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Product Market Deregulation and Growth: New Country-Industry-Level Evidence

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Abstract

The paper investigates the economic effects of major product market reforms in some of the historically most protected non-manufacturing industries. It relies on a unique mapping between new annual data on reform shocks and sector-level outcomes for five network industries (electricity and gas, land transport, air transport, postal services, and telecommunications) in twenty-six countries spanning over three decades. The use of a three-dimensional panel and careful instrumentation of reform shocks using external instruments enables us to control for economy-wide macroeconomic shocks and address possible sources of omitted variable bias more broadly. Using a local projection method, we find that major reductions in barriers to entry yield large increases in output and labor productivity over a five-year horizon, concomitant with a relative price decline. By contrast, there is only a weak positive effect on sectoral employment, and investment is essentially unaffected, suggesting that output gains from reform primarily reflect higher total factor productivity. It takes some time for these gains to materialize: effects become statistically significant two to three years after the reform, as prices start dropping, and productivity and output increase significantly. However, there is no evidence of any negative short-term cost from reform, including under weak macroeconomic conditions. These findings provide a clear case for intensifying product market reform efforts in advanced economies at the current juncture of weak growth.

JEL Classification Numbers: L51; O43; O47

Keywords: Structural reforms, deregulation, competition, entry barriers, product market, growth

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I. INTRODUCTION

Against the background of permanent output losses from the global financial crisis and a protracted decline in potential growth, structural reforms are being increasingly advocated by policymakers and institutions alike as a way to revive growth in advanced economies (e.g. Draghi, 2015; IMF, 2015, 2016; OECD, 2016). Product market reforms feature high on this agenda; even though widespread deregulation has already taken place across advanced economies (AEs) 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 countries (Koske et al., 2015). Furthermore, regulatory barriers to entry remain high in most network industries in emerging market economies (EMEs), and the potential growth slowdown in these countries has raised the urgency of reforms (Dabla-Norris et al., 2013; Cubeddu et al., 2014).

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 fiercely debated and is primarily an empirical matter. Conventional large-scale DSGE models generally find that price mark-up reductions yield gradual output gains, but their dynamics depends crucially on model-specific features and parametrization (e.g. Arpaia et al., 2007, Everaert and Schule, 2008; Gomes et al., 2011). Furthermore, recent papers that explicitly incorporate entry barriers in a more realistic set-up with endogenous firm entry and search and matching frictions in the labor market point to short-term costs from reforms, partly reflecting transitory losses from the downsizing of incumbents (Cacciatore and Fiori, 2015; Cacciatore et al., 2016a).[1] More broadly, no single modeling framework can possibly encompass all the relevant frictions and market imperfections that shape the dynamic effects of reforms. All models typically assume that firms maximize profits under monopolistic competition, while market structure (e.g. oligopolies) and firm behavior may well differ in practice, due e.g. to strategic behavior or non-profit objectives and presence of X-inefficiency (Leibenstein, 1966), particularly in large state-owned incumbent firms prior to deregulation in the network industries analyzed in the present paper. It could even be that reform might not boost supply in the industry considered in the short term, e.g. due to capital scrapping. This could happen in the event of abrupt industry-level liberalization that triggers the disappearance of incumbent firms, whereby not only physical but also potentially firm-specific human capital might be destroyed.

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 are five key non-manufacturing industries that have undergone significant deregulation, whose effects can therefore be assessed with the view to drawing more general lessons. The analysis makes use of a unique mapping between reform and outcome indicators in each of these five

[1] A further issue that goes beyond the scope of the present paper is whether product market reform could be contractionary in the short term when monetary policy is constrained by the zero lower bound (Eggertsson et al., 2014).

industries. Abrupt reductions in barriers to entry are identified based on a new database of reform shocks that compiles all large regulatory and legislative measures taken in each sector for 26 countries starting from the 1970s (Duval et al., 2016). 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 shocks and sector-level outcomes yields a country-sector-time panel dataset that we then use to estimate the dynamic impact of reform shocks 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, 2015) or fiscal shocks (Jordà and Taylor, 2013). We address carefully possible sources of omitted variable bias in two ways: first, by controlling 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 in the sector considered the occurrence of reforms in that same sector in other countries, the initial regulatory stance, and—given the dominance of EU countries in our sample—a dummy variable for the existence of a recent EU directive that mandated deregulation but had not been transposed into domestic law.

Our empirical analysis yields three main findings. First, major reductions in barriers to entry yield large increases in output and labor productivity over the medium term, concomitant with a relative price decline. By contrast, sectoral employment and investment are essentially unaffected, suggesting that output gains from reform primarily reflect higher total factor productivity.[2] Second, it takes time for these gains to materialize: effects become statistically significant two to three years after the reform, as prices start dropping, and productivity and output increase significantly. Third, there is no evidence of any short-term cost from reform, including under weak macroeconomic conditions.

