Journal of Comparative Economics 48 (2020) 198–217

Contents lists available at ScienceDirect

Journal of Comparative Economics

journal homepage: www.elsevier.com/locate/jce

How fast does product market reform pay off? New evidence from non-manufacturing industry deregulation in advanced economies

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ARTICLE INFO

JEL classification:
L51
O43
O47

Keywords:
Deregulation
Competition
Entry barriers
Product market
Growth

ABSTRACT

The paper sheds light on the issue of how fast product market deregulation pays off and whether it entails any transitory costs by using a unique mapping between a new narrative dataset of major reform events and sector-level outcomes for 5 major non-manufacturing industries in 26 advanced economies over the period 1975–2011. Using a local projection method, we find that deregulation pays off only gradually: its positive effect on output becomes statistically significant three to four years after the reform, consistent with the notion that entry takes time due to real frictions. At the same time, there is no evidence of any significant transitory cost. Over the medium term, these product market reforms eventually yield large, highly significant increases in output, concomitant with a relative price decline. The typical major historical reform in advanced economies increased real value added and employment in the deregulated industry by about 10 percent and over 5 percent, respectively, and lowered relative prices by some 8 to 10 percent, after five years.

1. Introduction

Amid permanent output losses from the global financial crisis and a protracted decline in potential growth, structural reforms have been increasingly advocated by policymakers and institutions alike as a way to revive growth in advanced economies (e.g. Draghi, 2015; IMF, 2015, 2016; OECD, 2018). Product market reforms feature high on this agenda; even though widespread deregulation has already taken place across advanced economies in recent decades, there remains much scope for easing barriers to entry in retail trade, professional services, and some network industries in a number of advanced countries (Koske et al., 2015). In the United States, the need for, and potential gains from a new wave of product market deregulation and across-the-board cuts in red tape has been the subject of much debate and disagreement lately (see, for example, Cochrane, 2016; Davis, 2017; DeLong, 2016).

While the existence of long-term gains from easing barriers to entry in product markets has gradually become consensual since the seminal paper of Blanchard and Giavazzi (2003) (e.g. Ebell and Haefke, 2009; Fang and Rogerson, 2011; Felbermayr and Prat, 2011), their short- to medium-term impact remains debated by policymakers and academics alike, and it is largely unknown empirically. On the one hand, policymakers and international institutions have been increasingly advocated such reforms to speed up economic recoveries, notably in advanced economies in the aftermath of the global financial and euro crises of 2008–09 and 2010–12 (e.g. Draghi, 2015; OECD, 2018); indeed, this view motivated the inclusion of product market reforms in the individual macroeconomic

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[1] The authors would like to thank the editor, two anonymous referees, participants to the 2017 ASSA Meetings, IMF and OECD seminars, and the KDI-World Bank workshop on Economic Assessment of Regulatory Reform, for useful comments on an earlier version of the paper. The views expressed in this paper do not necessarily reflect those of the International Monetary Fund or its member countries. All errors are ours.

https://doi.org/10.1016/j.jce.2019.09.006
Received 15 November 2018; Received in revised form 25 July 2019; Accepted 24 September 2019
Available online 03 October 2019
0147-5967/

adjustment programs of Southern European countries during the euro area crisis of the early 2010s. On the other hand, other scholars have pointed out that labor and product market reforms are in fact likely to pay out only very slowly (Rodrik, 2015) or may even entail short-run losses (e.g. Krugman, 2014). Addressing this question matters not only for the economics, but also for the political economy of reform; any short-term employment and output losses in deregulated industries would add one item to the long list of obstacles to reform identified in the literature (see e.g. Tommasi and Velasco, 1996). Against this background, this paper aims to explore empirically the short-to-medium-term of product market deregulation.

The benchmark framework to analyze the dynamic effects of reductions in entry costs arguably remains that proposed by Blanchard and Giavazzi (2003). In their static set-up, reductions in entry barriers of the type we study here increase output, employment, and real wages only in the long term, once new firms enter the market in response to the initial increase in expected profits. In the short term there is no impact, per an assumption that the number of firms is initially fixed. Some of the more recent theoretical papers incorporate entry barriers in a fully dynamic general equilibrium set-up, with endogenous variation in the number of monopolistically competitive firms as in Bilbiie et al. (2012). In these models, still in line with Blanchard and Giavazzi (2003), a reduction in entry costs triggers firm entry and gradually increases output, employment, and real wages; however, it entails short-term losses, partly reflecting the downsizing of incumbents and the need to finance entry (Cacciatore and Fiori, 2015; Cacciatore et al., 2016a,b). This strand of the theoretical literature assumes that firms maximize profits under monopolistic competition. Market structure and firm behavior need not systematically conform with this mainstream assumption in practice, due e.g. to oligopolistic structures and associated strategic behavior (e.g. Aghion et al., 2005), non-profit objectives (e.g. Bertrand and Mullainathan, 2003) or the presence of X-inefficiency (Leibenstein, 1966). Some of these features could have implications for the short-term effects of deregulation, although these have not been studied to our knowledge. For example, relative to the aforementioned models, competition-increasing reductions in entry barriers are more likely to lead incumbents to shed labor and increase output and productivity in the presence of X-inefficiency and non-profit maximization prior to the reform—as might be expected, for example, from large state-owned enterprises in some sheltered industries. Therefore, empirical analysis is needed.

The present paper quantifies empirically the short- to medium-term impact of abrupt reductions in barriers to entry on sector-level output, employment, prices, investment, and productivity in the deregulated industries. The focus is on air transport, electricity and gas, rail and road transport, postal and courier activities, and telecommunications. These five large non-manufacturing industries provide key inputs to the rest of the economy (Bourlès et al., 2013), account for a sizeable share (over 12 percent on average across our sample) of total value added in the non-manufacturing business sector, and for an even bigger fraction of existing product market regulations in developed countries (e.g. Koske et al., 2015). Crucially for identification purposes, over our sample period, these industries have also undergone major deregulation that varied in timing and intensity across countries. In telecommunications, for example, the United States and the United Kingdom were early movers in the early 1980s, as part of the Reagan and Thatcher governments' supply-side agendas. Another advantage of focusing on industry-level regulations is to enable us to build an empirical strategy that carefully addresses key sources of omitted variable bias, through a rich set of fixed effects that absorbs confounding country-level factors such as macroeconomic conditions. While the market structure of network industries features a few specificities—such as usually large fixed costs and a natural monopoly element in their upstream, non-competitive part—their economic and regulatory weight means that analyzing them can carry more general lessons regarding the short-term effects from removing barriers to entry in product markets.

A further reason for focusing on these five network industries is that, for each of them, we are able to uniquely map reform and outcome indicators thanks to a new reform dataset. Abrupt reductions in barriers to entry are identified based on a new narrative database of major reform events that compiles all large regulatory and legislative measures taken in each sector for 26 countries starting from the 1970s (Duval et al., 2018). There are two major advantages to this new dataset in the context of the present paper, over and above its broader country and time-series coverage: i) it covers major deregulation events whose scope goes beyond that of the regulatory indicators published by the OECD—for example, the breakup of U.S. telecommunications monopoly AT&T in 1982 following an anti-trust lawsuit filed by the Department of Justice; ii) perhaps most importantly, it captures precisely the timing of reforms, which in some cases differs by one to two years from what would be inferred from large changes in OECD indicators of the regulatory stance (see Duval et al., 2018). For outcomes, we rely on the recent (2014) publication by the OECD of a more disaggregated version of its sectoral database STAN, which is now available at the ISIC Rev.4 level.

The unique mapping between reform events and sector-level outcomes yields a country-sector-time panel dataset that we then use to estimate the dynamic impact of major reform events over the five years from their implementation date. To this end, we rely on the local projection method (Jordà, 2005; Teulings and Zubanov, 2014), which has been used recently to study the dynamic impact of macroeconomic shocks such as financial crises (Romer and Romer, 2017) or fiscal shocks (Jordà and Taylor, 2013). We address carefully possible sources of omitted variable bias in two ways: first, through a rich set of fixed effects, most importantly controls for unobserved economy-wide (country-time) shocks that could potentially correlate with both reforms and outcomes; second, through IV techniques, using as external instruments for reform shocks the occurrence of reforms in the same sector in other countries and the initial regulatory stance.

Our empirical analysis yields two main findings. First, major reductions in barriers to entry deliver only gradually: output effects become statistically significant three to four years after the reform, as prices start dropping; somewhat surprisingly, however, there is no evidence of any transitory cost, apart from a short-lived drop in employment during the year of the reform that quickly turns into a rise—suggesting that the contraction or exit of some incumbent firms is quickly offset by the expansion and entry of others. Second, reductions in barriers to entry eventually yield large increases in output over the medium term, concomitant with a relative price decline; the typical major historical reform in advanced economies increased real value added in the deregulated industry by about 10 percent after five years.

