of order 1, as in De Loecker et al. (2020). More information about the estimation process and data used are found in the appendix.

[2] In the case of telecom, I have considered the "full liberalization directive," which sets the deadline for full liberalization the 1st of January 1998.

Table 1 Directive Timeline

Liberalized Industry	Directive	Year	Transposition/Effectiveness	NACE Code
Telecom	96/19/EC	1996	1998	61
Electricity	96/92/EC	1996	1999	351
Gas	98/30/EC	1998	2000	352
Postal	97/67/EC	1997	1999	53
Railways	2001/12/EC	2001	2003	491

Domestic Reforms. Following the literature, I have defined a variable capturing the intensity of domestic reforms in liberalized industries starting from the OECD Product Market Regulation (PMR) indicator (Alesina et al. 2005; Belloc et al. 2014; Gutierrez and Philippon 2018). The OECD provides this indicator for several network industries and professional services at the country level (more details in Nicoletti and Scarpetta 2003 and Alesina et al. 2005). The overall PMR is composed of four different sub-indicators measuring: entry barriers, public ownership, the market share of dominant players, and vertical integration. These sub-indicators have been firstly computed at the most granular industry definition available. Then, they are aggregated for each network industry using simple or revenue-weighted averages. Finally, the overall PMR score is computed as a simple average between the four components. This indicator ranges from 0 to 6, where higher values denote more restriction to competition.

Since the objective is to broadly capture the effect of domestic pro-competition reforms, I have tested hypotheses 1, 2, 3a, and 3b using the overall PMR score. Since MSs may have engaged in privatization to facilitate the reception of liberalization directives (Clifton et al. 2006), it is crucial to maintain both privatization and liberalization. However, the sub-indicators have been used to identify the diverse dynamics associated with national liberalization and privatization reforms.

Instead of using the PMR in levels, I have used its change from the first year of availability. Therefore, if, for instance, a given directive happens in year t, this index reflects the change in domestic pro-competition reforms in a specific industry before that directive. Indeed, figure 4(a) (section 4.2) shows that MSs started reforming their industries before European directives. Arguably, European institutions could have influenced the PMR index before a directive implementation. Nonetheless, the increasing country variance the PMR index (figure 4(b)) illustrates the existence of inherently different domestic trajectories. Hence, the PMR index can be helpful to capture the intrinsically national component of pro-competition reforms. Furthermore, since European liberalization directives were implemented throughout the nineties, going back to the mid-1970s could seem excessive. However, by doing so, I can capture the effects of policies in first-mover countries and the efforts that some MSs made to facilitate the reception of European directives. Finally, I have defined ΔPMR, by interacting the change in PMR with a sector dummy, indicating whether an industry has been liberalized through a European directive. In this way, ΔPMR measures the intensity of domestic pro-competition reforms in sectors affected by European legislation.[3]

Controls. To identify the causal effect of institutional variables, I have included a series of covariates that can control for alternative economic mechanisms affecting markups. Larger and more productive firms

[3] ΔPMR has been multiplied by -1, so larger values denote more pro-competition reforms.

tend to charge higher markups and consequently lowering competition, as discussed in section 2.1 (Autor et al. 2020; De Loecker et al. 2020). Therefore, I include revenues (to proxy size) and productivity to control for these potential confounding factors.[4] I also control firms' "imperfect pass-through" behavior since it can confound the effect of liberalizations on markups (De Loecker et al. 2016). Unfortunately, Orbis data does not allow me to estimate firms' marginal costs, which are necessary to assess changes in cost structures. Thus, I have proxied this variable using the unit variable cost computed as the ratio between firms' variable costs and output.[5]

In addition to the controls used in the baseline model (i.e., size, productivity, and unit variable costs), I include openness and capital intensity to control for the "superstar firms" alternative mechanism. To proxy for openness, I use sectoral import penetration (as in Levinsohn 1993) and the ratio between sectoral exports and output, which are computed using OECD Stan data.[6] These variables allow controlling for openness in both directions: "in and out" a given industry. Finally, I use capital intensity, measured as the ratio of total assets over revenues, to control for lower employment shares characterizing superstar firms.

4 Empirical Strategy

I estimate the effect of domestic and European reforms using a staggered differences-in-differences (DID) methodology. This approach represents a popular quasi-experimental identification strategy to recover the causal effect of policies (Cunningham 2021). Treatment and control units are compared before and after intervention, whereby different outcomes can be attributed to the policy. In the present study, the treatment group comprises firms in industries liberalized through European directives, whereas the rest fall in the control group. This technique is "staggered" because industries receive the treatment at different periods according to the timeline of liberalization directives. The use of a quasi-experimental method differentiates this study from previous quantitative studies on liberalization (e.g., Alesina et al. 2005; Gutierrez and Philippon 2018).

A critical identification requirement of this design is the conditional independence assumption (CIA). This assumption requires the absence of confounding factors creating a spurious and non-causal correlation between the treatment and the outcome. If certain variables, however, that determine treatment assignment and simultaneously affect the outcome are not controlled for, then the CIA is not satisfied. Arguably, the low productivity, the presence of large oligopolistic incumbents, and the cost-inefficiencies of state-owned network industries motivated their liberalization. At the same time, productivity, size, and cost structure determine markups. Therefore, causal identification requires controlling these factors in the baseline model.

Table 2 reports the mean of covariates in the treatment and control group, plus the standardized difference, a common statistic used to assess balance. At first glance, it may seem that certain variables, such as revenues, present some imbalances. However, when looking at the standardized difference, these imbalances are not particularly severe, and the value of this statistic is relatively similar to studies with analogous designs (e.g., Prager and Schmitt 2021). In any case, section B.2 shows techniques to remediate potential issues concerning treatment and control group imbalances.

[4] Firm-level productivity has been estimated using the same methodology adopted for markups. More details are found in the appendix.

[5] Firms' output has been obtained by deflating sales using the GDP deflator.

[6] Import penetration is computed as $\frac{sector\ imports}{sector\ gross\ output + sector\ imports - sector\ exports}$. Since high-productivity firms tend to export more and charge larger markups (Melitz and Ottaviano 2008; De Loecker and Warzynski 2012), it would be ideal to have firm-level data on export-revenues. Unfortunately, this statistic is available only for a few firms in Orbis.

Table 2 Summary Statistics

	Liberalized Industries	Other Industries	Standard Difference
Markups	2.40	1.74	0.24
Revenues	€370.54	€23.65	0.25
Productivity	0.44	0.24	0.33
UVC	€0.61	€1.15	-0.11
Export Ratio	0.09	0.14	-0.50
Import Penetration	0.08	0.14	-0.56
Capital Intensity	3.67	11.81	-0.12

Note: Revenues are reported in million.

4.1 Hypothesis 1

In this section, I test hypothesis 1, conceiving European and domestic reforms as producing a mutually reinforcing effect on competition. To do so, I follow a two-step approach. Firstly, I run the following two-way fixed effect DID regression:[7]

$$\log \mu_{jict} = eu_{it} + X_{jict} + \alpha_j + \tau_t + \epsilon_{it}, \tag{1}$$

where only European liberalization affects markups once baseline controls are included.[8] I also use firm fixed-effects (α_j) to control for time-invariant firm-level characteristics (e.g., location) and year effects (τ_t) to account for time-varying unobserved factors that are common across firms (e.g., crises). Secondly, I run a similar specification where I add my measure of domestic pro-competition reforms and its interaction with the eu variable:[9]

$$\log \mu_{jict} = eu_{it} \times \Delta PMR_{ict} + eu_{it} + \Delta PMR_{ict} + X_{jict} + \alpha_j + \tau_t + \epsilon_{it}. \tag{2}$$

While eu_{it} and ΔPMR_{ict} measure the autonomous effect on competition of European and national reforms, respectively, the interaction $eu_{it} \times \Delta PMR_{ict}$ captures the combination of these two-level policies.