The paper relates to the empirical literature on the economic impact of product market reforms. A strand of studies using country-time or country-time-industry panel data documented a significant positive 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; Inklaar et al., 2008; Nicoletti and Scarpetta, 2003). However, these studies typically do not control for country-specific macroeconomic shocks, and rely at best on internal instruments such as GMM, rather than plausible exogenous instruments, to address omitted variable bias more broadly. Some more recent studies seek to partly address omitted variable bias by estimating the indirect impact of product market reform in non-manufacturing 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" 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). We

[2] In countries with stringent employment protection legislation at the time of the reform, product market liberalization is however found to boost net sectoral job creation, consistent with existing theory and previous cross-country time-series evidence (see e.g. Fiori et al., 2012).

focus here on the more straightforward, direct impact of reforms on the deregulated non-manufacturing industries themselves, also using more disaggregated sector-level data than in previous papers. Country-specific studies of deregulation in specific industries have documented positive effects in the area of retail trade using regional or state-level data (Bertrand and Kramartz , 2000; Skuterud, 2005). The paper also bears some connection to the voluminous firm-level literature on the impact of competitive pressures on innovation, technology adoption, and productivity (e.g. Aghion et al., 2004, 2009b; Gal and Hijzen, 2016). Our focus here is on the overall (dynamic) economic outcomes of deregulation at the aggregate sector level.

Existing studies typically focus on the long-term impact of reform, or capture their dynamic effects in a crude way through simple autoregressive-distributed lag specifications. None explores the dynamic response of outcomes to reform shocks, a key focus of our paper. Recent attempts in this direction include Bouis et al. (2012) on cross-country time-series data, and Dabla-Norris et al. (2015) in a country-sector-time panel of advanced economies. However, these studies either cannot or do not control for country-wide macroeconomic shocks, and do not address omitted variable bias more generally. Yet, as we show below, there is some evidence that reforms are indeed endogenous to outcomes. Finally, while these papers identified large reform shocks based on large declines in the values of OECD indicators of product market regulation, our paper relies on a new database that explicitly maps reform shocks to actual regulatory and legislative measures.

The remainder of this paper is structured as follows. Section II describes the dataset and the methodology used for the construction of reform shocks 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 III describes the empirical setup. Section IV presents the econometric results. Section V concludes.

II. 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 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 shocks in each of these industries for 26 countries over the past four decades assembled by Duval et al. (2016). 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 reform shocks before presenting some descriptive statistics.

A. Data Sources and Identification of Reform Shocks

STAN database

The OECD Structural Analysis (STAN) database provides annual information on sector-level inputs, output, and prices (value-added deflators) 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, 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 variables are expressed in natural logarithm.[6]

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.

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[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.

[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.

Identification of reform shocks

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. (2016). They identify reform shocks 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 of the following three alternative criteria has to be met: (i) 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; (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 policy action underpinning the change in the indicator.

A major advantage of this approach is to document the precise nature and identify the exact timing of major legislative and regulatory actions taken by advanced economies since the early 1970s in the area of barriers to entry. 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 shocks, three (air transport, postal and courier services, telecommunications) can be readily mapped to the three corresponding STAN industries (see Table B of the appendix). For each of these three sectors, the reform shock 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 shocks 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 shocks to construct the summary "electricity and gas" reform shock variable, and weighted averages of road and rail transport reform shocks to construct the "rail and road transport" reform shock 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 234 reform shocks: 60 in electricity and gas, 52 in rail- and road transport, 36 in air transport, 32 in postal services, and 54 in telecommunications.

Additional sources

For instrumentation purposes, we identify all past competition-relevant EU directives covering any of our network industries, which in turn are drawn from a comprehensive historical list of all single market directives available on the European Commission website (http://ec.europa.eu/internal_market/score/docs/relateddocs/20150414/im-directive_en.pdf). Finally, country-specific macroeconomic variables (real GDP and GDP deflator) come from the World Bank WDI and IMF WEO databases.

Final dataset

Using all the sources listed above, we obtain a country-sector-time panel dataset covering 26 countries[8] 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. Finally, in our baseline regressions we drop any country-sector time series for which the dependent variable is not available for at least three years preceding the first liberalization shock. This condition is imposed to assure the presence in the estimation sample of a pre-shock period with which the post-shock period can be compared to. Imposing this condition eliminates approximately 10% of the dataset, but does not qualitatively change the results.