The paper relates to the empirical literature on the economic impact of product market deregulation, its key contribution being to explore the short-term effects of reform in deregulated industries. A strand of studies using country-time or country-time-industry panel data documented a significant positive long-run effect of product market reform on productivity, investment, employment and/or output (e.g. Aghion et al., 2009a; Alesina et al., 2005; Bassanini and Duval, 2009; Conway et al., 2006; Fiori et al., 2012; Inklaar et al., 2008; Nicoletti and Scarpetta, 2003). These studies rely on aggregate indicators of the overall regulatory stance in the network industries we analyze in the present paper. However, they typically do not control for country-specific macroeconomic conditions and are subject to omitted variable concerns more broadly. Some of the more recent studies exploit country-industry level data, and seek to partly address omitted variable bias by estimating the indirect impact of product market reform in non-manufacturing (network) industries on outcomes in other industries that use non-manufacturing goods and services as inputs. Their identification strategy draws on Rajan and Zingales (1998) and assumes that deregulation in "upstream" (network) industries should benefit disproportionately those "downstream" industries that make greater use of inputs from the deregulated upstream industries. They find supportive evidence, which could reflect the impact of deregulation in non-manufacturing industries on input variety and quality, or innovation rents, in other industries (Barone and Cingano, 2011; Bourlès et al., 2013). Duval and Furceri (2018) also document positive short-term spillovers from deregulation to upstream industries through increased input demand from the deregulated industries. Our paper explores the *direct* impact of reforms on those deregulated non-manufacturing (network) industries themselves, and it focuses on the short term to shed light on the under-researched issues of how fast reforms pay off and whether they entail any transitory costs. Country-specific studies of deregulation in specific industries have documented positive long-run output and efficiency effects (e.g. from deregulation of network industries in the United States, see Winston, 1998), employment gains (e.g. in retail trade, see Bertrand and Kramarz, 2002; Skuterud, 2005), and favorable price impacts across deregulated industries (e.g. Winston, 1998), or in specific ones (e.g. road transport, see Combes and Lafourcade, 2005), during the 1980s and 1990s. While less studied, wages have generally been found to decline little, possibly because deregulation merely shrunk the wage premium of unionized workers vis-à-vis their non-unionized counterparts (Card, 1996; Hendricks, 1994). The paper also bears some connection to the firm-level literature on the impact of competitive pressures on innovation, technology adoption, and productivity (e.g. Aghion et al., 2004, 2005; 2009b; Arnold et al., 2016; Gal and Hijzen, 2016). Our focus here is on the dynamic effects of major reductions in barriers to entry for the deregulated sector as a whole, with the view to assessing the dynamic macroeconomic impact of these reforms. Finally, and perhaps most importantly, existing studies typically focus on the long-term impact of reform. They do not explore the short-term versus medium-term responses of outcomes to deregulation—a key focus of our paper, helped by the combination of a new narrative dataset of product market reforms with a local projection method.

The remainder of this paper is structured as follows. Section 2 describes the dataset and the methodology used for the construction of major reform events and provides stylized facts on the links between past reforms in the five non-manufacturing industries considered and the dynamics of output, prices, employment, investment and productivity. Section 3 describes the empirical setup. Section 4 presents the econometric results. Section 5 concludes.

2. Data, reform identification, and stylized facts

This paper relies on a new dataset matching product market regulation and sector-level data on output and inputs for five network industries: electricity and gas, land (rail and road) transport, air transport, postal and courier services, and telecommunications. Sector-level output and inputs are taken from the most recent version of the OECD Structural Analysis (STAN) database. Major product market reforms are drawn from a new database on major reform events in each of these industries for 26 countries over the past four decades assembled by Duval et al. (2018). Major reforms are identified primarily by examining documented legislative and regulatory actions reported in all available issues of the *OECD Economic Surveys*, combined with information from OECD indicators of regulation in energy, transport and communications, following a "narrative approach" used for example to identify monetary and fiscal shocks and periods of high financial distress by Romer and Romer (2004, 2010, 2015).

This section elaborates further on the data sources and the identification of the major reform events before presenting some descriptive statistics.

2.1. Data sources and identification of major reform events[2]

2.1.1. STAN database

The OECD Structural Analysis (STAN) database provides annual information on sector-level inputs, output, and prices (value-added deflators and labor cost) across OECD countries. The dataset spans the period 1970–2011, with a coverage varying widely across countries, years and sectors.[3] For our analysis, we collect information on real value added, prices, employment, real wages (the ratio of labor costs to total employment, deflated by the value-added price of the sector considered), gross fixed capital formation, and labor productivity—simply defined as the ratio of real value added to total employment.[4,5] In the econometric analysis, all

[2] For a summary, see Table A.1 in the appendix.

[3] Coverage is poor in some cases (e.g. Greece, Ireland, Japan, Korea, New Zealand, Switzerland, United Kingdom). For Australia, series starting from the early-mid 1990s are sourced from Labor Force and Australian System of National Accounts (unpublished), with the help of the Australian Bureau of Statistics whose support we gratefully acknowledge. Finally, while data coverage for Central and Eastern European countries is reasonably good, it typically starts after the transition period in the mid-1990s.

variables are expressed in natural logarithm.[6]

2.1.2. OECD indicators of product market regulation in network industries

The OECD indicators of regulation in energy, transport and communications (ETCR) measure product market regulation in seven network industries: telecoms, electricity, gas, post, rail, air passenger transport and road freight, from 1975 to 2013. They are based on over 700 qualitative and quantitative indicators reflecting the *de jure* regulatory provisions in these sectors and can be broken into the following sub-indicators: barriers to entry, public ownership, and, depending on sectors, vertical integration, market structure and price controls (for details, see Koske et al., 2015).[7] Scores range from zero to six, with a lower value indicating a less stringent degree of regulation.

2.1.3. Identification of major reform events

Major episodes of reductions in barriers to entry in each of the seven original network industries are identified using a new database on major reforms constructed by Duval et al. (2018). They identify reform events by examining documented legislative and regulatory actions in all individual network industries reported in all available OECD Economic Surveys for 26 advanced economies over the period 1970–2014, as well as additional country-specific sources. The methodology is closely related to the "narrative approach" used for instance by Romer and Romer (2004, 2010, and 2015) to identify monetary and fiscal shocks and periods of high financial distress.

For any of these actions to qualify as major reforms, one among the following three alternative criteria has to be met: (i) at the time of the reform, the OECD Economic Survey uses strong normative language suggestive of an important measure (for example, "major reform"); (ii) the policy action is mentioned repeatedly across different editions of OECD Economic Surveys, and/or in the retrospective summaries of key past reforms that are featured in some editions, for the country considered—a criterion that is very rarely met in isolation in practice, unlike the other two;[8] (iii) the OECD indicator of regulatory barriers to entry displays a very large change (in the 5th percentile of the distribution of the change in the indicator). When only the latter condition is met, an extensive search through other sources is performed to identify the precise timing and content of the policy action underpinning the change in the indicator.

In a context where our goal is to identify and trace out economies' responses to major reform events, this approach has several strengths compared to indirect methods used in other papers that rely exclusively on changes in OECD policy indicators. Specifically, our approach: i) identifies the exact timing of major legislative and regulatory actions taken by advanced economies since the early 1970s in the area of barriers to entry (while in many cases this timing would be incorrectly inferred from large changes in existing OECD indicators, see the examples of Austria, Belgium and Ireland in Table A.2 in the Appendix; the timing can even be off by two years in some other cases, see Duval et al., 2018); ii) identifies the precise reforms that underpin what otherwise looks like a gradual decline in OECD policy indicators without any obvious break (for example, the series of reforms that took place in the telecommunications industry in many countries in the mid-late 1990s); iii) goes beyond the scope of available OECD indicators in some cases (for example, as one example in Table A.2 illustrates, telecoms deregulation in the United States took place as a result of the antitrust lawsuit against AT&T, which was not a government measure and as such is not captured by the existing OECD indicator). The main limitation is that two large reforms that lower barriers to entry in a given sector can involve different specific actions. For example, barriers to entry in electricity may be reduced through vertical separation of the incumbent firm, or alternatively through a new law that regulates and ensures third-party access to the incumbent's electricity network. Our analysis does not distinguish between such cases, and as such it should be seen as estimating the average impact of major historical reforms.

Of the seven sectors covered by this dataset of reform events, three (air transport, postal and courier services, telecommunications) can be readily mapped to the three corresponding STAN industries (see Table A.3 in the appendix). For each of these three

[4] The structure of the data is based on the NACE Rev.2 industrial classification. We use the ISIC Rev.4 data when available, but extend the dataset with the ISIC Rev.3 data through backward extrapolation where needed.

[5] Reflecting capital stock measurement issues at this level of disaggregation and the short-term focus of the analysis, we ignore total factor productivity.

[6] The sectoral price deflator is defined relative to the aggregate price deflator and is therefore constructed as the logarithm of the ratio between the sectoral price deflator and the GDP deflator.

[7] The raw data are collected through detailed questionnaires filled by OECD country authorities, supplemented with data from publicly available sources. The qualitative information is transformed into quantitative information by assigning a numerical value to each possible response to a given question, and the coded information is normalized over a zero to six scale, with a lower value reflecting more pro-competitive regulation. Scores along these individual, low-level dimensions of regulation (e.g. how the terms of third-party access to the electricity transmission grid are determined, whether there is a liberalized wholesale market for electricity, etc.) are then aggregated into higher-level indicators for each industry (e.g. "entry barriers" indicator for electricity). For each industry, several higher-level indicators are constructed (e.g. "entry barriers", "public ownership", "vertical integration", "market structure" for the electricity sector), as well as an even higher-level summary indicator of the overall stringency of regulation in the industry considered (e.g., overall stringency of anti-competitive regulation in electricity sector as a whole). In the present paper, we focus primarily on entry barriers in each sector.

[8] Out of 312 major reforms in total, only 9 are identified based only on this criterion: New Zealand (1993), Switzerland (2001) and United States (1992) in electricity or gas; Japan (1995) in telecommunications; United Kingdom (1981) in postal services; United States (1980 and 1979) in rail and air transport (respectively); United Kingdom (1980, 1986) in road transport. In all of these cases, the description of the reform implies pretty clearly a major event (for details, see Appendix in Duval et al., 2018). Our results are not sensitive to removing these major reforms from our dataset.

sectors, the reform event variables take value 1 for those country-year observations that correspond to major reductions in barriers to entry, and 0 otherwise.

For the other four sectors (electricity, gas, rail transport, road transport), the mapping between reform events and STAN data is also straightforward but requires some aggregation. Specifically, reform shocks for the aggregate "electricity and gas" and "rail and road transport" sectors are constructed so as to be mapped to the two STAN industries "Electricity, gas, steam and air conditioning supply" and "Land transport and transport via pipelines", respectively. We use weighted averages of electricity and gas reform binary variables to construct the summary "electricity and gas" reform shock variable, and weighted averages of road and rail transport reform binary variables to construct the "rail and road transport" reform event variable. In both cases, we use weights equal to ¾ and ¼, which correspond approximately to the average shares of electricity and gas in total "electricity and gas" value added, and to the average shares of rail and road in total "rail and road transport" value added, for a subset of sample countries for which disaggregated historical data are available. This implies that the actual reform shock variable for the "electricity and gas" sector takes value 0.75, 0.25, 1 or 0 depending on whether there is a major reduction in barriers to entry in electricity only, gas only, both, or neither electricity nor gas. A similar coding approach is adopted for the "rail and road transport" sector. For all other sectors, the reform variable takes value 1 in the event of reform and 0 otherwise. This procedure yields 312 individual reform events: 101 in electricity and gas, 30 in rail- and road transport, 92 in air transport, 31 in postal services, and 58 in telecommunications.