The first column of table 3 shows the results of model (1). European liberalization directives reduced market power, although this effect is only moderately significant. The negative impact of European liberalizations on markups is not surprising, given the extent and scope of these reforms. However, the non-inclusion of domestic reforms could result in biased estimates if these are also relevant for competition. If this is the case, we should observe estimates to change significantly when these are included.

[7] Standard errors are clustered at the country-industry (NACE 2-digit industry) level as common in the literature (see De Loecker et al. 2016). Additionally, clustering standard errors avoid autocorrelation issues affecting DID studies with several time periods (Bertrand et al. 2004).

[8] As standard in the literature, I use the log of economic variables to linearize possible non-linear relationships between the dependent and the independent variables. Baseline controls are the log of revenues, productivity, and UVC. The subscripts have the following meaning: j denotes firm, i industry, c the country, and t the year.

[9] Callaway and Sant'Anna (2020) warn about the possibility of biased estimations in DID with two-way fixed effects. Therefore, I re-run the baseline estimation using their methodology in the appendix, obtaining similar results.

Table 3 Main Specification Results

	(1) EU-Directives	(2) EU-Directives & National Reforms
eu	-0.077*	0.030
	(0.045)	(0.059)
ΔPMR		0.018
		(0.030)
$eu \times \Delta PMR$		-0.038***
		(0.011)
Firm Effects	Yes	Yes
Year Effects	Yes	Yes
Observations	2,959,344	2,959,344
R-squared	0.930	0.930

Note: *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1. Baseline controls are included. Standard errors are clustered at the country-industry level.

Therefore, I run model (2), which also accounts for the national dimension of pro-competition policy. Indeed, in this specification, estimates change substantially (table 3, second column). The European directives' effect on competition is no more significant. By contrast, the interaction of European and domestic reforms has a negative and strongly significant effect on market power. In line with hypothesis 1, the Commission alone may not possess the sector-specific information to tailor liberalization to the domestic context. Therefore, domestic institutions are necessary for the effectiveness of European directives.

At the same time, the non-significant coefficient of ΔPMR indicates a mutual relationship. Firstly, MSs may need the Commission as a "liberalization arbiter", ensuring that reforms are reciprocal between countries and firms can thus compete on a larger scale. Otherwise, domestic reforms in isolation can produce only a limited effect, as the insignificance of ΔPMR suggests. Secondly, certain MSs may not possess the political capital to carry on these reforms without the Commission's support. The non-significant effect of ΔPMR, however, has another possible meaning. MSs have low power to systematically distort competition reforms to advantage their firms in a system where the Commission possesses monitoring and sanctioning powers. This result, thus, goes against the conclusions of the strategic liberalization view.

Figure 2 plots the marginal effect of eu at ΔPMR different levels to appreciate the magnitude of this interdependent effect. At the ΔPMR maximum level (i.e., 6), European directives reduce log markups by nearly 54% (11% in levels) more than when $\Delta PMR = 0$. This result indicates that the effect of European directives monotonically increases with domestic efforts.

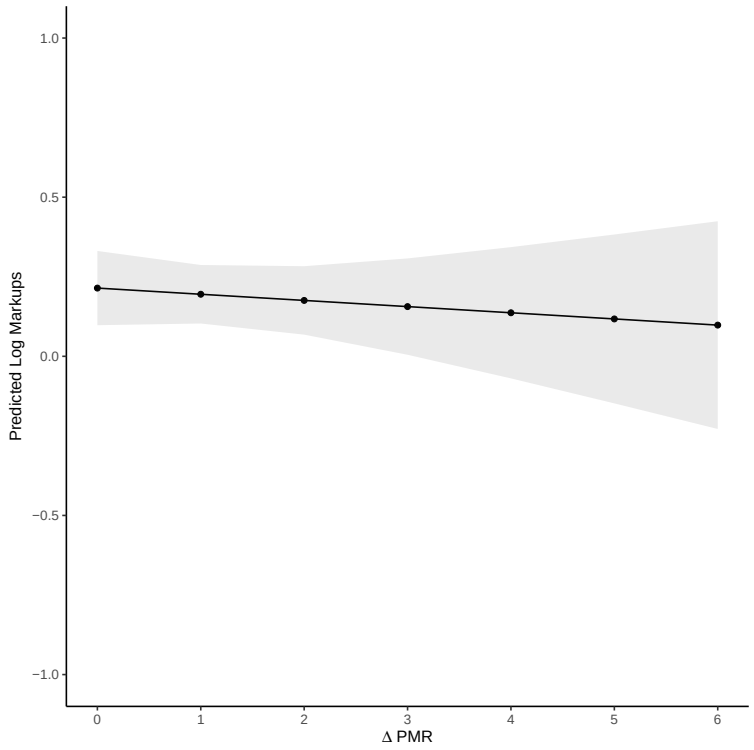

Figure 2 Marginal Effects of European Directives on Log Markups

I re-run equation (2) by trying different combinations of fixed effects to control for potential time-varying factors affecting countries and industries. Specifically, I add country-year effects (table 4, first column) to control for macroeconomic shocks such as crises that tend to diminish markups (Aghion et al. 2008; Weche and Wambach 2021). Industry-specific effects (table 4, second column), instead, control for different levels of technological innovation across sectors (Aghion et al. 2008), which can impact the markup distribution (Autor et al. 2020). Industries are defined following KLEMS segments, which for certain industries are less granular than the NACE-2d. This choice allows me to account for factors that can potentially affect similar industries transversally. Finally, I add country-industry effects (table 4, third column) to control for country-specific sector technological developments.

Table 4 shows that the interaction between supranational and domestic reforms remains the critical factor that decreases market power. The coefficient of $eu \times \Delta PMR$ is always negative, and its magnitude remains similar across every specification. Furthermore, controlling for sector-specific technological developments reinforces the importance of political choices for competition. In this respect, Levi-Faur (2003, 2004) concludes that technological progress drove liberalization, while this policy would have diffused anyway without European and intergovernmental dynamics. Undoubtedly, innovation can be an important factor in liberalizing an industry as it can facilitate competition. However, these results show that accounting for technological dynamics does not diminish the relevance of European and domestic institutions.

Table 4 Alternative Fixed Effects

	(1)	(2)	(3)
eu	0.002	0.041	0.045
	(0.053)	(0.069)	(0.049)
ΔPMR	0.018	0.037	-0.011
	(0.028)	(0.039)	(0.035)
$eu \times \Delta PMR$	-0.023**	-0.053***	-0.035**
	(0.010)	(0.018)	(0.014)
Firm Effects	Yes	Yes	Yes
Country-Year Effects	Yes	No	No
Industry-Year Effects	No	Yes	No
Country-Industry-Year Effects	No	No	Yes
Observations	2,959,334	2,959,344	2,959,075
R-squared	0.931	0.930	0.933

Note: *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1. Baseline controls are included. Standard errors are clustered at the country-industry level.

4.1.1 Parallel Trends

All the previous results would be invalidated if "parallel trends" - the critical identification assumption of the DID design – are not satisfied. This assumption requires that absent the treatment, the outcome in the treatment and control group changes at the same rate. The violation of parallel trends results in the CIA violation and biased causal effect. Following standard practices, I check for parallel trends by plotting the $eu \times \Delta PMR$ and after a European directive occurs. Specifically, I interact this variable with a binary indicator for the four years before the adoption, the seven years after, and the eight and all the remaining years (as in Autor 2003). For the parallel trends assumption to be satisfied, the pre-treatment differences should be statistically zero (Cunningham 2021).