B. Stylized Facts

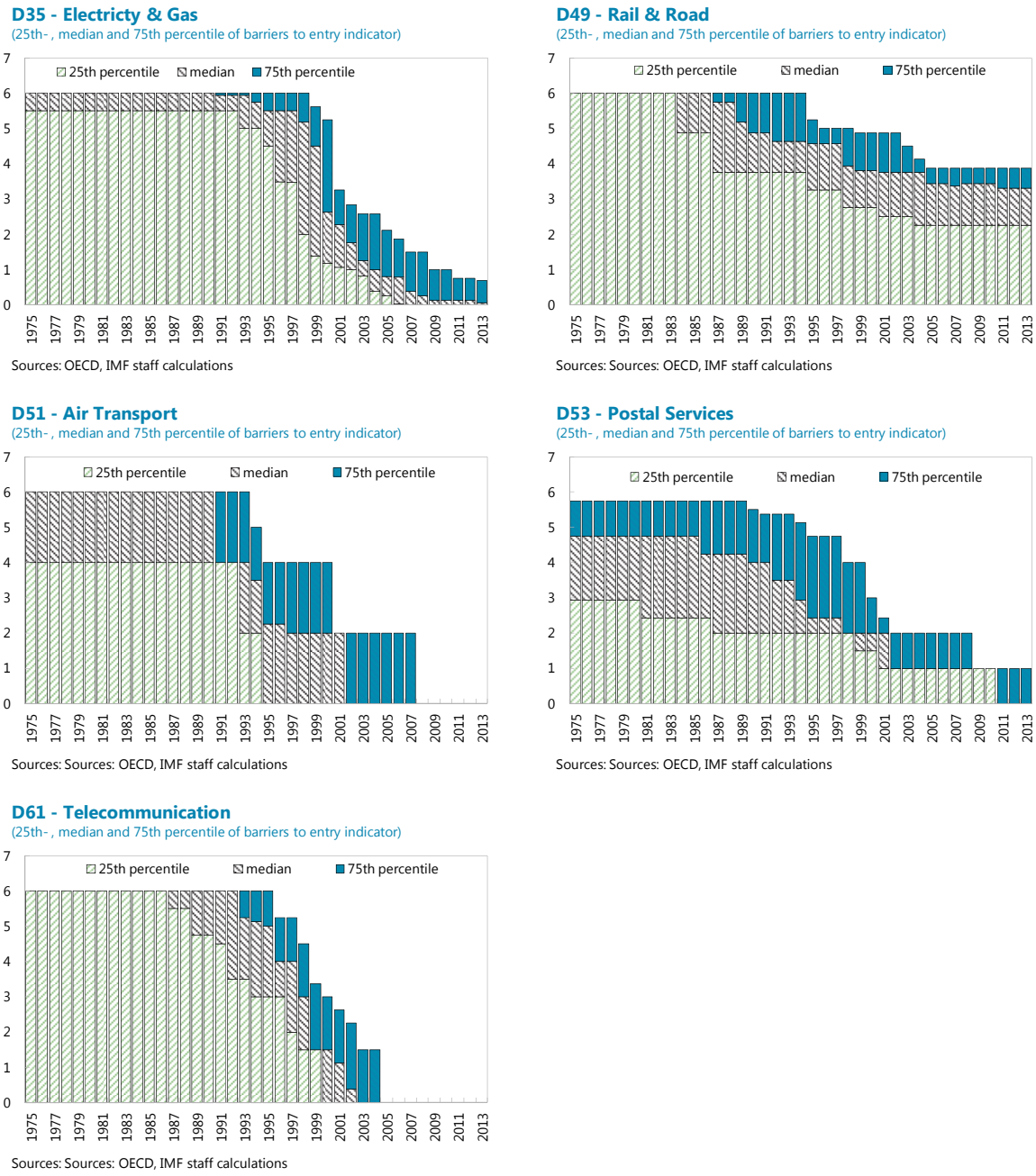
Before proceeding to the formal econometric analysis, we first provide a few stylized facts regarding our dataset that already hint at some of our key results. We start with a brief historical overview of deregulation in barriers to entry in our five network industries, before presenting simple difference-in-differences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

Figure 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

[8] 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.

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.

Figure 1. Barriers to Entry in Network Industries

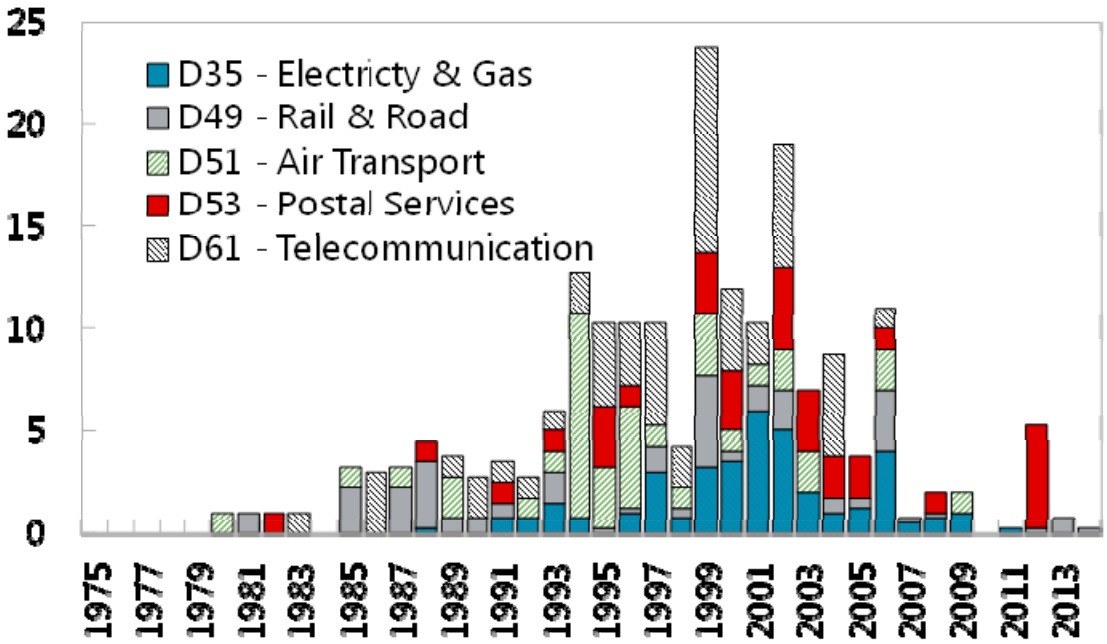


Source: OECD regulatory database, and authors' calculations. The chart shows the evolution over time of the stringency (on a 0-6 scale) and cross-country dispersion (median, 25th and 75th percentiles) of barriers to entry in each of the five network industries.