Finally, country-specific macroeconomic variables (real GDP and GDP deflator) come from the World Bank WDI and IMF WEO databases.

2.1.4. Final dataset

Using all the sources listed above, we obtain a country-sector-time panel dataset covering 26 countries[9] and 5 sectors over the period 1975 to 2011. The panel is unbalanced, as the length of the time series varies greatly. We further impose a common sample for value added, employment, and labor productivity so as to obtain consistent impulse response functions for each of these variables and make meaningful comparisons between them.

2.2. Stylized facts

Before proceeding to the formal econometric analysis, we first provide a few stylized facts regarding our dataset that already hint at some of our key results. We start with a brief historical overview of deregulation in barriers to entry in our five network industries, before presenting simple difference-in-differences measures that illustrate how sector-level input and output variables of interest changed around the identified reform events.

Fig. 1 shows the evolution over time of OECD indicators in each of the five network industries. While all of them have undergone liberalization over the past three decades, they differ in the timing, speed, and scope of the deregulation. Major progress has been achieved in airline and telecommunication industries, with little remaining room for reform in most advanced economies. By contrast, scope for further progress still exists in postal services, electricity and gas, as well as (to a lesser extent) land transport, where reform has often started more recently and has proceeded at a slower pace. Furthermore, the gradual reduction in the cross-country dispersion of regulatory settings within each area is indicative of some gradual convergence in regulation, i.e. countries that initially had stricter regulations have unsurprisingly undergone greater liberalization.

Fig. 2 shows the distribution of our identified reform events over time (and across sectors). Two facts stand out. First, while a few countries such as the United States and the United Kingdom started implementing major reforms in some sectors already in early 1980s, deregulation was most intense and widespread in the 1990s and the 2000s—with some resurgence most recently as reforms were carried out in the wake of the global financial and euro area crises. For example, the early 90 s saw a fairly synchronized liberalization of the air transport sector, with many identified reforms over the 1993–1995 period as a result of the adoption of a single European market for aviation with the agreement by the European Council of Ministers of the Third Aviation Liberalization Package in June 1992. A similar pattern can be observed a couple of years later for the telecommunication sector, and to a lesser extent for electricity and gas. The existence of such waves of liberalizations, and the role of external pressure in driving them, will be exploited in our instrumental variables strategy.

As a first pass on the effects of these major reforms, we examine the evolution of sector-level output and inputs over a 10-year window centered around the year of the reform. For each of the five sectors, we compute a difference-in-differences measure of the average impact of reform on each variable of interest, where the "treatment group" consists of (country-sector-year) observations that experienced a reform in a particular year R and the "control group" comprises observations from the same industry that did not experience any reform over a 10-year window surrounding year R. In other words, by noting X^{REFORM} the average 5-year growth rate of the variable of interest for the treatment group, and $X^{NO\ REFORM}$ the average 5-year growth rate of the variable of interest for the control group, the difference-in-differences measure is defined as:

$$\left(X^{REFORM}_{R,\,R+5} - X^{REFORM}_{R-5,R}\right) - \left(X^{NO\ REFORM}_{R,\,R+5} - X^{NO\ REFORM}_{R-5,\,R}\right) \quad (1)$$

Fig. 3 shows the average value of this "difference-in-differences" measure across all reform events and sectors in our sample. For

[9] Australia, Austria, Belgium, Canada, Czech Republic, Denmark, Estonia, Finland, France, Greece, Germany, Hungary, Iceland, Ireland, Italy, Luxembourg, Mexico, Japan, Korea, Netherlands, New Zealand, Norway, Poland, Portugal, Slovak Republic, Slovenia, Spain, Sweden, Switzerland, United Kingdom, United States.

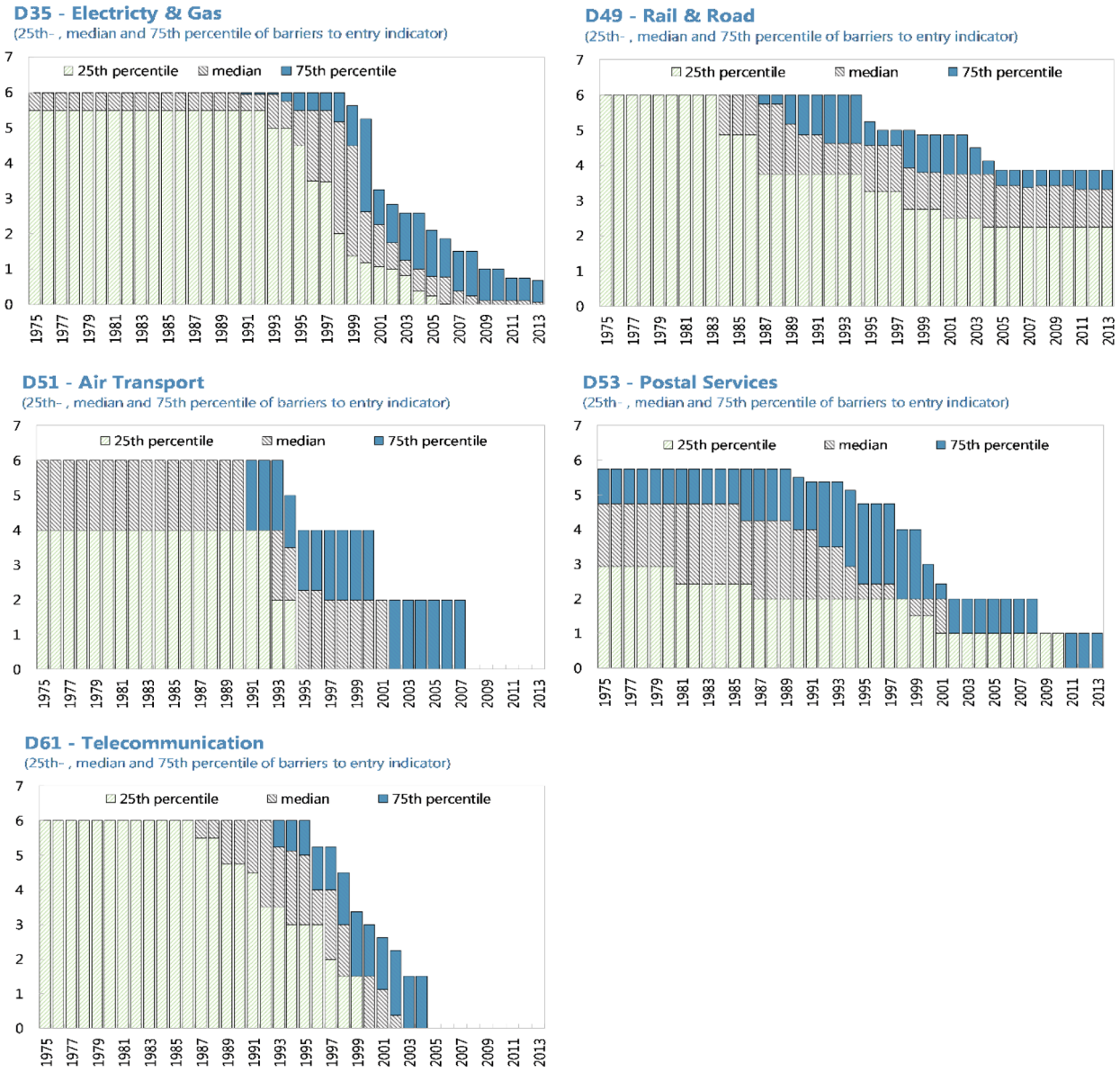

Fig. 1. Barriers to entry in network industries.
Source: OECD regulatory database, and authors' calculations. The chart shows the evolution over time of the stringency (on a 0–6 scale) and cross-country dispersion (median, 25th and 75th percentiles) of barriers to entry in each of the five network industries.

the "electricity and gas" and "rail and road transport" sectors, only simultaneous major reforms to both sub-components—i.e. both electricity and gas, and both rail and road transport, respectively—are considered for simplicity. The results are clear: compared with non-reforming observations, reforming observations experience a substantial pick-up in the growth rates of real value added, employment, and investment, as well as a decline in the relative price of value added (ratio of value-added deflator to GDP deflator) and concomitant increase in real wages. Relative to non-reforming country-sector observations, real value added rises by 9 percent in cumulative terms over the 5 years following the reform. While this chart suggests that reforms coincide with faster growth of output and inputs and lower prices, it does not explore the full dynamics of these effects and does not address possible sources of omitted variable bias. Both points are taken up in the econometric analysis.

3. Empirical setup

The descriptive analysis of the previous section suggests that reductions in barriers to entry go hand in hand with substantial changes in sectoral inputs and outputs. In this section, we rely on the local projection method to estimate more formally the dynamic impact of reforms on these sector-level outcomes. We start with OLS estimates, before addressing possible sources of omitted variable

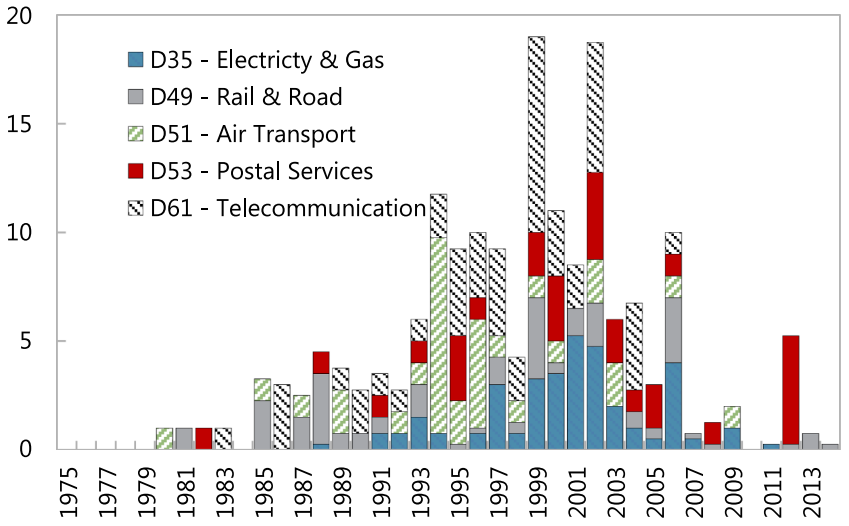

Fig. 2. Histogram of barriers to entry reforms.
Distribution across sectors and over time of major reductions in barriers to entry.
Note: Authors' calculations. The size of vertical bars represents the number of reform shocks occuring in any given year. For the "electricity and gas" sector, shocks take values 0.25, 0.75 or 1 depending on whether they affect only gas, electricity, or both. A similar coding approach is adopted for the "rail and road transport" sector. For all other sectors, a reform shock is scored 1 in the event of reform and 0 otherwise.