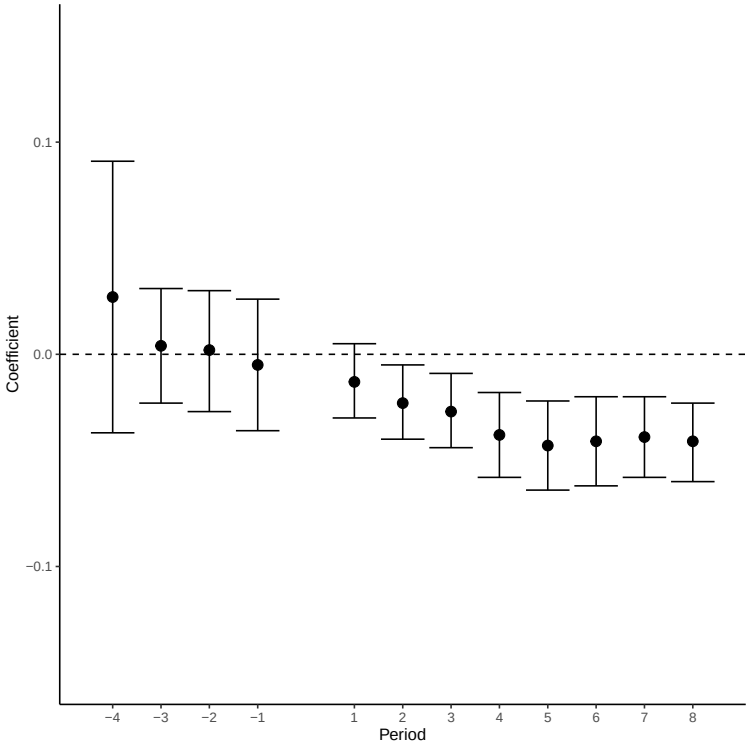

Figure 3 Parallel Trends

Figure 2 supports the existence of parallel trends since every pre-intervention coefficient is not significant. Thus, divergent pre-treatment trends potentially biasing the estimated causal effect appear to be absent. Besides being a helpful check for parallel trends, lagged coefficients also serve as a "placebo test."[10] The rationale behind placebo tests is to improve the soundness of the research design by checking whether a fictitious treatment affects the outcome. In this specific case, a placebo test using lagged treatment coefficients allows checking for Granger's (1969) causality by investigating whether "causes happen before consequences" (Angrist and Pischke 2008: 237). This test seems satisfied since pro-competition policies reduce markups only after their implementation. Finally, figure 3 shows that the combined pro-competition effect of domestic and European reforms gains strength with time. This behavior seems plausible since these reforms often radically change the industrial organization of a sector, whereby they need time to manifest their effects entirely.

4.1.2 The "Superstar Firm" Alternative Explanation

Eventual superstar firm dynamics – resulting from increasing trade openness in sectors more exposed to foreign competition – can obfuscate the causal interpretation of mutually reinforcing reforms. Precisely, If competition deteriorates in these sectors because of superstar firms, this will create an artificial negative coefficient of $eu \times \Delta PMR$, given the inherently comparative nature of the DID methodology. Therefore, to rule out this alternative explanation, I re-run model (2) by controlling for industry openness and capital intensity, firstly separately and then together.

Table 5 shows that in each of these new three models, the size of the $eu \times \Delta PMR$ coefficient is only

[10] Additional placebo tests are conducted in the appendix.

marginally changed. These results thus indicate that the joint effect of European and domestic competition reforms is not altered by potential superstar firm dynamics in other sectors. This finding, however, does not mean that these dynamics are absent. Indeed, Autor et al. (2020) find evidence of superstar firm effects in certain European countries, although less pronounced than in the US. Finally, the robustness of $eu \times \Delta PMR$ to superstar firms dynamics and the inclusion of various economic controls indicates that reforms matter for competition once contingent market mechanisms are considered, thereby rejecting the "null hypothesis" that market forces – and not political choices – matter for competition.

Table 5 Opennes & Capital Intensity

	(1)	(2)	(3)
eu	0.029	0.014	0.019
	(0.063)	(0.056)	(0.058)
ΔPMR	0.034	0.014	0.026
	(0.027)	(0.027)	(0.024)
$eu \times \Delta PMR$	-0.037***	-0.031***	-0.032***
	(0.013)	(0.010)	(0.012)
Firm Effects			
Year Effects			
Observations	2,202,454	2,959,272	2,202,406
R-squared	0.939	0.931	0.941

Note: *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1. Standard errors are clustered at the country-industry level. All the columns include baseline controls. In addition, the first includes the log of the export ratio and import penetration, while the second one the log of capital intensity. The third one includes all these additional controls together.

4.2 Hypothesis 2 & 3

After testing hypothesis 1, this section investigates its thrust at a more granular level. If complementarities between the European and domestic dimensions magnify the effect of liberalization through greater information sharing, then this effect should be larger in industries where cooperation was greater (hypothesis 2). In section 2.2, I borrowed the insights of comparative political economists to hypothesize that competition should have increased more in the telecom than in the electricity sector, given that, in the former, liberalization was less contested.

Now, I test this assertion by focusing on the two industries separately. I firstly run equation (2) considering only the telecom or electricity liberalization.[11] Therefore, the variables eu, ΔPMR, and their interaction capture dynamics related to the specific industry examined. I repeat this exercise but with a control group limited to the same KLEMS segment (i.e., utilities) for electricity or a broader sector definition for telecom. I use the "information and communication" sector for telecom because this industry itself coincides with a KLEMS segment. The logic of this more restricted control group follows the same rationale

[11] In the appendix, I also include capital intensity to control for different fixed cost structures characterizing the two industries.

used in the previous section. In each of these specifications, I exclude the other liberalized industries which otherwise will be included in the control group.

In table 6, we can evaluate the results of this exercise. When the control group is the entire sample, the effect of multilevel reforms on markups is 66% larger in the telecom than in the electricity industry. A similar difference is found when the control group is the same KLEMS segment or sector.These results, therefore, can be interpreted as supporting cooperation and the information-sharing mechanism at the basis of hypothesis 2.[12]

Table 6 Electricity vs. Telecom

	Entire Sample		Same Klems/Sector	
	Electricity	Telecom	Electricity	Telecom
eu	-0.035	0.273	0.013	0.359**
	(0.105)	(0.198)	(0.084)	(0.170)
ΔPMR	0.042	0.069	0.001	0.056
	(0.049)	(0.059)	(0.040)	(0.053)
$eu \times \Delta PMR$	-0.056**	-0.093**	-0.065***	-0.101**
	(0.026)	(0.045)	(0.019)	(0.040)
Firm Effects	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes
Observations	2,953,465	2,953,747	19,338	55,891
R-squared	0.930	0.930	0.923	0.903

Note: *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1. Standard errors are clustered at the country-industry level. All the columns include baseline controls.

The effect of cooperation in the telecom industry can also be appreciated by focusing on more institutional and legislative aspects. Figure 4(a) shows that, on average, the PMR score decreased more in the telecom than in the electricity industry. Furthermore, the increasing variation of PMR (figure 4(b)) indicates that - although domestic reforms might have been done in anticipation of European legislation - MSs possess a significant agency over the liberalization process in contrast to the hegemonic view.

[12] In table 6's last columns, eu has a positive and significant coefficient. A possible interpretation is that opening this industry across MSs has forced firms to acquire a larger size and, consequently, charge larger markups with respect to firms in the same sector. However, the negative coefficient of ΔPMR shows that domestic reforms can complement European directives and reverse this effect.