Figure 2 shows the distribution of our identified shocks 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 the late 1970s and 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 90s saw a fairly synchronized liberalization of the air transport sector, with 10 identified reforms in 1993 alone 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.

Figure 2. Histogram of Barriers to Entry Reforms

Distribution across sectors and over time of major reductions in barriers to entry



Source: 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..

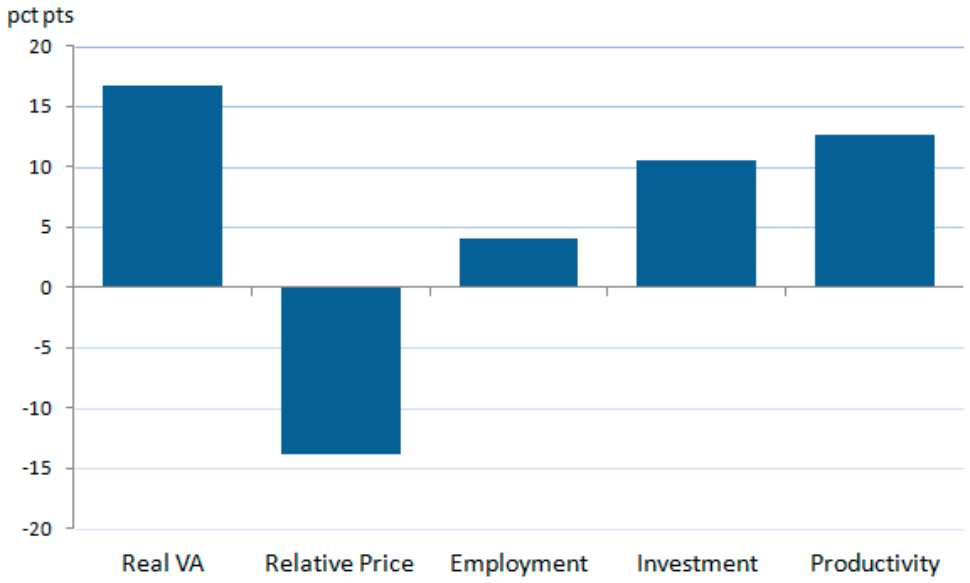
As a first pass on the effects of these major reforms, we examine the evolution of sector-level output and inputs over a 10-year window centered around the year of the reform. For each of the five sectors, we compute a difference-in-differences measure of the average impact of reform on each variable 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:

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

Figure 3 shows the average value of this "difference-in-differences" measure across all reform shocks and sectors in our sample. Given the illustrative nature of the exercise, 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 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). Relative to non-reforming country-sector observations, real value added increases by more than 16 per cent 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.

Figure 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.

III. 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 bias through instrumental variable (IV) estimation

A. 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 (2015) 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 shock variable at time t, a set of fixed effects and additional controls. Specifically, for j = 0, 1, …, 5, we estimate:

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

where y is the variable of interest, *reform* is the reform shock variable, and *controls* include all past and future reform shocks in order to address any omitted variable bias arising from any auto-correlation of reform shocks. 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 shocks—through country-time 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, we use an industry-specific time trend rather than industry-year fixed effects, as the latter would absorb some of the impact of product market deregulation. Therefore we do not include them and control instead for industry-specific time trends in order to capture heterogeneity in trends across sectors (e.g., the faster trend growth of the telecommunications sector compared with the electricity and gas sector in advanced economies). Standard errors are clustered at the country-sector level.

B. Instrumental Variables Estimation

Equation (2) may 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 shock variable. In order to address these, we use instrumental variables techniques, considering three broad types of instruments that capture the scope for reforms, peer pressure from reforms in other countries and, for EU countries, other external pressure stemming from EU directives in network industries:

- 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. In order to measure the initial stance of regulation, we use the three-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 a large number of other countries is carrying or has just carried out reform.

- External pressure stemming from the adoption of competition-relevant EU directives in the sector considered is captured by a sector time-varying dummy variable that takes value 1 if an EU directive for the sector considered was adopted over the past five years (period t-4 to t) *and* has not yet resulted in a reform in the country considered. This is meant to capture the fact that EU countries are typically given a few years to transpose an EU directive into national law—the so-called transposition delay. While this instrument should be primarily relevant for EU countries, which dominate our sample, their adoption might also influence reform patterns in non-EU countries as well.

All three instruments can be considered as exogenous to a country-sector's present and future outcomes, and 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 should not have any effect on the latter other than through pressure on domestic authorities to undertake reform. The same holds true for EU directives.

Because our variable of interest is a bounded indicator variable, equation (2) belongs to the family of endogenous dummy variable models (Heckman, 1978). Instead of a traditional 2SLS approach, we follow Wooldridge's suggested two-step procedure, which is suited for our model (Woodridge, 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.[9] The second step involves a standard IV estimation of equation (2) using the predicted value of our first-step probit model as an

[9] 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

(continued…)

instrument for the reform shock 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.