Fig. 3. A descriptive "difference-in-differences" measure of the impact of reform.
Difference in cumulative growth of selected variables between reforming and non-reforming observations between post-reform and pre-reform periods.
Source: Authors' calculations. The chart shows the average difference (in percentage points), between "reform" and "no reform" observations, of the difference between cumulative growth of the variable of interest over [R; $R + 5$] and cumulative growth over [R-5; R], where R is the year a major reform is observed in a particular country-sector. The "reform" observations are country-sector observations which experienced a major reform in year R. The "no reform" observations are observations for the same sector but in countries that did not experience any major reform over a 10-year window surrounding year R. For the "electricity and gas" and "rail and road transport" sectors, only simultaneous shocks to both sub-components—i.e. both electricity and gas, and both rail and road transport, respectively—are considered in these calculations.

bias through instrumental variable (IV) estimation.

3.1. Local projection method

The local projection method was proposed by Jordà (2005) to produce impulse response functions without the need for specifying and estimating a multivariate dynamic system. This approach has been advocated by Stock and Watson (2007) and Auerbach and Gorodnichenko (2012), among others, as a flexible alternative to vector autoregression (autoregressive distributed-lag) specifications

since it does not impose dynamic restrictions and is more robust to misspecifications of the data generating process. Accordingly, the local projection method has gained prominence across various strands of empirical literature—to take a recent example, Romer and Romer (2017) use this method to estimate the dynamic macroeconomic impact of financial crises.

In our case, the local projection method involves running a series of regressions of the log-difference of a given sector-level variable of interest (output, employment, investment, relative value-added deflator) between $t + j$ and t-1 on the reform event variable at time t, a set of fixed effects and additional controls. Specifically, for $j = 0, 1, \ldots, 4$, we estimate:

$$y_{c,s,t+j} - y_{c,s,t-1} = \beta_j^* reform_{c,s,t} + \sum_{k=1}^{j} \gamma_k^* controls_{c,s,t+k} + \lambda_{ct} + \lambda_{cs} + trend_s + e_{cst} \quad (2)$$

where y is the variable of interest, *reform* is the reform event variable, and *controls* include all past and future reform events in order to address any omitted variable bias arising from any auto-correlation of reform events. Note that the approach's flexibility means that it can accommodate highly non-linear dynamic effects of reforms. While our focus will on the short-to-medium term (1-to-5-year horizon), it is worth noting that over the long term, the specification neither assumes, nor precludes, that major reforms have permanent level effects, permanent growth effects, or only temporary level effects on the variable of interest.

Relative to macroeconomic (cross-country time-series) studies of the impact of reforms, our three-dimensional panel enables us to fully control for country-wide macroeconomic factors—which may correlate with reform events—through country-year fixed effects. We also include country-industry fixed effects and an industry-specific linear time trend. The former controls for structural country-industry-specific factors, such as permanent cross-country differences in the evolution of certain sectors that could arise for example from differences in comparative advantage. The industry-specific time trend is meant to control for the different trend growth rates of different industries at the global level, partly reflecting different rates of technological progress—e.g. the boom of the telecommunications industry observed over the sample period. As reforms in some sectors have been clustered around particular years (see above), we use an industry-specific time trend rather than industry-year fixed effects, as the latter could absorb some of the impact of product market deregulation. The industry-specific time trend absorbs the key relevant source of industry-level heterogeneity, namely different trend growth rates across industries (for example, faster trend growth in telecommunications sector compared to electricity and gas). Note that this set of fixed effects and controls is tighter than that featured in the recent related papers in the field using similar country-industry-level time-series data, none of which also includes country-industry fixed effects (see Bourlès et al., 2013, and Barone and Cingano, 2011).

3.2. Instrumental variables estimation

Despite our rich set of fixed effects and controls, Eq. (2) might still entail endogeneity bias, stemming from endogeneity of major reforms to (country-sector-level) growth prospects. It may also involve attenuation bias, due to possible measurement error in the reform event variable. To address these, we use instrumental variables techniques, considering two types of instruments that capture the scope for reforms and peer pressure from reforms in other countries:

- The scope for reform is larger in country-sector pairs where the initial stance of product market regulation is stricter. Indeed, once an area is deregulated, the scope for reform vanishes, and so does the likelihood of reform. To measure the initial stance of regulation, we use the three-year—and, in a robustness check, five-year—lagged value of the OECD indicator of the stringency of product market regulation in the country-sector considered.
- For a given country-sector pair, peer pressure is captured by the number of other countries in the sample that have carried out a major reform in that same sector over the past three years. A country is more likely to implement a reform when many other countries are carrying or have just carried out a similar reform.[10]

Both instruments can be considered as exogenous to a country-sector's present and future outcomes, and they should not have any direct effect on the left-hand side variable. For example, reforms in other countries are not driven by sector-level outcomes in the country considered, and they should not have any effect on the latter other than through pressure on domestic authorities to undertake reform. In an extension, based on a more restrictive model than our baseline, we show that this identification is not only conceptually compelling but is also supported by standard overidentification tests.

Since our variable of interest is a bounded indicator variable, Eq. (2) belongs to the family of endogenous dummy variable models (Heckman, 1978). In our preferred estimation, we use Wooldridge's two-step procedure instead of a traditional 2SLS approach (Wooldridge, 2010, Chapter 21). The first step involves the estimation of a probability model—in our case, an ordinal probit model of the reform variable with our exogenous instruments as explanatory variables.[11] The second step involves a standard IV estimation of

[10] In principle, this instrument might not be excludable if deregulation in foreign countries affected domestic outcomes in the sector considered. However, this is unlikely to be the case here given the largely non-tradable nature of the network industries considered here before *domestic* deregulation was implemented. One partial exception is (the cross-border component of) air transport; however, as will be shown below, our results are robust to excluding this industry from the analysis.

[11] We use an ordinal probit, given that the variable of interest—and therefore the dependent variable in the probability model—can take more than two values (0, 0.25, 0.75 or 1). The long time-series dimension of our sample also enables us to include all fixed effects featured in Eq. (2) in the probit model without generating biased and inconsistent estimates (see e.g. Greene, 2002). The probit model also includes the sector-specific time trends.

Table 1
Baseline OLS estimates: Effect of major reform event on real value added.

	(1) t	(2) $t+1$	(3) $t+2$	(4) $t+3$	(5) $t+4$
Entry Reform	0.13	2.25	3.83*	6.31**	8.96***
	(1.15)	(1.74)	(2.27)	(2.56)	(3.27)
Country-Year FE	Yes	Yes	Yes	Yes	Yes
Country-Ind. FE	Yes	Yes	Yes	Yes	Yes
Ind. trend	Yes	Yes	Yes	Yes	Yes
N	2,368	2,266	2,164	2,062	1,960

The dependent variable is the log difference of real value added between year t-1, the year preceding the event, and year $t + j$. Three lagged reform events as well as the forward events between t and $t + j$—following Teulings and Zubanov (2014)—are included, but not reported. Cluster robust standard errors are reported in parentheses. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively.

Eq. (2) using the predicted value of our first-step probit model as an instrument for the reform variable—in other words, the predicted value of the probit model is used as the instrument in the first stage of the 2SLS procedure, not directly as a regressor in the second stage. This approach has three useful properties (Woodridge, 2010, Chapter 21): i) there is no need to account for the fact that a generated instrument is used; ii) under mild assumptions, the IV estimator in the second step is asymptotically efficient in the class of IV estimators that make use of our chosen instruments; iii) it is robust to misspecification of the probit model in the first step.[12]

4. Econometric results

This section presents briefly the baseline OLS regression results and then turns to IV estimates.

4.1. Baseline OLS results

Table 1 shows the baseline OLS results for real value added in the deregulated industry over the five years—including the reform year—following a major reform. The estimated coefficient is positive but statistically insignificant upon impact. Its value then gradually increases over time, reaching about 6 percent after four years and 9 percent after five, with these cumulative effects being statistically significant at the 5 and 1 percent levels, respectively.[13]

The dynamic response of real value added and other sector-level outcomes is shown visually in Fig. 4 in the form of impulse response functions together with their 90 percent and 95 percent confidence intervals. The relative price of the deregulated industry falls after reform, with a concomitant rise in the real wage of a similar magnitude, both of which become statistically significant after 3 years. The difference between the two, which is the growth of nominal wages—more precisely, the growth of nominal wages in the deregulated industry relative to nominal wages in the economy as a whole, since the latter is absorbed by the country-year fixed effects—is roughly zero. These findings are roughly consistent with the theoretical predictions of Blanchard and Giavazzi (2003), with the exception of nominal wages which would be predicted to decline due to shrinking product market rents to be captured by workers.[14] Breaking down real value added into employment and labor productivity, we find that both components gradually rise after deregulation, although these increases remain borderline (in)significant over the medium term.[15] The estimated effect on investment is both flat and insignificant; while somewhat surprising, this result might partly reflect endogeneity concerns, an issue we take up in the next section. Also noteworthy is the one percent drop in employment in the very short term, which will become statistically significant in the IV estimates below.

4.2. Instrumental variables results

We now turn to IV estimates. Table 2 first shows the importance of our two instruments in explaining the occurrence of a reduction of barriers to entry. Their coefficients are both positive and highly statistically significant, in line with our prior that

[12] The drawback is that the eventual number of excluded instruments in the 2SLS is equal to the number of endogenous variables. This makes it impossible to support the validity of the excluded instruments with overidentification tests.