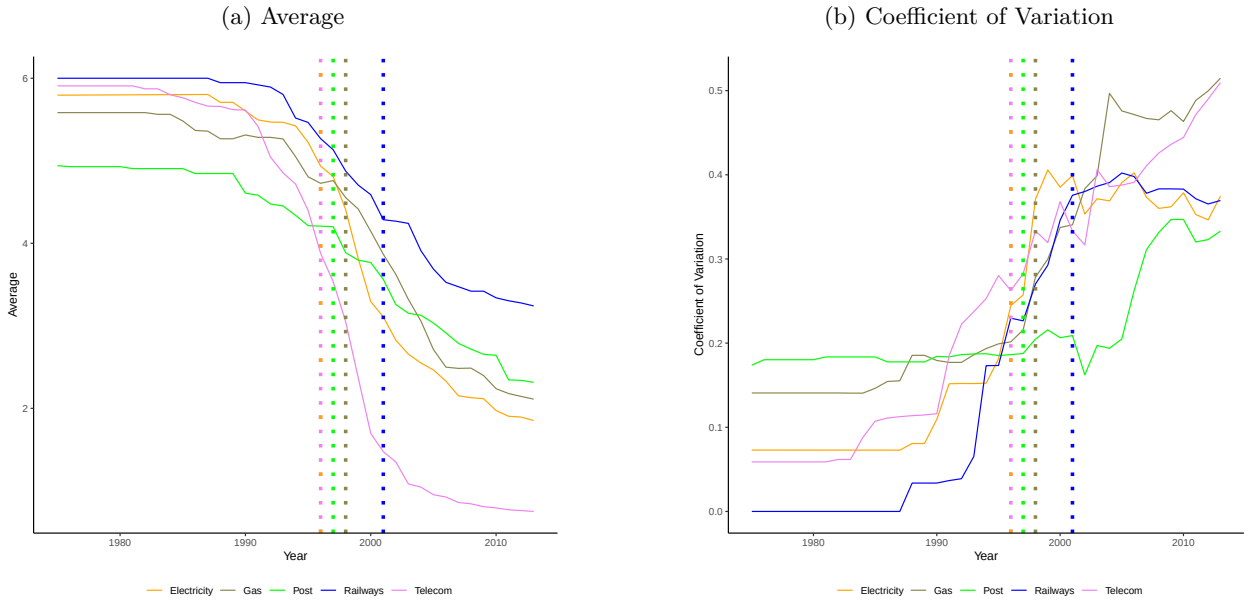

Figure 4 Product Market Regulation in Liberalized Industries, 1975-2013

Note: The PMR indicator ranges from 0 to 6 with larger values indicating more restriction to competition (the opposite than ΔPMR). The average and the coefficient of variation (i.e., the standard deviation divided by the mean) have been computed across countries. Dotted lines indicate the year of European directives.

Figure 4, however, reveals interesting dynamics also concerning convergence. On average, there has been a tendency to converge towards lower restrictions, in line with hypothesis 3a. At the same time, figure 4(b) shows that countries implemented these reforms with increasing variation over time. This latter trend has ambiguous implications concerning convergence. If the increasing variation is due to laggard MSs removing barriers faster than countries with ex-ante lower restrictions, we observe convergence. If, however, initially better-positioned countries reformed their industries more, then the reverse is true.

To resolve this ambiguity, I run the two models below:

$$\log \mu_{jict} = \sum_{v=1}^{4} eu_{it} \times Q_{t_0}^{v} + X_{jict} + \alpha_j + \tau_t + \epsilon_{it}. \tag{3}$$

$$\log \mu_{jict} = \sum_{v=1}^{2} eu_{it} \times M_{t_0}^{v} + X_{jict} + \alpha_j + \tau_t + \epsilon_{it}. \tag{4}$$

In model (3), I interact eu with a dummy Q_{t0}^{v} denoting the quartile of the EU-wide PMR distribution in which a given domestic liberalized industry falls in an initial period t_0. I have subtracted -6 and then multiplied PMR by -1 so that industries with lower restrictions are in higher quartiles. Similarly, the indicator $M_{t_0}^{v}$ is defined on the median of the same distribution. In this case, domestic liberalized industries with restrictions below the median in t_0 have $M_{t_0}^{2} = 1$, while for those below $M_{t_0}^{1} = 1$. I have decided to use both quartiles and median to make results less dependent on the specific classification. The subscript t_0 can assume three values to reduce the findings' sensitivity to the initial period chosen. Firstly, I consider 1995, when Austria, Denmark, and Finland joined the EU. Secondly, I use 1998, when the first directive analyzed

became effective. Finally, I also consider as a starting point the PMR average between 1975 (the first year available) and the year of the European directive affecting the industry.

Table 7 Ex-ante restrictions and reforms.

	(1)	(2)	(3)
	1995	1998	Pre-Liberalization Average
		Quartiles	
$eu \times Q^1_{t_0}$	-0.007	-0.055	-0.124
	(0.067)	(0.096)	(0.095)
$eu \times Q^2_{t_0}$	0.014	-0.035	-0.034
	(0.185)	(0.141)	(0.209)
$eu \times Q^3_{t_0}$	-0.195	-0.034	0.255*
	(0.202)	(0.116)	(0.136)
$eu \times Q^4_{t_0}$	-0.076*	-0.101**	-0.098**
	(0.043)	(0.041)	(0.038)
		Median	
$eu \times M^1_{t_0}$	0.005	-0.046	-0.069
	(0.115)	(0.088)	(0.130)
$eu \times M^2_{t_0}$	-0.091**	-0.081*	-0.079*
	(0.046)	(0.048)	(0.042)
Firm Fixed Effects	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes
Observations	2,959,338	2,959,344	2,959,344
R-squared	0.930	0.930	0.930

Note: *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1. Baseline controls are included. Standard errors are clustered at the country-industry level.

Table 7 shows that European directives seem to produce a negative and significant effect on markups in liberalized industries at the bottom of the restrictions distribution, both when using quartiles and medians. These results, therefore, suggest that MSs reforming their industries in advance are also those where European legislation generated the largest pro-competition effects, indicating divergent effects in line with HP 3b. Thus, these results align more with the "Commission as liberalization arbiter" mechanism. On the one hand, following European directives is now easier for firms of early liberalizers to operate in initially more resistant MSs. On the other hand, because of the initially lower restrictions, firms from laggard MSs find it easier to enter early reformers' industries than the reverse, explaining the larger pro-competition effects in the latter group. However, although these results support the mechanism mentioned above, they do so only partly. Indeed, full support requires showing that MSs with successful firms effectively pressured the Commission

to ensure liberalization in other countries. Investigating this behavior necessitates methodologies and data beyond the scope of this paper.

Finally, these findings show the effect of European directives as a function of a static initial restriction level. Yet, the baseline results of table 3 suggest that domestic reforms over time can have a strong and significant pro-competition effect when interacted with European directives. Therefore, laggard MSs may have improved their positioning provided the right amount of reform efforts. This seems plausible since Gutierrez and Philippon (2018) show that PMR has decreased faster in countries characterized by initially high restrictions.

4.3 Hypothesis 3

The lack of the Commission's formal power to force MSs to privatize their industries can be exploited to further test the importance of national reforms for European directives. Indeed, the highly diverse government-ownership levels across countries reveal various and inherently national approaches to privatization. As theorized by hypothesis 4a, privatization should increase competition by facilitating foreign entry. Yet, privatization without any elements of liberalization simply transforms a public into a private monopoly, with no effect on market power (hypothesis 4b).