IV. Econometric Results

This section presents the baseline OLS and IV regression results. An extension assesses the extent to which the estimated impact of reform varies across the business cycle, and also explores interactions between product market reform and the stringency of employment protection legislation.

A. Baseline OLS Results

Table 1 shows the baseline OLS results for real value-added in the deregulated industry over the five years following a major reform. The estimated coefficient is positive but statistically insignificant upon impact, namely in the reform year. Its value then gradually increases over time, exceeding 7 percent and becoming statistically significant at the 10% level after four years. After five years, the cumulative effect is about 10% and is statistically significant at the 5% level.[10]

Table 1. Baseline OLS Regression: Effect of Reform Shock on Real Value Added

Effect of Reform Shock on **Real Value Added**					
	(1)	(2)	(3)	(4)	(5)
	t	t+1	t+2	t+3	t+4
Reform Shock	0.62	2.98	4.54	7.31*	10.09**
	(1.36)	(2.21)	(3.20)	(3.69)	(4.66)
Country-Year FE	Yes	Yes	Yes	Yes	Yes
Country-Ind. FE	Yes	Yes	Yes	Yes	Yes
Ind.-specific Trend	Yes	Yes	Yes	Yes	Yes
N	2269	2181	2093	2005	1917

The dependent variable is the log difference of real value added between year t-1, the year preceding the shock, and year t+j. Cluster robust standard errors are reported in parentheses. *,**, and *** denote statistical significance at the 10, 5, and 1% levels, respectively.

The estimated impact of reform on other sector-level outcomes is shown visually in Figure 4 in the form of impulse response functions together with their 90% and 95% confidence

sample also enables us to include all fixed effects featured in equation (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.

[10] Extending this table beyond year five (t+4) would show the estimated effect leveling off starting from year six.

intervals. As expected, the relative value-added deflator of the deregulated sector (with respect to the aggregate GDP deflator) falls markedly and rapidly following a major reduction in barriers to entry. This price decline is significant already in the second year after the reform, and levels off at about 8-9% after three years. The bottom two graphs in Figure 4, breaks the effect on value added into an employment component and a productivity component.[11] While both employment and labor productivity react in a similar manner, only the latter eventually shows a positive, statistically significant effect after five years.[12] Combined with the absence of any statistically significant effect on gross fixed capital formation (not reported here and in the remainder of this paper given that estimates typically fluctuate around zero with large standard errors), these results suggest that the real value-added gains from reform primarily reflect an increase in total factor productivity. It is also noteworthy that deregulation does not appear to reduce employment in the short term, possibly reflecting that hiring by new entrants offsets any downsizing or exit of incumbents.

Figure 4. Baseline OLS Regressions – Impulse Response Functions

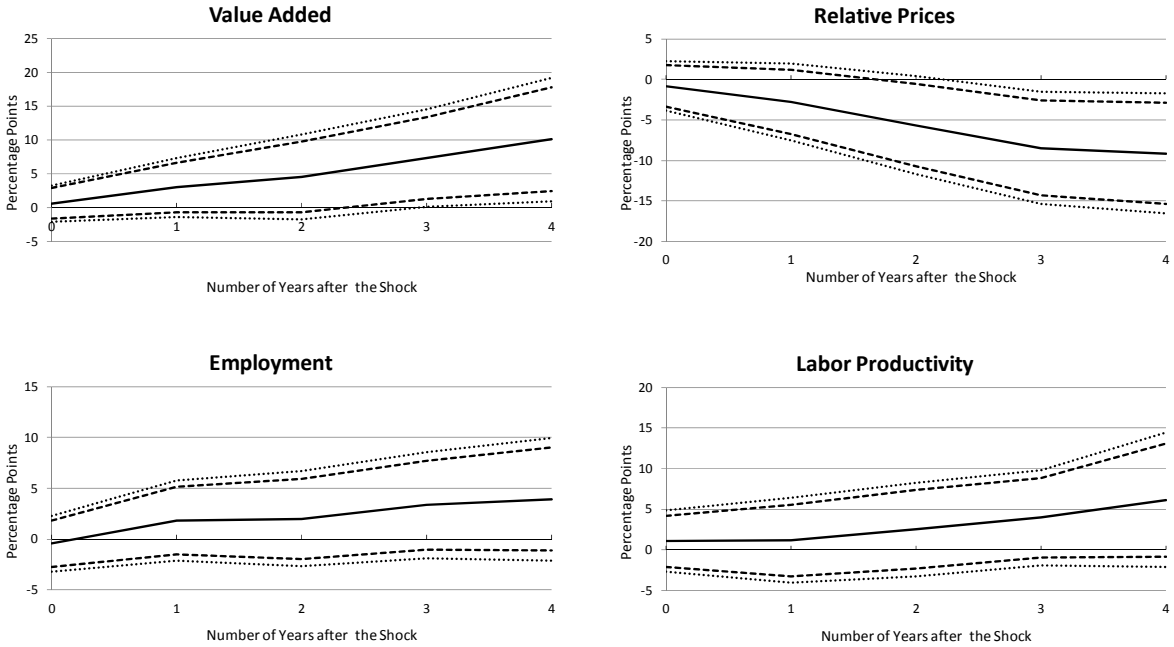


Figure 4: Impulse response functions for baseline regression of the log difference in the corresponding dependent variable between t-1, the year preceding the shock, and t+j. The dashed- and dotted lines show 90- and 95 percent confidence intervals.