[13] Extending this table beyond year five ($t + 4$) would show the estimated effect levelling off starting from year six. We do not report these estimates here as our focus is on the short-to-medium-term effects of deregulation. Also, using as an instrument the five-year rather than three-year lag of the stance of product market regulation yields comparable estimates. These results are available upon request.

[14] As noted in the introduction, the absence of any significant decline in (relative) nominal wages following deregulation is not inconsistent with previous literature, which has found only small or insignificant effects, partly because only unionized workers appeared to have been affected (see e.g. Hendricks, 1994).

[15] This decomposition follows readily from defining labor productivity as the ratio of real value-added to total employment. It should be noted that by property of OLS, the two borderline (in)significant effects on employment and labor productivity can—and indeed here do—combine into a statistically significant effect on real value added.

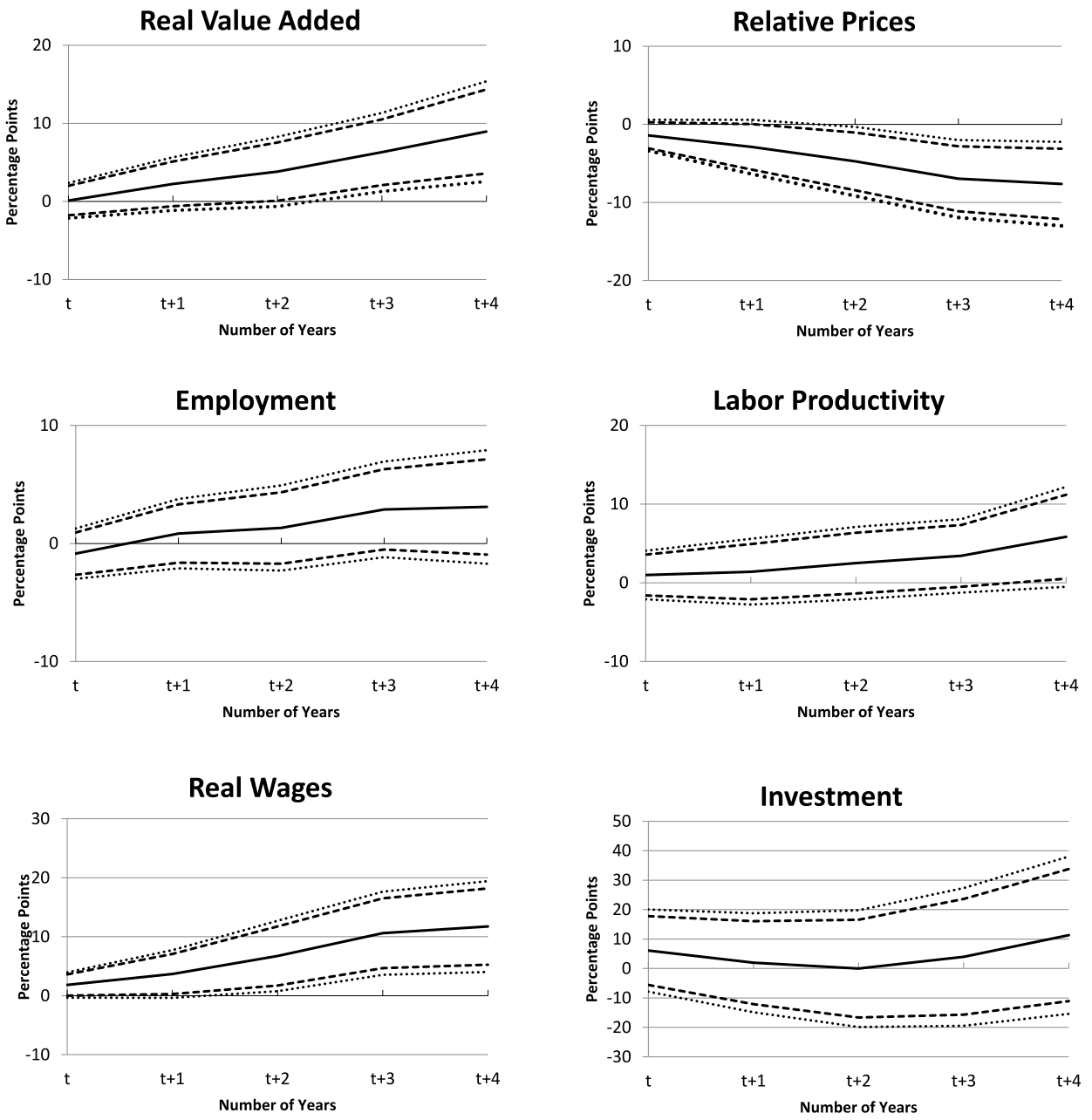

Fig. 4. OLS regressions – Impulse response functions.
Note: Impulse response functions for OLS estimations, of the log difference of real value added, relative prices, employment, labor productivity, real wages and investment, between t-1, the year preceding the shock, and $t + j$. The dashed and dotted lines show 90 and 95 percent confidence intervals, respectively.

both the scope for reforms and the external pressure to reform increase the likelihood of a significant reduction in barriers to entry.

Table 3 shows the IV results for real value added. The dynamics and magnitude of the estimated effects of deregulation are qualitatively similar to those obtained with OLS in Table 1. Point estimates and statistical significance rise gradually over time; after five years, real value added is about 10 percent higher than in the absence of reform, an effect that is significant at the 1 percent confidence level.[16] The 1st stage F-statistics is very high in all specifications. This is not surprising as the preceding probit model includes, besides the excluded instruments, all variables of the second stage; notably the country-year

[16] Also similar to the baseline OLS regression, the estimated coefficients level off afterward.

Table 2
Ordinal probit model of major reductions of barriers to entry.

Dependent variable: occurrence of major reduction of barriers to entry	
Peer pressure	0.128***
	(4.44)
Regulatory stance$_{t-3}$	0.450***
	(5.04)
Country-Year FE	Yes
Country-Ind. FE	Yes
Ind. trend	Yes
N	2,363

Note: Table shows estimates from ordinal probit regressions. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively.

Table 3
IV estimates: Effect of major reform event on real value added.

	(1) T	(2) $t+1$	(3) $t+2$	(4) $t+3$	(5) $t+4$
Entry Reform	1.90	5.88**	7.99**	9.49**	10.24**
	(1.99)	(2.66)	(3.30)	(3.77)	(4.57)
Country-Year FE	Yes	Yes	Yes	Yes	Yes
Country-Ind. FE	Yes	Yes	Yes	Yes	Yes
Ind. trend	Yes	Yes	Yes	Yes	Yes
N	2,363	2,266	2,164	2,062	1,960
1st Stage F	315.79	315.06	243.42	352.99	300.12

The dependent variable is the log difference of real value added between year t-1, the year preceding the event, and year $t+j$. Three lagged reform events as well as the forward reform events between t and $t+j$ are included, but not reported. Cluster robust standard errors are reported in parentheses. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively. In column 1, the number of observations differs from those of Table 1 due to incomplete coverage for the lagged regulatory stance at the end of the series.

and country-industry fixed effects, as well as the industry-specific time trends.[17]

The estimated impact of reform on real value added and other sector-level outcomes is shown visually in Fig. 5 in the form of impulse response functions. Results are qualitatively and quantitatively similar to the OLS results. One exception is that the short-term drop in employment and its increase over the medium term are now both stronger and more statistically significant. The negative short-term impact is in line with Bassanini and Cingano (2018) who, however, find this negative effect to be highly persistent, while it gradually turns positive in our case—consistent with a view that, over time, job creation by new entrants and expanding incumbents more than offsets any downsizing or exit of other incumbents that takes place upon impact.[18] Unlike OLS, IV estimates point to a statistically significant (at the 5 percent confidence level) impact of deregulation on investment over the medium-term.[19] One interpretation of this finding is that aggregate investment does not rise initially because incumbent firms face increased competition and shrink, but it eventually increases as new firms enter the market, prices fall and output expands.

It should be kept in mind that these estimates only reflect effects in the deregulated industries themselves, and do not factor in spillovers to other industries that could either amplify or mitigate impacts on the macroeconomy. On the one hand, as flagged above, industry deregulation entails well-documented positive spillovers to other downstream and upstream industries—those that supply inputs to, or demand inputs from the deregulated industries themselves (Barone and Cingano, 2011; Bourlès et al., 2013; Duval and Furceri, 2018). On the other hand, gradual resource reallocation to expanding deregulated industries could mean smaller gains at the economy-wide level compared to the industry level.

[17] These F-statistics should not be interpreted applying conventional "rules of thumb", such as for instance Staiger and Stock's (1997), that have often been used to assess the strength of instruments. This is because the F-statistics shown here is that of a regression of the actual reform variable on the predicted reform variable—as predicted by a separate (probit) regression of the reform variable on our two instruments. Also, although two instruments are used to predict the probability of a reform, the first stage of the 2SLS procedure uses only one instrument, which is this prediction. Consequently, no Hansen statistic is produced.

[18] The difference in results may reflect the fact that Bassanini and Cingano (2018) use more aggregate data (KLEMS), a different definition of reform shocks (the change in the OECD's "barriers to entry" sub-indicator), and a different econometric approach.

[19] Separately, we also tested for interactions between major entry barrier reforms and: i) labor market regulation, proxied by the OECD indicator of employment protection legislation for regular workers, given the extensive but inconclusive literature on interactions between labor and product market regulations—while theory typically points to substitutability (see e.g. Blanchard and Giavazzi, 2003; Cacciatore et al., 2016a), empirical evidence is more mixed, with some papers finding supportive evidence for complementarity (e.g. Aghion et al., 2008); ii) business conditions, measured following the approach of Auerbach and Gorodnichenko (2012), given recent evidence suggesting that the short-term effects of structural reforms may vary depending on prevailing macroeconomic conditions (e.g. Duval and Furceri, 2018). However, none of these interactions turned out to be statistically significant, and as a consequence we do not report the corresponding results here.