To assess these hypotheses, I decompose ΔPMR into sub-indicators disentangling the economic effects of liberalizations and privatizations. As in Alesina et al. (2005), I define a variable capturing the intensity of domestic liberalization (Δlib) by averaging the entry barriers and vertical integration components of the PMR score. The extent of privatization ($\Delta priv$) is captured by considering only the public ownership component of the PMR score. Then, I run the following two regressions:

$$\log \mu_{jict} = eu_{it} \times \Delta lib_{ict} + eu_{it} + \Delta lib_{ict} + X_{jict} + \alpha_j + \tau_t + \epsilon_{it}, \tag{5}$$

$$\log \mu_{jict} = eu_{it} \times \Delta priv_{ict} + eu_{it} + \Delta priv_{ict} + X_{jict} + \alpha_j + \tau_t + \epsilon_{it}, \tag{6}$$

and I confront these results with ones previously obtained by running equation (2). At this stage, it is necessary to clarify the interpretation of the various interaction terms. The variable $eu \times \Delta priv$ combines the effects of European liberalization with domestic privatization. Instead, $eu \times \Delta lib$ accounts for the combined impact of liberalizations at the national and European levels. Finally, $eu \times \Delta PMR$ captures the joint impact of European directives with domestic pro-competition reforms combining both elements of privatization and liberalization. The first two columns of table 8 show the estimates of equations (5) and (6), while the last reproposes the result of equation (2) for comparison. The second column of table 8 shows that domestic privatization alone ($\Delta priv$) does not significantly impact market power. Without reforms that also promote entry, private investors can exploit the existing barriers to further their market power. Therefore, these results are consistent with hypothesis 4b.

Privatization has a negative and strongly significant effect on markups when it is combined with European directives. Yet, this effect is lower than the combination of "pure" liberalization (first column), which in turn is less than $eu \times \Delta PMR$ (third column). So, how can we interpret these results?

The inherently national and early efforts to privatize network industries have created a fertile ground for European directives. However, this effect was greater when governments have combined privatization with domestic liberalization. Thus, in line with the sequencing literature, this finding adds a layer of sophistication to hypothesis 4a, envisaging a positive synergy between privatization and liberalization.

Table 8 Privatization & Liberalization

	(1) Liberalization	(2) Privatization	(3) Baseline
eu	-0.000	-0.024	0.030
	(0.038)	(0.032)	(0.059)
Δlib	0.011		
	(0.017)		
$eu \times \Delta lib$	-0.026*		
	(0.015)		
$\Delta priv$		0.031	
		(0.019)	
$eu \times \Delta priv$		-0.021***	
		(0.008)	
ΔPMR			0.018
			(0.030)
$eu \times \Delta PMR$			-0.038***
			(0.011)
Firm Effects			
Year Effects			
Observations	2,959,344	2,959,246	2,959,344
R-squared	0.930	0.930	0.930

Note: *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1. Standard errors are clustered at the country-industry level. All the columns include baseline controls.

5 Conclusions

This paper has tried to shed more light on the economic impact of competition reforms where the European and domestic dimensions interact. In doing so, it has proposed a new framework where European and domestic institutions are mutually interdependent for increasing competition in network industries. On the one hand, the Commission needs governments' cooperation to adapt European directives to the domestic industrial and institutional setting. On the other, governments recur to the Commission to ensure reciprocity in liberalization and when they lack the political strength to carry on such reforms. I tested this proposition by showing that European and domestic reforms together significantly reduce market power, whereas they produce non-significant effects when considered separately.

Industry heterogeneity, initial barriers, and privatization have been used as further tests to corroborate the validity of the core hypothesis and disentangle the main mechanisms. The greater cooperation and information sharing in the telecom industry can explain why multilevel reforms were more effective than in the electricity sector. Instead, the importance of early reforms for the pro-competition effect of European directives seems to suggest the prevalence of the Commission's role as liberalization arbiter over as an ally (or alibi) to win domestic resistance. Finally, the capacity of privatization to magnify the effect of European

directives shows that national reforms – although heterogenous – were not simply the imposition of the Commission's will on MSs.

This paper makes three main contributions. Firstly, it shows that Commission's and MSs' interests are conflicting only apparently when it comes to competition. In this regard, this paper bridges the EU-centric and strategic liberalization views by providing a framework capable of reconciling domestic interests, heterogenous reforms, and European institutions. Both views are indeed correct in attributing different ambitions and goals to European authorities and governments; yet, what they miss is recognizing those interests as compatible for increasing competition.

Secondly, this paper contributes to the vibrant but limited empirical literature assessing the economic effects of liberalization in Europe (e.g., Alesina et al. 2005; Griffith et al. 2010; Gutierrez and Philippon 2018). Furthermore, in line with Besley et al. (2021), it also shows the importance of institutions and reforms in industries that are naturally less exposed to competition.

Thirdly, the analysis reveals that political dynamics may improve economic outcomes. This conclusion starkly departs from the literature claiming that optimal policies require the authorities' complete independence from politics (Kydland and Prescott 1977; Barro and Gordon 1983; Rogoff 1985). In my framework, the Commission's independence serves as an essential counterbalance to potentially distorting national interests and to ensure a level-playing field between MSs. Yet, MSs' role does not end with an act of full delegation, as in Gutierrez and Philippon (2018). By contrast, the involvement of governments allows agreements to reflect institutional constraints and industrial characteristics, which one-size-fits-all policies would overlook. Furthermore, these policies would likely produce undesired effects in a European economy still characterized by significant domestic heterogeneity.

This study, however, comes with a significant limitation. The reciprocal liberalization and external ally mechanisms have been investigated only indirectly according to their conjectured economic implications. Yet, those mechanisms also have a strong political component. Namely, the pressures exerted by certain governments through the Commission to open industries in other MSs and the use of the European dimension as an external constraint to impose liberalization domestically. Investigating these political dynamics claims for a richer theoretical model explaining the design and content of agreements between the Commission and governments and how these depend on the domestic political-economic setting. Furthermore, such a model should demonstrate how cooperative outcomes can be sustained as a part of equilibrium strategies and evolve over time. Indeed, given the evolving economic and political context concerning network industries, it is unlikely that agreements result from a static single-shot game between the Commission and MSs.

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Appendix A Markups Estimation

A.1 Theory

This section follows the theoretical framework to estimate markups proposed by De Loecker and Warzinsky (2012) and De Loecker et al. (2020). In each period t, firms minimize the cost function subject to its output constraint:

$$C_{it} = P_{it}^{V} V^{it} + r_{it} K_{it} + F_{it} \tag{A.1}$$

$$\bar{Q}_{it} = Q(\Omega_{it}, V_{it}, K_{it}) \tag{A.2}$$

$V = (V^1, ..., V^N)$ is the set of variable inputs of production (labor, intermediate inputs, materials, and so on), K_{it} is the capital stock, and Ω_{it} is the Hicks-neutral productivity which is firm-specific. $P_{it}^{V} = (P^1, ..., P^N)$ is the price vector of variable inputs, so $P_{it}^{j} \in P_{it}^{V}$ indicates the price of the variable input j. The term r_{it} is the user cost of capital, and F_{it} is the fixed cost. This approach requires that the variable input of interest adjusts without frictions over one period (i.e., a year), whereas capital is subject to adjustment costs and other frictions.