The estimates above give us an estimate of the average impact of the average historical major reform. As a complement, in order to get a better sense of the magnitude of the estimated real value-added effect, Table 2 reports OLS estimates where the reform dummy is interacted with the change in the corresponding OECD indicator—which measures the stringency of

[11] This decomposition follows readily from defining labor productivity as the ratio of real value-added to total employment.

[12] It is noteworthy that a reduction of barriers to entry does not appear to lead to a short-term reduction in employment, which suggests that hiring by new entrants can fully offset any downsizing of incumbents.

barriers to entry regulation in the sector considered on a 0 to 6 scale— over the same year. The results suggest that the effects estimated with the dummy indicator are comparable to the effect of a liberalization roughly equivalent to a two-point reduction in the value of the ETCR indicator—the point estimate implies that such a reduction increases sectoral output by about 11 (=5.5*2) percent over 5 years, with about three fourths of the impact coming from improved labor productivity and the remainder from an increase in employment. The cumulative effect on relative prices is also fairly similar to that obtained when using the dummy indicator above. However, the statistical significance of the estimates is reduced, in line with the notion that the narrative approach we use to identify major reform shocks reduces measurement error—particularly as regards the timing of reform—relative to an approach that would only rely on changes in OECD indicators.

Table 2. Effect on Real Value Added Accounting for the Size of the Reform Shock

Effect of Reform Shock on **Real Value-Added**

	(1)	(2)	(3)	(4)	(5)
	t	t+1	t+2	t+3	t+4
Reform Shock x Change	0.42	2.05*	2.97*	3.94**	5.46**
	(0.83)	(1.16)	(1.53)	(1.66)	(2.22)
Country-Year FE	Yes	Yes	Yes	Yes	Yes
Country-Ind. FE	Yes	Yes	Yes	Yes	Yes
Ind.-specific Trend	Yes	Yes	Yes	Yes	Yes
N	2264	2175	2086	1997	1908

The dependent variable is the log difference of real value added between year t-1, the year preceding the shock, and year t+j. Cluster robust standard errors are reported in parentheses.
*,**, and *** denote statistical significance at the 10, 5, and 1% levels, respectively.

B. Instrumental Variable Results

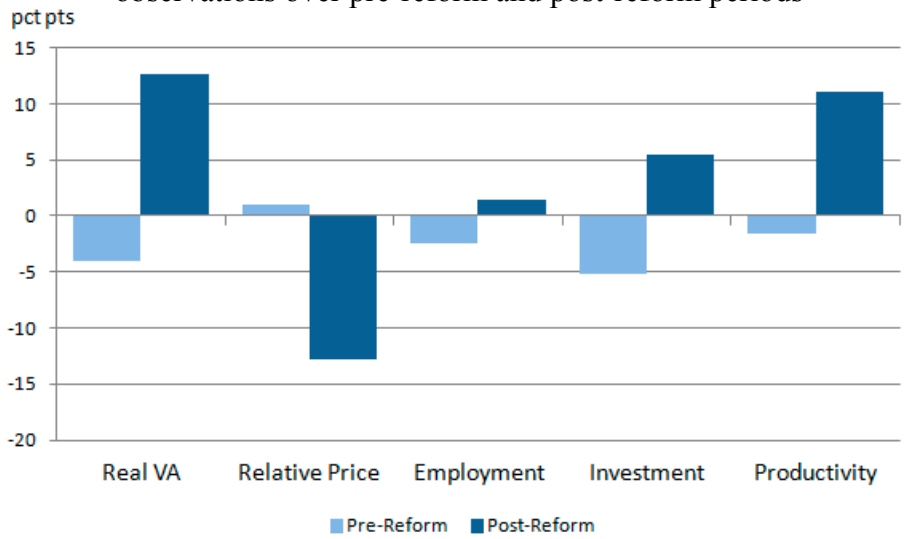
As an illustration of the need to instrument reform shocks, Figure 5 breaks the "difference-in-differences" measure shown in Figure 3 into a pre-reform difference and a post-reform difference, i.e. using the notations introduced in section II.B, respectively:

$$\left(X_{R-5,R}^{REFORM} - X_{R-5,R}^{NO\ REFORM} \right) \text{ and } \left(X_{R,R+5}^{REFORM} - X_{R,R+5}^{NO\ REFORM} \right)$$

Compared to the post-reform differences between reforming and non-reforming countries, the pre-reform differences are relatively marginal for the growth rates in real value added, prices, and labor productivity, pointing to no systematic difference between reforming and non-reforming units along these dimensions. This is however much less true for more forward-looking variables such as investment and employment. Reforming observations exhibit substantially lower investment and employment growth compared to their non-reforming counterparts. Insofar as lower investment growth signals poorer growth prospects, reforming observations may well experience systematically weaker growth outcomes than non-reforming observations in the absence of a reform. This would imply that our baseline OLS results underestimate the true impact of a reduction in barriers to entry.