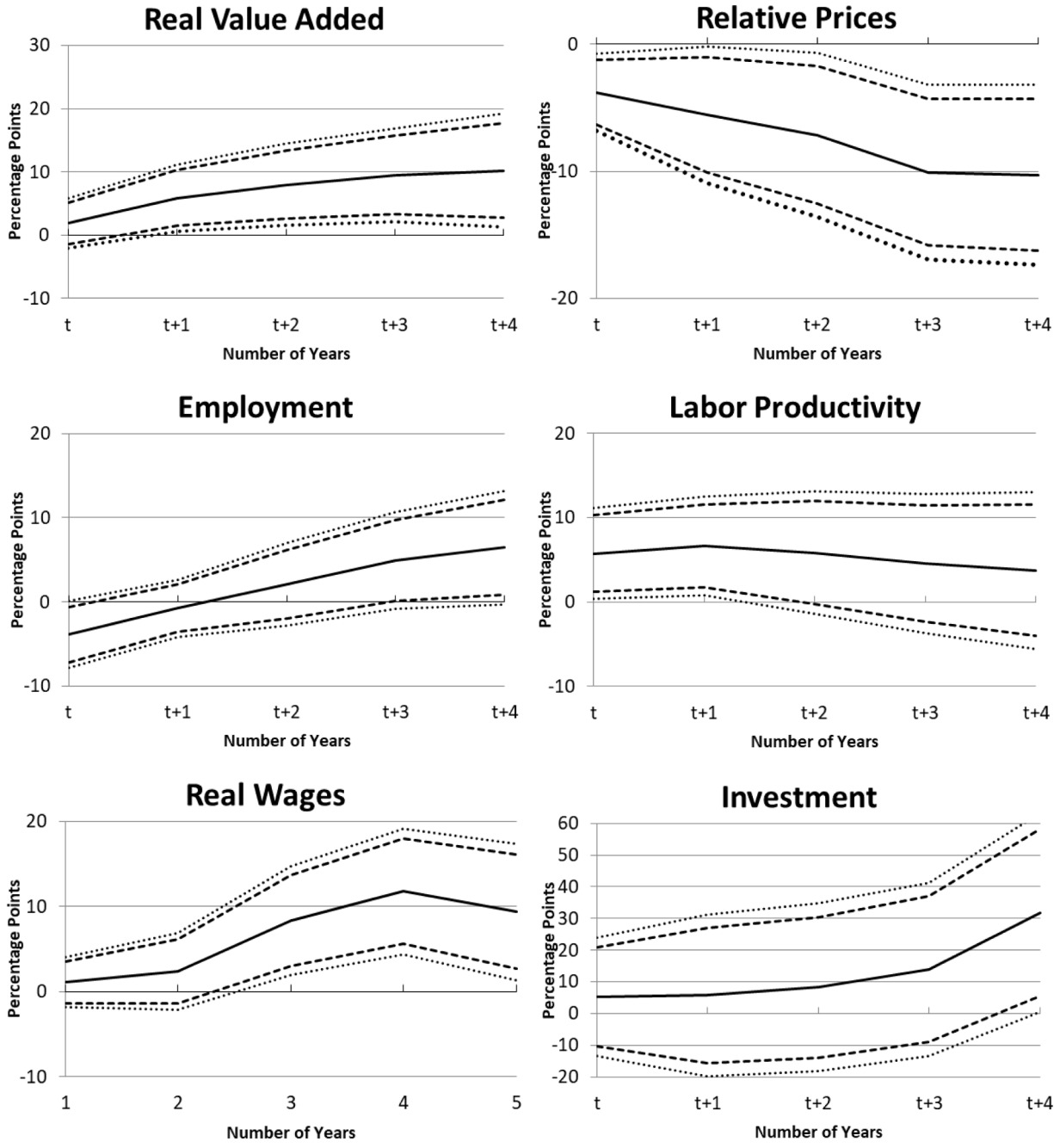

Fig. 5. IV regressions – Impulse response functions.
Note: Impulse response functions from IV estimates, where the first stage uses the predicted probability of a reform from ordered probit as instrument, and the second stage explains the log difference of real value added, relative prices, employment, labor productivity, real wages and investment, between t-1, the year preceding the shock, and $t + j$. The dashed and dotted lines show 90 and 95 percent confidence intervals, respectively.

4.3. Extensions and robustness

In this section we present a series of robustness checks of our main results. We show that results are robust to controlling for major changes in public ownership, alternative estimators, and removing specific countries or industries from the sample. We also perform a placebo test.

Table 4
Controlling for public ownership reforms.

Real value added	(1) t	(2) $t+1$	(3) $t+2$	(4) $t+3$	(5) $t+4$
Entry Reform OLS	0.22	1.19	1.98	5.01*	6.69**
	(1.16)	(1.70)	(2.08)	(2.66)	(3.15)
Public Ownership Reform OLS	−0.35	4.01	7.20*	5.38	8.63
	(2.46)	(3.67)	(4.31)	(4.69)	(6.44)
Entry Reform IV	3.54	7.29**	8.76*	17.02***	16.85**
	(2.63)	(3.45)	(4.75)	(6.39)	(7.53)
Public Ownership Reform IV	−2.23	0.62	3.12	−1.99	0.84
	(2.52)	(3.08)	(4.00)	(4.23)	(6.03)
Country-Year FE	Yes	Yes	Yes	Yes	Yes
Country-Ind. FE	Yes	Yes	Yes	Yes	Yes
Ind. trend	Yes	Yes	Yes	Yes	Yes

Note: The table shows results from OLS and IV regressions similar to those of Tables 1 and 3 but controlling for public ownership major reforms. Three lagged reform events as well as the forward reform events between t and $t+j$ are included, but not reported. Standard errors are in parentheses. *,**, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively.

Table 5
Placebo test.

Real value added	(1) t	(2) $t+1$	(3) $t+2$	(4) $t+3$	(5) $t+4$
Entry Reform OLS	0.61	−1.58	−3.05*	−4.19*	−2.67
	(0.93)	(1.85)	(1.73)	(2.51)	(3.33)
Entry Reform IV	2.37	0.31	−0.91	−0.28	−1.58
	(2.68)	(5.54)	(6.06)	(8.37)	(8.77)
Country-Year FE	Yes	Yes	Yes	Yes	Yes
Country-Ind. FE	Yes	Yes	Yes	Yes	Yes
Ind. trend	Yes	Yes	Yes	Yes	Yes

Note: The table shows results from OLS and IV placebo regressions (similar to Tables 1 and 3) in which the reform is assumed to take place five years earlier than the actual reform year. Three lagged reform events as well as the forward reform events between t and $t+j$ are included, but not reported. Standard errors are in parentheses. *,**, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively.

Table 6
Effect of major reform event on real value added at $t+4$ when dropping individual industries.

Effect on real VA when dropping industry:	(1) Electricity and gas	(2) Rail and road transport	(3) Air transport	(4) Postal services	(5) Telecoms
Entry Reform OLS	9.42**	9.88**	5.47*	12.50***	6.51
	(3.58)	(3.81)	(3.14)	(3.67)	(4.61)
Entry Reform IV	10.17**	11.97**	9.08**	21.30***	11.53**
	(4.57)	(4.99)	(4.69)	(5.73)	(5.82)

Note: The table shows results for OLS and IV regressions (similar to Tables 1 and 3) for real value added at year $t+4$ when one of the five industries in the sample is dropped. Lead and lagged major reform events, country-industry and country-time fixed effects as well as industry specific time trends are included but not reported. Standard errors in parentheses. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively.

4.3.1. Controlling for major changes in public ownership

Major changes in barriers to entry may occur simultaneously with other types of product market reforms, primarily changes in public ownership (e.g. privatization). The literature suggests that privatization episodes can affect output, job losses, labor productivity, and wages (see e.g. Megginson and Netter, 2001 for a survey). Therefore, major changes in public ownership in deregulated industries could be a confounding factor in the above regressions.

To investigate this possibility, we re-run our OLS and IV regressions, but now controlling also for major reforms of public ownership (occurring between three years before the year t of the barrier to entry reform and $t+j$, *for* $j = 0,\ldots,4$), which we identify using the same methodology as above (for details, see Duval et al., 2018). As can be seen in Table 4, our main results regarding the impact of entry deregulation on real value added remain qualitatively unchanged and increase somewhat in magnitude.[20]

The real value-added effect of public ownership reforms is not statistically different from zero. As the full results displayed in the appendix show, the estimated effects of entry reforms on other variables also remain largely unchanged or are marginally

[20] Results for other dependent variables are reported in Tables A.4 and A.5 of the Appendix.

strengthened when controlling for public ownership reforms while, by contrast, the latter are typically not found to be statistically significant in these regressions.[21]

4.3.2. Placebo test

We further support the causal interpretation of our results through a placebo test. Specifically, we run a placebo regression in which major reforms to barriers to entry are assumed to have taken place five years before the actual reform dates, keeping the sample identical. Note that we need at least a 5-year gap between the actual and placebo reform dates given that we test for the impact of actual reforms up to 4 years after their inception—here, the impact of the placebo reform at horizon $t + 4$ is the impact of the actual reform at horizon t-1, which we expect to be zero. Table 5 shows the cumulative effects of the placebo reform on real value added in both OLS and IV regressions. The placebo effect estimated by OLS is either insignificant, or *negative* and significant at the 10 percent level. The latter result is in line with the notion that reforms may be partly driven by poor past economic conditions. Consistent with this, and quite reassuringly, IV estimates of the placebo effect are generally close to zero and statistically insignificant at all horizons. Similar placebo tests for other sector-level outcomes also generally yield statistically insignificant estimates at all horizons.[22]

4.3.3. Sensitivity to specific industries or countries

Finally, to check that our results are not driven by specific industries or countries, we re-run our baseline regressions dropping each industry or country at a time. Due to space constraints, we restrict ourselves to the effect on real value added at $t + 4$, reporting both OLS and IV results when dropping industries (Table 6) and countries (Appendix Tables A.6 and A.7). While OLS estimates are sensitive to dropping the telecommunication sector (and to a lesser extent air transport), IV estimates are robust. Notably however, the impact of deregulation on output doubles when postal services are excluded from the sample, which suggests much weaker gains from reform in this industry than in others (Table 6, column 4). Conversely, there is some evidence that entry deregulation paid off more in air transport than in other industries (Column 3). The output effects of deregulation are found to be very little sensitive to dropping specific countries, in terms of both magnitude and statistical significance (Appendix, Tables A.6 and A.7); point estimates vary somewhat but always remain significant at least at the 5 percent level. The sensitivity of results for other dependent variables to dropping certain sectors is similar. The effects on prices and employment weaken somewhat when dropping air transport or telecommunications, while the effect on labor productivity strengthens when dropping postal services (see Appendix Table A.8).