The associated Lagrangian of the problem is:

$$L(V_{it}, K_{it}, \Lambda_{it}) = F_{it} + P_{it}^{V} V_{it} + r_{it} K_{it} - \lambda_{it}(\bar{Q} - Q_{it}(\cdot)), \tag{A.3}$$

Where λ is the Lagrange multiplier and $\bar{Q}$ is a scalar indicating the target production level. The first-order condition for a generic variable input $V_{it}^j \in V_{it}$ is:

$$\frac{\partial L(.)}{\partial V_{it}^j} = 0 \Rightarrow P_{it}^j = \lambda_{it} \frac{\partial Q(.)}{\partial V_{it}^j}. \tag{A.4}$$

By multiplying and dividing for $\frac{V_{it}^j}{Q_{it}}$ I obtain the elasticity of output to the generic variable input V_{it}^j:

$$\theta_{it}^j \equiv \frac{\partial Q(.)}{\partial V_{it}^j} \frac{V_{it}^j}{Q_{it}} = \frac{P_{it}^j}{\lambda_{it}} \frac{V_{it}^j}{Q_{it}} \tag{A.5}$$

The Lagrange multiplier denotes how the minimum cost varies, if we vary marginally output. In other words, it is simply the marginal cost. Thus, $\mu_{it} = \frac{P_{it}}{\lambda_{it}}$, with P_{it} being the price charged by the firm. By substituting the latter expression into (A.5), I get:

$$\mu_{it} = \theta_{it}^j \frac{P_{it} Q_{it}}{P_{it}^j V_{it}^j}. \tag{A.6}$$

Note that the term $\frac{P_{it} Q_{it}}{P_{it}^j V_{it}^j}$ is simply the inverse of the share of input j's cost in total revenues, which I denote as α_{it}^j. Therefore, (A.6) becomes:

$$\mu_{it} = \frac{\theta_{it}^j}{\alpha_{it}^j}. \tag{A.7}$$

Expression (A.7) implies that to recover markups is enough to focus only on one variable input. I have chosen materials for most countries since it is easier for firms to adjust this input between subsequent periods. However, I have used a variable input bundle for Danish, Greek, Irish, and British businesses since, in Orbis Historical, very few of them report materials expenditure. This bundle is recovered from the cost of goods sold, which includes all variable production costs, without separating the various components. In practice, the only thing that changes is that the vector V is treated as a scalar (De Loecker et al. 2020). Finally, the cost-share α_{it}^j is easily recoverable from balance sheet data, while θ_{it}^V requires the estimation of an industry production function.

A.2 Estimation Procedure

This section follows the production function estimation procedure of Levinsohn and Petrin (2003), and its adaptation to the markups case by De Loecker and Warzynski (2012) and De Loecker et al. (2016, 2020). I thus invite readers to consult these papers for more details.

Consider the following (gross) log Cobb-Douglas production function:

$$y_{it} = \beta_0 + \beta_l l_{it} + \beta_m m_{it} + \beta_k k_{it} + \omega_{it} + \epsilon_{it}, \tag{A.8}$$

where l is labor, m materials, and k capital, while ω_{it} is the firm's productivity. This term is unobserved to the researcher but known by the firm. To obtain y, l, m, and k, I have deflated operating revenues, cost of employees, material costs, and tangible fixed assets using the OECD GDP deflator, and I have subsequently taken the log. When estimating markups for Denmark, Greece, Ireland, and the UK, equation (A.8) include the term v_{it} in place of l_{it} and m_{it}. This term has been obtained by deflating the cost of goods sold and taking the log. Apart from this change, the estimation procedure is identical.

The production function has been defined at the NACE 2-digit industry level.[13] Therefore, the various coefficient denotes the different industry elasticities associated with the related inputs. In my estimation

[13] I have considered all the NACE 2-digit apart from those defining public sector administration (84) and extraterritorial activities (99).

routine, I consider time-invariant betas as in De Loecker and Warzynski (2012) and De Loecker et al. (2016). Time-varying beta would require estimating the production function for each industry-year group separately. However, doing so leads to a tiny estimation sample for particular years and industries.

A crucial assumption is that the generic variable input demand is a function of the state variable (capital), productivity, and other market factors $\mathbf{z_{it}}$.[14] Following De Loecker et al. (2012), I have used m_{it} (v_{it} for the countries specified above):

$$m_{it} = m(\omega_{it}, k_{it}, \mathbf{z_{it}}). \tag{A.9}$$

If the function m is invertible, then we can express the unobserved firm productivity as:

$$\omega_{it} = h(m_{it}, k_{it}, \mathbf{z_{it}}). \tag{A.10}$$

This approach takes the name of the *control function* technique. This technique allows me to obtain a proxy of ω_{it} to include in our estimation. Otherwise, ignoring productivity will lead to biased estimates since it creates a correlation between the regressors and the error term. The procedure is divided into two stages.

A.2.1 First Stage

I define the function ϕ as:

$$\phi_{it}(l_{it}, m_{it}, k_{it}, \mathbf{z_{it}}) = \beta_0 + \beta_l l_{it} + \beta_m m_{it} + \beta_k k_{it} + h(m_{it}, k_{it}, \mathbf{z_{it}}) \tag{A.11}$$

By substituting (A.11) into (A.8) we get:

$$y_{it} = \phi_{it}(v_{it}, k_{it}, \mathbf{z_{it}}) + \epsilon_{it}. \tag{A.12}$$

Then, I regress y_{it} on a third order polynomial expansion of $\phi_{it}(v_{it}, k_{it}, z_{it})$ in all its terms as in De Loecker et and Warzynski (2012), and store $\hat{\epsilon}_{it}$ and $\hat{\phi}_{it}$.

A.2.2 Second Stage

Productivity is assumed to follow a a Gauss-Markov process of order 1:

$$\omega_{it} = g(\omega_{it-1}) + \xi_{it}. \tag{A.13}$$

In order to obtain a preliminary estimate of ω_{it}, I compute the coefficients' starting values by running an OLS regression with time dummies as follows:

$$y_{it} = \beta_0 + \beta_l l_{it} + \beta_m m_{it} + \beta_k k_{it} + \tau_t + \epsilon_{it}, \tag{A.14}$$

The estimated coefficients are the starting values β_i^*. A Similar approach has been used by Levinsohn and Petrin (2003) (see appendix), and De Loecker and Warzynski (2012) (see replication file), and Ackerberg et al. (2015) (see appendix).

Now it is possible to construct a preliminary estimate of productivity, that is $\hat{\omega}_{it}$ using (A.11):

$$\hat{\omega}_{it} = \hat{\phi}_{it} - \beta_m^* m_{it} - \beta_l^* l_{it} - \beta_k^* k_{it} - \beta_0^*. \tag{A.15}$$

The term $\hat{\omega}_{it-1}$ is defined in the same fashion but using lagged values.

[14] As in De Loecker and Warzynski (2012), I include year indicators and firms' market shares.

The assumption about productivity evolution implies:

$$\hat{\omega}_{it} = \rho\hat{\omega}_{it-1} + \epsilon_{it}. \tag{A.16}$$

Then I store the predicted values from (A.16) $\hat{E}_{it}$. By substituting (A.13) into (A.8) we obtain:

$$y_{it} = \beta_0 + \beta_l l_{it} + \beta_m m_{it} + \beta_k k_{it} + g(\omega_{it-1}) + \xi_{it} + \epsilon_{it} \tag{A.17}$$

I define the following moment condition using (A.17) :

$$E[(\xi_{it} + \epsilon_{it}) \begin{pmatrix} l_{it-1} \\ m_{it-1} \\ k_{it} \end{pmatrix}] = 0. \tag{A.18}$$