Figure 5. Comparison of Reforming and Non-reforming Observations over Pre-reform and Post-reform Periods

Difference in cumulative growth of selected variables between reforming and non-reforming observations over pre-reform and post-reform periods



Average differences (in percentage points) of cumulative 5-year growth of "Reform" versus "No Reform" observations, over [R-5; R] and [R; R+5] respectively for the Pre-Reform and for the Post-Reform periods, where R is the year a major reform is observed in a particular country/sector (using joint reforms in case of "rail and road" and "electricity and gas"). The "Reform" observations are country/sector observations which experienced a major reform in year R. The "No Reform" observations are observations of the same sector as the "Reform" observations in a different country but which did not experience any major reform over a 10-year window surrounding year R.

As explained in more detail in the previous section, we address endogeneity concerns by instrumenting our reform shock with three country-sector-level instruments—the lagged regulatory stance, the number of other reforming countries in the sector considered over the past three years, and the existence of an EU Directive that has not yet been transposed into national law—using an ordered probit to explain the occurrence of a reform. Following Wooldridge's two-step procedure, the predicted value from this probit model is then used as an instrument in the first stage of the 2SLS procedure, rather than directly as a regressor in the second stage.

Table 3 shows the IV results.[13] As expected, while the dynamics of the impact of reform is qualitatively similar to that estimated with OLS, its magnitude is substantially larger. Statistical significance is also increased despite the less precise estimation due to the first step. Point estimates peak at almost 14 percent after five years. Similar to the baseline OLS regression, the estimated coefficient levels off afterward.

[13] These F-statistics should not be interpreted applying conventional "rules of thumb", such as for instance Staiger and Stock's (1997), that are often 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 three instruments. Also, although three 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.

Table 3. IV Regression: Effect of Reform Shock on Real Value Added

Effect of Reform Shock on **Real Value Added**

	(1)	(2)	(3)	(4)	(5)
	t	t+1	t+2	t+3	t+4
Reform Shock	-1.13	2.09	3.46	9.26**	13.66***
	(1.86)	(2.98)	(3.84)	(4.00)	(5.08)
N	2264	2181	2093	2005	1917
1st Stage F	243.54	228.05	205.49	177.80	206.74

The dependent variable is the log difference of real value added between year t-1, the year preceding the shock, and year t+j. Cluster robust standard errors are reported in parentheses. *,**, and *** denote statistical significance at the 10, 5, and 1% levels, respectively.

The impulse responses of other variables are shown in Figure 6. These are again qualitatively similar to those estimated with OLS, although point estimates and significance levels change somewhat. The reduction of relative prices is of a similar magnitude, though statistical significance is now somewhat reduced in the outer years. In contrast, the effects of reform on employment and labor productivity are slightly amplified, both economically and statistically. Their relative contribution to value-added gains remains unchanged: the labor productivity effect of deregulation is larger and more statistically significant than the employment effect. Finally, while the estimated impact of reform on employment is negative in the first year, it is not statistically significant. There is therefore no evidence of short-term employment costs from reductions in barriers to entry in network industries, at odds with Bassanini (2015) who uses more aggregate data (KLEMS) and a somewhat different approach.

Figure 6. IV Regressions – Impulse Response Functions

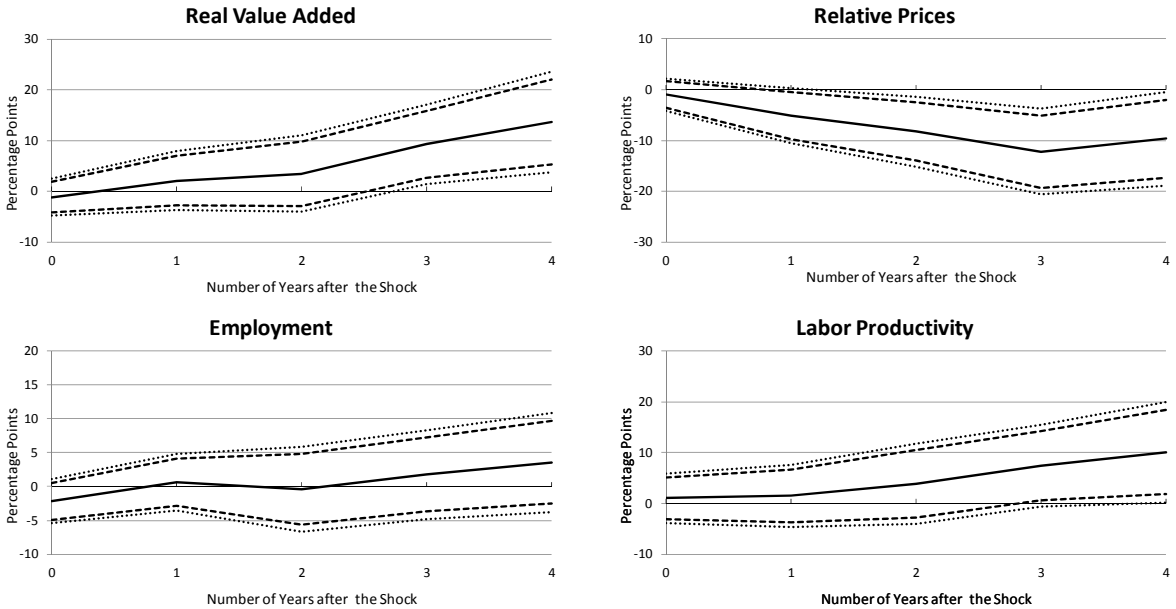


Figure 6. Impulse response functions for IV estimation, where the first stage uses the predicted probability of a reform from ordered probit as instrument, and the second stage explains the log difference of real value added between t-1, the year preceding the shock, and t+j. The dashed and dotted lines show 90 and 95 percent confidence intervals.