5. Conclusion

The paper has assessed the short-to-medium-term impact of product market deregulation by focusing on major reform episodes in some of the historically most protected non-manufacturing industries, based on a unique mapping between a new narrative dataset of reform shocks and sector-level outcomes for five network industries in twenty-six countries spanning over three decades. The use of a three-dimensional panel and careful instrumentation of reform shocks using external instruments enabled us to control for economy-wide macroeconomic shocks and address possible sources of omitted variable bias more broadly. Using a local projection method, we found that deregulation pays off only gradually but does not entail any short-term costs—with the exception of a borderline significant fall in employment during the year of the reform. Major cuts in barriers to entry have a statistically positive impact on output three to four years after the reform, concomitant with price declines. The medium-term impact on real value added in the deregulated industry is large, in the order of 10 percent after five years, on average across major past episodes of reduction in barriers to entry. These results suggest that entry takes time due to real frictions. They are also broadly consistent with the dynamic effects of reductions in entry costs predicted by the canonical model of Blanchard and Giavazzi (2003). On the policy front, while our findings cannot be readily interpreted as macroeconomic effect, they tend to strengthen the case for intensifying product market reform efforts in advanced economies, particularly in Europe, but also in emerging economies where the scope for deregulation remains larger and trend growth also weakened over the past decade.

Appendix

Table A.1, Table A.2, Table A.3, Table A.4, Table A.5, Table A.6, Table A.7, Table A.8.

[21] A number of other country-sector-year level confounders might be envisaged, including reforms of different dimensions of regulation such "vertical integration" and "market structure", or labor market reforms implemented over the same period that would have different effects on different sectors (for example because of different labor intensities). Regressions (not reported here but available upon request) controlling for reforms of "vertical integration" and "market structure" yield results that are qualitatively similar to the baseline results. The same holds true when controlling for major labor market reforms as identified by Duval et al. (2018) in the areas of job protection and unemployment benefits—where each of these country-level dummy reform variables is interacted with the country-sector-level labor income share in value added, under the assumption that labor market reforms might *a priori* have a larger impact on those country-sectors that are more intensive in labor—note that any effects of (country-level) labor market reforms that do *not* vary across sectors are already fully controlled for by the country-time fixed effects.

[22] Results not reported, but available from the authors on request.

Table A.1
Variable definitions and data sources.

Variable	Definition	Source
PRDK	Production (gross output) in volume.	OECD STAN Structural Analysis Database. See http://www.oecd.org/sti/ind/stanstructuralanalysisdatabase.htm for details. Where useful to increase coverage, data is complemented by an older vintage and backward interpolated based on national sources. LPRD is calculated as VALK/EMPN. Real wages are calculated as LABR/VALP/EMPN. For the empirical analysis variables are in logs. Sectoral prices levels (VALP) are transformed into log-deviation from the GDP deflator (source: World Bank).
PRDP	Production deflator.	
VALK	Value added in volume.	
VALP	Value Added deflator.	
GFCK	Gross fixed capital formation in volume.	
EMPN	Number of persons engaged (total employment).	
EMPE	Number of employees.	
LPRD	Labor Productivity.	
LABR	Labor Costs (compensation of employees).	
ETCR	OECD indicators of regulation in energy, transport and communications (ETCR) summarizing regulatory provisions in seven sectors: telecoms, electricity, gas, post, rail, air passenger transport, and road freight along six dimensions: Barriers to entry, Public ownership, Market structure, Vertical integration, Price controls, and Constraints on business operation.	OECD. See http://www.oecd.org/regreform/reform/44754663.pdf and Conway and Nicoletti (2006) for details.
GDP	Real GDP.	IMF WEO database.
GDPP	GDP deflator.	World Bank WDI.
EPLR	OECD Employment Protection Legislation index for regular employment.	OECD.
Reform	Major reforms identified from a narrative approach using *OECD Economic Surveys* and OECD indicators of regulation in energy, transport and communications (ETCR).	Duval et al. (2018).

Table A.2
Examples of reforms identified in the telecommunications industry.

Country	Year	Content	Normative language	Mentioned in later OECD Surveys	Large change in OECD Indicator
Austria	1998	…progress has been made in the telecommunications sector, where a new telecommunications law came into force in August 1997. The law establishes the regulatory framework for introducing competition into the market for telecommunications. With respect to infrastructure, it allows the establishment of telecom networks without approval and without fees. Suppliers of telecom services with a dominant market position are required to grant competitors open access to their network. (pg. 113, 1998)	…progress has been made in the telecommunications sector… (pg. 113, 1998)	1999, 2003	Yes in 1999
Belgium	1998	The government decides to liberalize telecommunications markets completely from 1 January 1998. (pg. 104, 1999)	liberalize telecommunications markets completely (pg. 104, 1999)	1999	Yes
Denmark	1995	Tele Danmark, the national provider of telecommunication services is scheduled to be exposed to competition along the lines of EU's Telecommunication Directive. As a first step toward liberalization, the company's monopoly on operating the network was curtailed in mid-1995. (pg. 106, 1996)	…the company's monopoly on operating the network was curtailed… (pg. 106, 1996)	No	No
Finland	1994	As of 1 January 1994, private suppliers of long-distance domestic telecom services have been allowed to operate. Moreover, with effect from 1 July 1994, two private suppliers of international telecommunication services have been admitted to the market, which so far has been the exclusive domain of the state-owned PTT. With these measures, the liberalization of the telecom industry in Finland appears to be progressing more than required by the EEA and EU agreements (pg. 43–44, 1995)	…liberalization of the telecom industry in Finland appears to be progressing more than required by the EEA and EU agreements (pg. 43–44, 1995)	2004	Yes

(*continued on next page*)

Table A.2 (*continued*)

Country	Year	Content	Normative language	Mentioned in later OECD Surveys	Large change in OECD Indicator
Ireland	1999	The government decides to liberalize voice telephony in December 1998, and an independent telecommunications regulator is set up. (pg. 116, 1999)	Full deregulation of the telecoms sector has now been achieved. (pg. 21, 1999)		Yes in 1998
New Zealand	1990	All statutory protections of Telecom's monopoly will be removed by April 1989: other firms will be able to provide network services in competition with Telecom. By July 1989, Telecom will allow competitors connections with Telecom's own network on fair and reasonable terms. (pg. 53, 1989)	All statutory protections of Telecom's monopoly will be removed (pg. 53, 1989)	1991 2005	Yes
United States	1983	…the most important deregulatory move in telecommunications came with the antitrust suit against AT&T by the U.S. Department of Justice which was filed in 1974 and settled in early 1982. As part of the settlement, AT&T agreed to divest itself of the local portions of its twenty-two Bell operating companies, which were restructured into seven separate regulated monopolies…AT&T can continue to provide long-distance service and to manufacture terminal equipment, but customers can choose any long-distance carrier they wish… This choice is enhanced by the equal access provisions of the decree, which require that all long-distance companies get the same connection to local networks as that afforded to AT&T… (pg. 69–70, 1986)	…the most important deregulatory move… (pg. 69–70, 1986)	1989 2004	No

Table A.3
Industry correspondence.
Sources: Conway and Nicoletti (2006) and United Nations Statistics Division ISIC Rev.4 structure.

OECD ETCR indicator	Production, value added, gross fixed capital formation, and employment data (STAN)
Gas (production/import, transmission, supply). Electricity (generation, transmission, distribution, supply).	D35 – Electricity, gas, steam and air conditioning supply.
Railways (passenger and freight transport, operation of infrastructure). Road transport (freight).	D49 – Land transport and transport via pipelines.
Airlines (passenger transport, international and domestic routes).	D51 – Air transport.
Post (basic letter, basic parcel, courier).	D53 – Postal and courier activities.
Telecoms (trunk, international, mobile).	D61 – Telecommunications.

Table A.4
OLS estimates of impact of entry reform, controlling for public ownership reform.

	(1) t	(2) $t+1$	(3) $t+2$	(4) $t+3$	(5) $t+4$
	Dependent variable: Real Value Added				
Entry Reform	0.22	1.19	1.98	5.01*	6.69**
	(1.16)	(1.70)	(2.08)	(2.66)	(3.15)
Public Ownership Reform	−0.35	4.01	7.20*	5.38	8.63
	(2.46)	(3.67)	(4.31)	(4.69)	(6.44)
	Dependent variable: Relative Prices				
Entry Reform	−1.75	−2.99	−4.94**	−7.00***	−7.23***
	(1.07)	(1.84)	(2.28)	(2.51)	(2.51)
Public Ownership Reform	1.47	0.47	0.73	0.04	−1.59
	(1.72)	(2.48)	(3.35)	(3.88)	(4.66)
	Dependent variable: Employment				

(*continued on next page*)

Table A.4 (*continued*)

	(1) t	(2) $t+1$	(3) $t+2$	(4) $t+3$	(5) $t+4$
Entry Reform	−1.29	0.70	0.33	2.19	2.53
	(1.02)	(1.67)	(1.91)	(2.09)	(2.62)
Public Ownership Reform	1.67	0.51	3.82	2.20	1.67
	(1.04)	(2.30)	(3.51)	(3.61)	(4.05)
	Dependent variable: Investment				
Entry Reform	5.41	2.50	−2.04	5.09	15.67
	(7.17)	(8.92)	(9.97)	(12.28)	(14.80)
Public Ownership Reform	4.74	−5.48	7.80	−7.35	−15.99
	(9.88)	(16.15)	(18.86)	(14.32)	(19.09)
	Dependent variable: Labor Productivity				
Entry Reform	1.51	0.50	1.64	2.82	4.16
	(1.64)	(2.30)	(2.47)	(2.76)	(3.73)
Public Ownership Reform	−2.01	3.50	3.39	3.18	6.97
	(2.42)	(3.48)	(4.39)	(4.42)	(6.41)
	Dependent variable: Real Wages				
Entry Reform	2.02*	3.64*	5.97**	9.69***	10.24***
	(1.10)	(1.94)	(2.82)	(3.60)	(3.69)
Public Ownership Reform	−0.72	0.04	3.25	3.47	5.47
	(2.15)	(3.17)	(4.72)	(5.98)	(6.35)

Note: This table shows results of the same OLS regressions as those in Table 1 when controlling for major public ownership changes, and for various dependent variables. Three lagged reform events as well as the forward reform events between t and $t + j$ are included, but not reported. Standard errors are in parentheses. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively.