The term $\xi_{it} + \epsilon_{it}$ is constructed as follows:

$$(\hat{\xi_{it}} + \epsilon_{it}) = y_{it} - (\beta_0 + \beta_l l_{it} + \beta_m m_{it} + \beta_k k_{it} + \hat{E}_{it}), \tag{A.19}$$

where $\hat{E}_{it}$ substitutes for $g(\omega_{it-1})$ since:

$$\hat{E}_{it} = E(\omega_{it}|\omega_{it-1}) = g(\omega_{it-1}). \tag{A.20}$$

Now it is possible to recover the parameters of interest using a Generalized Method of Moments (GMM) estimation. Since I am using a log Cobb-Douglas function, the coefficient β_m denotes the output elasticity to materials. Following De Loecker and Warzisky (2012) and De Loecker et al. (2020), I implicitly allow for measurement errors in output and unobserved shocks to the production function, which are combined in ϵ_{it}, precisely:

$$y_{it} = logQ_{it} + \epsilon_{it} \Rightarrow Y_{it} = Q_{it}e^{\epsilon_{it}}, \tag{A.21}$$

where Q_{it} and Y_{it} are real and observed output in levels, respectively. So, I can correct observed revenues R_{it} using $\hat{\epsilon}$. That is, $R_{it}^{corr} = \frac{R_{it}}{\hat{\epsilon}_i t}$. Now it is possible to retrieve markups:

$$\mu_{it} = \theta^m \frac{R_{it}^{corr}}{material\ costs_{it}}. \tag{A.22}$$

Finally, I obtain an estimate of firm level productivity as follows:

$$\hat{\phi}_{it} - \hat{\beta}_0 - \hat{\beta}_l l_{it} - \hat{\beta}_m m_{it} - \hat{\beta}_k k_{it} \tag{A.23}$$

Appendix B Robustness Checks

In this section, I perform various robustness checks to ensure the soundness of the paper's core hypothesis.

B.1 Dataset

I trim the top and bottom 5% in the baseline analysis to avoid outliers. Now, I re-run (2) trimming at the 4%, 3%, 2%, and 1%.

Table B.1 Different Levels of Trimming

	(1) 4%	(2) 3%	(3) 2%	(4) 1%
eu	0.020	0.039	0.024	0.050
	(0.059)	(0.059)	(0.056)	(0.064)
ΔPMR	0.028	0.033	0.046*	0.057*
	(0.030)	(0.026)	(0.027)	(0.032)
$eu \times \Delta PMR$	-0.033***	-0.031**	-0.029**	-0.030*
	(0.012)	(0.015)	(0.014)	(0.018)
Firm Effects	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes
Observations	3,090,052	3,227,394	3,380,247	3,557,545
R-squared	0.930	0.931	0.931	0.929

Note: *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1. Standard errors are clustered at the country-industry level. All the columns include baseline controls.

As it is possible to see from (B.1), $eu \times \Delta PMR$ has a negative and significant effect in every different specification.

A possible source of bias concerning the data is that firms "self-select" in the treatment and control groups. For instance, a firm can change industries to avoid liberalization. Although it seems unlikely that large firms, such as EDF or EON, would radically change their business to avoid these reforms, I re-run (2) by excluding firms that have changed their NACE 2-digit classification over the years.

Table B.2 Excluding Moving Firms

eu	0.030
	(0.052)
ΔPMR	0.018
	(0.026)
$eu \times \Delta PMR$	-0.038***
	(0.009)
Firm Effects	Yes
Year Effects	Yes
Observations	2,955,310
R-squared	0.930

Note: *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1. Standard errors are clustered at the country-industry level. Baseline controls are included.

Again, as shown by table *B.2*, the core results remain unchanged.

B.2 Control Group Robustness

A possible critique of my empirical strategy concerns the differences between the treatment and the control group. Arguably, firms in liberalized industries are inherently different from manufacturing businesses. I address this critique in three ways: propensity score weighting, matching, and a diverse control group composed of firms in the same KLEMS segment.

Propensity score weighting is a technique to address selection bias when randomization is not feasible (Rosenbaum and Rubin 1983). The first step is estimating the probability (i.e., the propensity score) that a specific unit in the sample will receive the treatment conditional on covariates. In my case, I have estimated the probability that a firm will be liberalized through a European directive, using the log of revenues, productivity, and unit variable cost as control, in addition to country and year effects. These variables are likely to correlate with the treatment since the leitmotiv of liberalization was to increase efficiency in state-owned industries characterized by low productivity (Buch-Hansen and Wigger 2011). Then, I define weights based on the inverse of the estimated probability and reweight all the observations in the sample accordingly. The intuition is that reweighting the data by the propensity creates a synthetic control group with characteristics analogous to treated units (Acemoglu et al. 2019).

Similarly, the idea of matching is creating a control group as similar as possible to the treatment group. Control units are matched with treated firms according to the similarity of the propensity score.[15] The difference with inverse propensity score weighting is that units that are not similar enough are discarded, reducing the regression sample significantly. For this reason, weighting is generally preferred over matching.

[15] I have performed the matching using a caliper of 0.25.

However, I run equation (2) on the matched sample as well for completeness.

Finally, I have created a new control group with firms belonging to the same KLEMS industry segment of liberalized industries. Thus, firms within the same segment are expected to have more comparable characteristics, thereby serving as a better control group for liberalized industries.

Table B.3 shows the results of this robustness exercise. The interaction of European directives with national reforms is negative and strongly significant in all three specifications. These findings further corroborate the core thesis of this paper by showing that its validity does not depend on inherent differences between treatment and control units.

Table B.3 Alternative Control Groups

	(1) Propensity Score Weighting	(2) Matched Sample	(3) Same KLEMS
eu	0.097	0.092	0.046
	(0.074)	(0.062)	(0.051)
ΔPMR	0.063	0.048	0.022
	(0.049)	(0.033)	(0.033)
$eu \times \Delta PMR$	-0.049***	-0.039***	-0.038***
	(0.016)	(0.010)	(0.014)
Firm Effects	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes
Observations	2,959,344	11,120	63,987
R-squared	0.933	0.936	0.911

Note: *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1. Baseline controls are included. Standard errors are clustered at the country-industry level.

B.3 Alternative Variables

As a further robustness check to test the importance of the interaction between domestic and European reforms, I adopt the following specification:

$$\log \mu_{jict} = \sum_{v=1}^{4} eu_{it} \times \Delta Q_t^v + X_{jict} + \alpha_j + \tau_t + \epsilon_{it}, \tag{B.1}$$

where I interact the treatment variable with an indicator ΔQ_t^v, denoting the quartile of the ΔPMR distribution by year, following the DID specification proposed in Prager and Schmitt (2021). Even in this specification, the effect of European legislation is amplified by domestic reforms. However, this effect is not perfectly monotonic since the of $eu_{it} \times \Delta Q_t^4$ is lower than $eu_{it} \times \Delta Q_t^3$.

Table B.4 Interaction by quartiles of ΔPMR

$eu \times \Delta Q^1$	-0.012
	(0.052)
$eu \times \Delta Q^2$	-0.111**
	(0.045)
$eu \times \Delta Q^3$	-0.125***
	(0.045)
$eu \times \Delta Q^4$	-0.098**
	(0.049)
Firm Effects	Yes
Year Effects	Yes
Observations	2,959,344
R-squared	0.930

Note: *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1. Standard errors are clustered at the country-industry level. Baseline controls are included.

A possible concern is that the evolution of markups depends on ex-ante competition conditions. Precisely, particularly high initial markups in network industries may have magnified the extent of these reforms. Thus, I control for this factor by including three lags of μ as additional controls. The inclusion of different lags does not alter the significance of $eu \times \Delta PMR$. By contrast, the magnitude of this coefficient has increased (B.5)

Table B.5 Controlling for lags of μ

eu	0.102
	(0.124)
ΔPMR	0.048
	(0.030)
$eu \times \Delta PMR$	-0.048**
	(0.023)
Firm Effects	
Year Effects	
Observations	1,134,169
R-squared	0.953

Note: *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1. Standard errors are clustered at the country-industry level. Three lags of μ in addition to baseline controls are included.

Finally, I repeat the comparison of the electricity and telecom industry, including capital intensity, to account for the typical higher investments and fixed costs characterizing the former industry. Although coefficients are less significant when the control group is the entire economy, the coefficients' magnitude remains stable (table B.6).