C. Interactions

As an extension, we assess whether the effects of product market liberalization vary depending on the reform environment including overall business conditions and other institutional settings.

One open issue is whether a decline in barriers to entry delivers weaker short-term gains when implemented in bad times rather than in good times. Cacciatore et al. (2016b) show that this is ambiguous *a priori*. On the one hand, for prospective entrants facing sunk investment costs, the net present value of post-entry profits is lower in recessions. On the other hand, insofar as recessions drive some firms out of business, mark-ups may be higher, thereby increasing incentives for entry. To test for this conjecture, we interact the reform shock variable with the measure of business conditions proposed by Auerbach and Gorodnichenko (2012), namely a smooth transition function of the economy's aggregate GDP growth rate that tends toward 1 in recessions and toward 0 in expansions.[14] Results (not reported) do not show any significant impact of the business cycle position on the effect of entry deregulation—a finding in line with cross-country time-series evidence in IMF (2016) and Bordon et al. (2016).

Previous theory and empirical evidence also suggest that the stringency of employment legislation can shape the effect of product market reforms (e.g. Blanchard and Giavazzi, 2003; Fiori et al., 2012). The intuition is that where employment protection legislation is more stringent, real wages are more likely to exceed levels that clear the labor market, and labor demand and employment to be below the full-employment level. In such countries, there is greater potential for product market reform to deliver job gains. Table 4 reports estimates obtained when interacting the reform variable with the (pre-reform, i.e. t-1) value of the OECD indicator of the stringency of employment protection legislation for regular contracts (EPLR)—which is measured on a 0 to 6 scale. The results suggest that the employment effect of entry deregulation is indeed larger where employment protection regulation is more stringent. Also consistent with theory, this positive interaction appears to capture a permanent effect as it remains statistically significant beyond the fifth year following the reform (not shown).

[14] This measure is defined as:

$$F(z_t) = \frac{\exp(-\gamma z_t)}{1+\exp(-\gamma z_t)},$$

where z is the standardized (zero-mean and unit-variance) annual growth rate of real GDP and γ is a parameter set to 1.5 as in Auerbach and Gorodnichenko (2012).

Table 4. Effect of Reform Shock on Real Value Added: The Role of Employment Protection Legislation

Effect of Reform Shock on **Value-Added**

	(1)	(2)	(3)	(4)	(5)
	t	t+1	t+2	t+3	t+4
Reform Shock	-0.13	2.60	2.44	4.00	4.84
	(1.48)	(2.08)	(2.27)	(2.51)	(2.92)
Reform Shock × EPLR	4.28	10.66**	7.13	11.16	16.39**
	(3.39)	(5.11)	(6.35)	(6.92)	(7.67)
Country-Year FE	Yes	Yes	Yes	Yes	Yes
Country-Ind. FE	Yes	Yes	Yes	Yes	Yes
Ind. Trend	Yes	Yes	Yes	Yes	Yes
N	2321	2245	2152	2059	1966

The dependent variable is the log difference of real value added between year t-1, the year preceding the shock, and year t+j. Cluster robust standard errors are reported in parentheses.

*,**, and *** denote statistical significance at the 10, 5, and 1% levels, respectively.

V. CONCLUDING REMARKS

The paper has reassessed the growth 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 new annual data on 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 a sizeable average impact of major past reforms on real value added in the deregulated industries, in the order of 10% to 14% after five years depending on the specifications. This effect is concomitant with an increase in labor productivity and a relative price decline. By contrast, the estimated impact of deregulation on sectoral employment is weak, and investment is essentially unaffected, suggesting that output gains primarily reflect higher total factor productivity. In line with theory, the results also tentatively support a positive short-term impact on net job creation in the deregulated sector in countries where employment protection legislation is more restrictive. Finally, although it takes several years for the benefits from reform to materialize, there are no short-term costs, and no evidence that gains could be weaker when governments deregulate in times of weak overall demand conditions. These findings provide a clear case for intensifying product market reform efforts in advanced economies at the current juncture of weak growth.

Appendix

A. 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. 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.	
ETCR	OECD indicators of regulation in energy, transport and communications (ETCR) summarising 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.

B. Sector Correspondence

OECD ETCR indicator	Production, value added, gross fixed capital formation, and employment data (STAN)
Gas (production/import, transmission, supply).	D35 – Electricity, gas, steam and air conditioning supply.
Electricity (generation, transmission, distribution, supply).	
Railways (passenger and freight transport, operation of infrastructure).	D49 – Land transport and transport via pipelines.
Road transport (freight).	
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.

Sources: Conway and Nicoletti (2006) and United Nations Statistics Division ISIC Rev.4 structure.

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