Table A.5
IV estimates of impact of entry reform, controlling for public ownership reform.

	(1) t	(2) $t+1$	(3) $t+2$	(4) $t+3$	(5) $t+4$
	Dependent variable: Real Value Added				
Entry Reform	3.54	7.29**	8.76*	17.02***	16.85**
	(2.63)	(3.45)	(4.75)	(6.39)	(7.53)
Public Ownership Reform	−2.23	0.62	3.12	−1.99	0.84
	(2.52)	(3.08)	(4.00)	(4.23)	(6.03)
	Dependent variable: Relative Prices				
Entry Reform	−5.36**	−9.59**	−12.07**	−13.84**	−13.74**
	(2.28)	(4.32)	(5.20)	(5.90)	(6.38)
Public Ownership Reform	3.31**	3.95	4.78	4.31	3.42
	(1.68)	(2.46)	(3.03)	(3.47)	(4.22)
	Dependent variable: Employment				
Entry Reform	−2.45	−2.65	−0.63	2.88	2.69
	(1.61)	(2.68)	(4.56)	(5.77)	(6.64)
Public Ownership Reform	2.04	2.00	4.04	1.53	0.46
	(1.25)	(2.09)	(3.35)	(4.10)	(4.60)
	Dependent variable: Investment				
Entry Reform	11.41	6.05	−1.32	9.50	6.50
	(10.06)	(15.91)	(17.00)	(20.18)	(22.67)
Public Ownership Reform	1.90	−5.32	8.78	−6.09	−11.30
	(9.89)	(15.46)	(17.62)	(13.07)	(19.73)
	Dependent variable: Labor Productivity				
Entry Reform	5.99**	9.94***	9.39*	14.14**	14.16*
	(2.58)	(3.82)	(5.54)	(6.86)	(8.22)
Public Ownership Reform	−4.27*	−1.39	−0.92	−3.51	0.37
	(2.48)	(3.14)	(4.13)	(4.25)	(6.50)
	Dependent variable: Real Wages				
Entry Reform	1.65	5.11	8.75	15.93**	13.60
	(2.04)	(3.80)	(6.13)	(7.50)	(8.56)
Public Ownership Reform	0.76	−0.69	3.72	−0.67	−0.66
	(1.99)	(3.95)	(5.80)	(6.55)	(6.74)

Note: This table shows results of the same IV regressions as those in Table 3 when controlling for major public ownership changes, and for various dependent variables. Three lagged reform events as well as the forward reform events between t and $t + j$ are included, but not reported. Standard errors are in parentheses. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively.

Table A.6
Effect of major reform event on real value added at $t + 4$ when dropping individual countries – OLS results.

Effect on real value added when dropping:	none	AUS	AUT	BEL	CAN	CHE	CHL
Entry Reform	8.96***	8.74***	7.63**	8.60**	9.87***	8.63**	8.96***
	(3.27)	(3.30)	(3.17)	(3.33)	(3.38)	(3.34)	(3.27)
N	2,368	1,915	1,800	1,900	1,843	1,919	1,960
Effect on real value added when dropping:	CZE	DEU	DNK	ESP	EST	FIN	FRA
Entry Reform	8.28**	8.42**	9.88***	9.64***	8.96***	9.56***	9.07***
	(3.35)	(3.64)	(3.33)	(3.36)	(3.27)	(3.38)	(3.44)
N	1,885	1,880	1,785	1,863	1,960	1,795	1,841
Effect on real value added when dropping:	GBR	GRC	HUN	IRL	ISL	ITA	JPN
Entry Reform	9.09***	9.85***	8.96***	8.96***	8.96***	8.98**	8.96***
	(3.30)	(3.29)	(3.27)	(3.27)	(3.27)	(3.53)	(3.27)
N	1,894	1,910	1,960	1,960	1,960	1,883	1,960
Effect on real value added when dropping:	KOR	LTU	LUX	LVA	MEX	NLD	NOR
Entry Reform	8.96***	8.96***	9.32***	8.96***	8.96***	6.63**	9.36***
	(3.27)	(3.27)	(3.31)	(3.27)	(3.27)	(3.11)	(3.53)
N	1,960	1,960	1,900	1,960	1,960	1,860	1,789
Effect on real value added when dropping:	NZL	POL	PRT	SVK	SVN	SWE	USA
Entry Reform	8.96***	8.96***	8.96***	8.62**	8.96***	9.10***	9.70***
	(3.27)	(3.27)	(3.33)	(3.30)	(3.27)	(3.28)	(3.43)
N	1,960	1,960	1,910	1,912	1,960	1,906	1,810

Note: The table shows OLS regression results for real value added at year $t + 4$ when the country mentioned is dropped from the estimation sample. Lead and lagged reform events, country-industry and country-time fixed effects as well as industry specific trends are included but not reported. Standard errors in parentheses. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively.

Table A.7
Effect of major reform event on real value added at $t + 4$ when dropping individual countries – IV results.

Effect on real value added when dropping:	None	AUS	AUT	BEL	CAN	CHE	CHL
Entry Reform	10.42**	13.34**	11.63**	12.25**	14.51***	13.78***	13.54***
	(4.29)	(5.23)	(5.18)	(5.04)	(5.19)	(4.95)	(4.80)
N	2,575	2,108	2,000	2,080	2,043	2,113	2,160
1st Stage F stat	290.33	240.10	203.40	240.13	239.42	221.95	268.75
Effect on real value added when dropping:	CZE	DEU	DNK	ESP	EST	FIN	FRA
Entry Reform	13.29**	12.40**	15.30***	15.52***	13.54***	14.94***	13.79***
	(5.68)	(5.32)	(4.81)	(5.38)	(4.80)	(4.97)	(4.83)
N	2,085	2,079	1,985	2,063	2,160	1,992	2,041
1st Stage F stat	194.60	208.91	301.68	198.41	268.75	244.80	262.67
Effect on real value added when dropping:	GBR	GRC	HUN	IRL	ISL	ITA	JPN
Entry Reform	14.65***	15.38***	13.54***	13.54***	13.54***	12.61**	13.47***
	(4.88)	(4.95)	(4.80)	(4.80)	(4.80)	(5.29)	(4.87)
N	2,087	2,110	2,160	2,160	2,160	2,063	2,129
1st Stage F stat	251.19	234.79	268.75	268.75	268.75	220.64	264.81
Effect on real value added when dropping:	KOR	LTU	LUX	LVA	MEX	NLD	NOR
Entry Reform	13.39***	13.54***	13.27***	13.54***	13.54***	9.90**	12.72***
	(4.90)	(4.80)	(4.98)	(4.80)	(4.80)	(5.00)	(4.54)
N	2,127	2,160	2,090	2,160	2,160	2,045	1,989
1st Stage F stat	256.49	268.75	251.61	268.75	268.75	188.73	239.30
Effect on real value added when dropping:	NZL	POL	PRT	SVK	SVN	SWE	USA
Entry Reform	13.81***	13.54***	13.78***	11.90**	13.54***	14.19***	14.34***
	(4.82)	(4.80)	(5.10)	(4.68)	(4.80)	(4.81)	(5.19)
N	2,147	2,160	2,097	2,112	2,160	2,085	2,010
1st Stage F stat	254.84	268.75	215.70	236.66	268.75	275.45	243.40

Note: The table shows IV regression results for real value added at year $t + 4$ when the country mentioned is dropped from the estimation sample. Lead and lagged reform events, country-industry and country-time fixed effects as well as industry specific trends are included but not reported. Standard errors in parentheses. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively.

Table A.8
Effect of major reform event at $t + 4$ when dropping individual sectors.

	Dropped sector:	(1) E&G	(2) R&R Tr	(3) Air Tr	(4) Postal	(5) Telecom
Value Added	OLS	9.42**	9.88**	5.47*	12.50***	6.51
		(3.58)	(3.81)	(3.14)	(3.68)	(4.61)
	IV	10.17**	11.97**	9.08*	21.30***	11.53**
		(4.57)	(4.99)	(4.69)	(5.73)	(5.82)
Relative Prices	OLS	−7.42**	−8.53**	−5.16**	−9.19***	−7.78**
		(3.33)	(3.56)	(2.14)	(3.27)	(3.52)
	IV	−10.68**	−12.41**	−3.46	−14.42***	−6.66
		(5.09)	(5.79)	(2.67)	(5.54)	(5.91)
Employment	OLS	3.50	4.41	1.84	4.40	0.24
		(2.97)	(2.98)	(2.70)	(2.86)	(2.90)
	IV	7.40**	6.67*	4.60	7.56*	6.74
		(3.46)	(3.53)	(3.05)	(4.22)	(4.18)
Labor Productivity	OLS	5.92	5.47	3.63	8.11**	6.27
		(3.63)	(4.07)	(2.29)	(3.90)	(5.20)
	IV	2.78	5.30	4.48	13.74**	4.79
		(4.52)	(4.90)	(3.99)	(5.71)	(7.76)
Real Wages	OLS	9.79**	9.99***	5.01	10.07***	4.67
		(3.93)	(3.64)	(3.33)	(3.73)	(2.93)
	IV	13.52***	14.05***	9.41***	15.32***	8.39*
		(4.78)	(5.45)	(3.57)	(5.79)	(4.99)

Note: The table shows results for OLS and IV regressions (similar to Tables 1 and 3) for real value added at year $t + 4$ when one of the five industries in the sample is dropped. Lead and lagged major reform events, country-industry and country-time fixed effects as well as industry specific time trends are included but not reported. Standard errors in parentheses. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively.

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