Table B.6 Electricity vs. Telecom with Capital Intensity

	Entire Sample		Same Klems/Sector	
	Electricity	Telecom	Electricity	Telecom
eu	-0.046	0.211	-0.000	0.322**
	(0.104)	(0.175)	(0.082)	(0.160)
PMR	0.037	0.059	-0.006	0.050
	(0.043)	(0.053)	(0.034)	(0.050)
$eu \times PMR$	-0.048*	-0.078*	-0.055***	-0.089**
	(0.026)	(0.040)	(0.020)	(0.037)
Firm Effects	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes
Observations	2,953,393	2,953,675	19,338	55,891
R-squared	0.931	0.931	0.924	0.904

Note: *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1. Standard errors are clustered at the country level. Capital intensity is included in additional to baseline controls are included

B.4 Alternative Methodologies

Callaway and Sant'Anna (2020) show that two-way fixed effects in staggered DID estimations could produce biased results. I thus control for this potential bias by adapting their methodology to my specific case. In so doing, I made the following adjustments. Firstly, the treatment variable is an interaction between eu and an indicator denoting whether ΔPMR is above the median in a given year, $Top \times \Delta PMR$. Unfortunately, repeating specification ($B.1$) is impossible since their package in R does not permit obtaining more than one treatment parameter coefficient. Moreover, I have chosen the median because a higher threshold implies that too few observations are included in the treatment group to precisely estimate the coefficient. Secondly, as Callaway and Sant'Anna (2020) recommend, I use a balanced panel dataset. Therefore, this is a valuable way to test that results do not depend on the unbalanced nature of the data. However, using a balanced dataset implies more than halving the number of observations. Therefore, I consider a restricted number of years (1997-2007) to limit this loss, and I cluster standard errors at the country level. I have made this choice because their methodology does not allow units to move between clusters. So, the use of country-industry or industry clusters will further reduce observations.

Callaway and Sant'Anna's (2020) methodology allows two methodological choices regarding the control group. Firstly, using units that are never treated. Secondly, units that will be treated in the future. That is, firms in industries that have not been liberalized yet, but that will be so at a later stage. I use both control group specifications for completeness. Finally, their methodology allows for three types of DID estimands: outcome regression, inverse probability weighting, and doubly robust methods. The latter is a method that combines the first two with the advantage that it relies on less strict modeling assumptions (Callaway and Sant'Anna (2020).

Table B.7 Callaway and Sant'Anna (2020) Methodology

	Never Treated		
	Doubly Robust	Outcome Regression	Inverse Probability Weighting
$eu \times Top\Delta PMR$	-0.186***	-0.237***	-0.185***
	(0.038)	(0.025)	(0.040)
	Not Yet Treated		
$eu \times Top\Delta PMR$	-0.186***	-0.237***	-0.185***
	(0.038)	(0.025)	(0.040)
Observations	1,189,848	1,189,848	1,189,848

Note: *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1. Standard errors are clustered at the country level. Baseline controls are included

As shown in table, B.7 the interaction $eu \times Top\Delta PMR$ has a negative and strongly significant effect in every specification. Concerning the estimates of the main specification (2), the coefficients are relatively larger in magnitude. This, however, might be due to the different years considered and the significantly lower number of firms.

I conclude this section by addressing potential endogeneity concerns related to the regressors used in model (2). Building on Acemoglu et al. (2019), I run two GMM estimations of model (2). In the first, I instrument baseline firm-level controls by their lagged values (table B.8, first column). In the second, I instrument all the variables with their lags (B.8, second column). As illustrated in table (B.8), the main thrust of the results is not changed by this alternative methodology.

Table B.8 GMM Estimation

	(1)	(2)
eu	0.047	0.083
	(0.055)	(0.089)
ΔPMR	-0.001	0.053
	(0.024)	(0.041)
$eu \times \Delta PMR$	-0.044***	-0.086***
	(0.011)	(0.018)
Firm Effects	Yes	Yes
Year Effects	Yes	Yes
Observations	1,777,841	1,777,841
R-squared	0.231	0.231

Note: *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1. Standard errors are clustered at the country level. Baseline controls are included

B.5 Additional Placebo Tests

As a further placebo test, I run an outcome test, using the lag of log markups as a dependent variable. The logic of this test is that the treatment should not affect lagged outcomes (Eggers et al. 2021). Indeed, as we can see from table (B.9), this is not the case.

Table B.9 Outcome Placebo Test

eu	-0.025
	(0.072)
ΔPMR	0.014
	(0.026)
$eu \times \Delta PMR$	-0.021
	(0.020)
Firm Effects	Yes
Year Effects	Yes
Observations	2,195,503
R-squared	0.897

Note: *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1. Standard errors are clustered at the country level. Baseline controls are included

I run the last placebo test in the spirit of Bertrand et al. (2004). These authors argue that in DID studies with several pre and post-intervention periods, standard errors are biased downwards, amplifying the significance of results. They show the gravity of this problem by running several Montecarlo simulations with fake treatment, finding that results are significant in 45% of cases. I adopt a similar approach by creating a fake eu and ΔPMR. For the first variable, I extract a year using a discrete uniform distribution over the period 1996-2013, excluding both the year of the actual liberalization and 1995, the first year in the sample; otherwise, there would be no pre-treatment period. For ΔPMR, I extract a random number using a continuous uniform distribution in the interval $[0, 6]$, the range of values that ΔPMR can assume. I then run model (2) 1000 times and store the p-value of $eu \times \Delta PMR$. It turns out that the coefficient of $eu \times \Delta PMR$ is significant only in 4.1% of cases. This result is reassuring since a fake treatment should produce a significant effect at 5% more or less in 5% of the simulations (Bertrand et al. (2004). Finally, figure B.1 plots the p-value density function of these simulations, where the red line denotes 0.05.

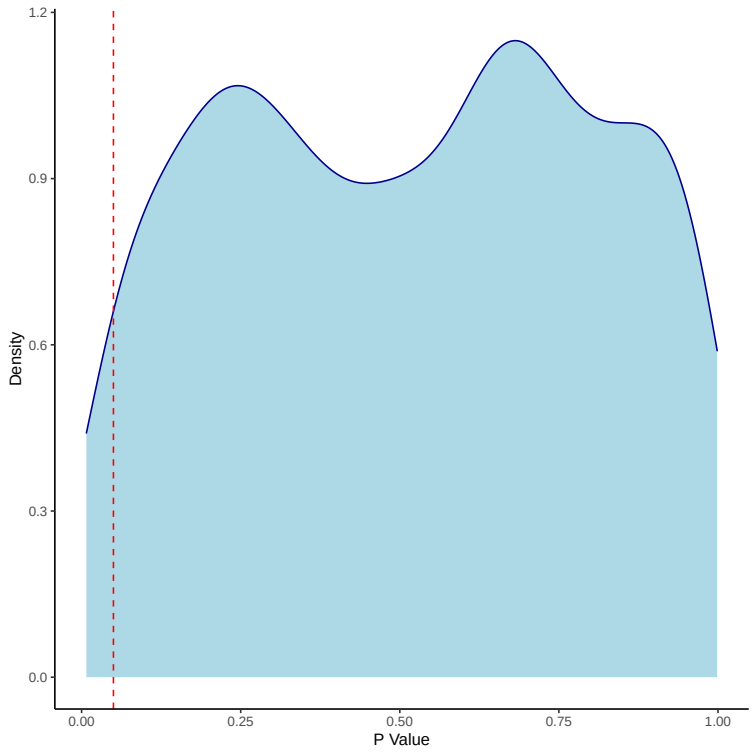

Figure B.1 P-value density of placebo regressions