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Can structural reforms unleash private investment?

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Abstract

This paper examines the impact of labour and product market reforms on private investment across 26 advanced economies from 1975 to 2020. To this end, we combine a narrative database on major structural reforms with local projections and augmented inverse probability weighting. We find that a major labour market reform typically increases the level of real private investment by 5% cumulatively within six years, while a major product market reform yields an impact of 3%. Structural reforms are particularly effective in stimulating private investment when the private sector has access to ample external finance and the rule of law is strong. Overall, our findings point to an important role for structural reforms in boosting private investment and thus long-term growth prospects in advanced economies.

Keywords: structural reforms, investment, product market reforms, labour market reforms

JEL Classification: E22, O43, L51

Non-technical summary

Private investment growth has slowed significantly in many advanced economies over the past decade, despite major investment needs related to technological shifts and the transition to a low-carbon economy. International organisations like the IMF and OECD have long promoted structural reforms as a way to boost private investment and support long-term economic growth. Yet, robust empirical evidence directly linking structural reforms to private investment remains surprisingly scarce. This study aims to address this gap. Using data from 26 OECD countries over the period 1975 to 2020 and state-of-the-art empirical methods, we analyse how major labour and product market reforms affect private investment over time.

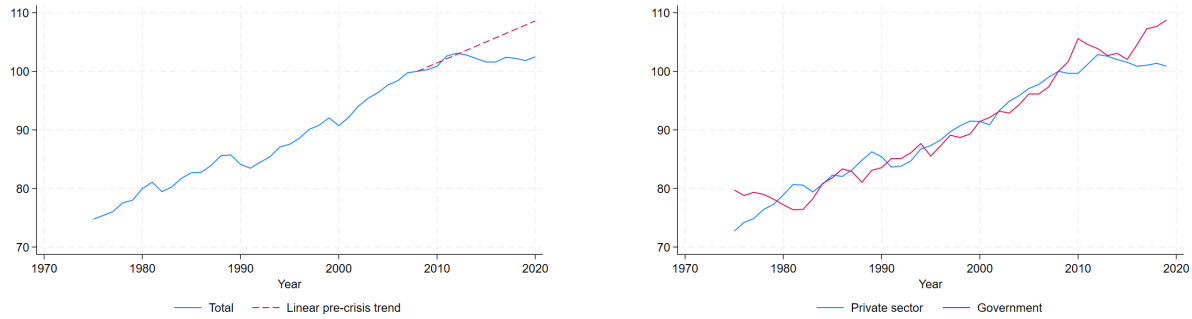
Our findings indicate that both labour and product market reforms can significantly boost private investment. A major labour market reform typically increases the level of real private investment by 5% cumulatively within six years, while a major product market reform yields an impact of 3%. Product market reforms thus tend to have smaller positive effects, possibly because they are often focused on specific network industries while labour market reforms are more likely to affect the wider economy. For both types of reform the benefits build up gradually and take around three years to become statistically significant. We also show that structural reforms are most effective in boosting private investment in countries with strong institutional frameworks and well-functioning financial systems, which provide the stability and financial resources needed to invest.

These results carry important policy implications. First, our estimates underscore the potential of well-designed labour and product market reforms to revive private investment and enhance long-term growth prospects in advanced economies. Unfolding this potential will however require a step-up in reform efforts, considering the scarcity of major labour and product market reforms in advanced economies over recent years. Second, our findings emphasise the critical role of institutional quality and access to finance in shaping reform outcomes. Insofar, efforts to improve institutional settings and the workings of financial markets – including through further cross-border integration – should not be seen as substitutes for, but rather as complements and critical enablers of successful labour and product market reforms.

1 Introduction

In the aftermath of the global financial crisis, many advanced economies experienced persistently weak investment dynamics relative to pre-crisis trends, mainly reflecting subdued private sector investment (Figure 1). At the same time, ongoing technological transformations—such as the diffusion of artificial intelligence—and the transition to a low-carbon economy entail substantial private investment needs. Improving the conditions that support private investment therefore remains a key policy priority in the current economic landscape. Against this backdrop, international policy institutions—including the International Monetary Fund, the OECD, and the European Commission—have frequently promoted structural reforms in product and labour markets to strengthen incentives to invest and innovate and enhance long-term growth prospects.

Figure 1: (Log) real investment in advanced economies



(a) Total investment

(b) Private and public investment

Sources: IMF, AMECO, own illustration. *Note:* The charts are based on median investment growth rates in a sample of 26 OECD countries, transformed into log level indices with base year 2008. The linear trend reflects the slope of a univariate regression based on data for the period 1975-2008.

Yet, empirical evidence directly linking structural reforms to private investment remains surprisingly scarce. To fill this gap, this paper examines the extent to which structural reforms in product and labour markets stimulate private investment and under which economic conditions such reforms are most effective. Using data for 26 OECD countries over the period 1975–2020, we study the dynamic effects of major reform episodes on private investment. To identify these episodes, we leverage the narrative structural reform database developed by Duval et al. (2018) and updated by Wiese, Jalles and de Haan (2024), which provides detailed information on major product and labour market reforms in OECD countries.

Methodologically, a key concern in our analysis relates to endogeneity: the incidence of structural reforms may not be random and may depend on macroeconomic conditions, which in turn may also affect private investment. In particular, reforms are often implemented in response to economic crises (Pitlik and Wirth, 2003; Agnello et al., 2015; Dias da Silva, Givone and Sondermann, 2017; de Haan and Parlevliet, 2018; Duval, Furceri and Miethe, 2021). In principle, this issue could be resolved by including the confounders as controls in a standard local projection framework. However, the covariates might affect the incidence of reforms in a non-linear and potentially unknown way, thereby undermining the "selection on observables" assumption underpinning causal identification (Jordà and Taylor, 2025).

To address this challenge, we first document the non-random nature of reform implementation in our sample. We then employ the local projection augmented inverse probability weighting (LP-AIPW) estimator proposed by Jordà and Taylor (2016). The approach combines two elements: a model that estimates the probability that a reform occurs given observable economic and political conditions, and a model that relates private investment to structural reforms and various control variables. In practice, reform and non-reform episodes are re-weighted based on their predicted probability—i.e. the likelihood that a reform would have been implemented, or not, given those conditions—such that the distribution of reforms in the sample becomes quasi-random. This helps to mitigate selection bias arising from observable factors that influence (possibly non-linearly) when reforms are undertaken and improves the credibility of the estimated causal effects.

Our econometric analysis reveals that major labour market reforms typically increase the level of real private investment by 5% cumulatively within six years, while major product market reforms yield an impact of 3%. The effects build up gradually, becoming statistically signifiant after three years. To better understand the conditions under which these effects materialise, we further examine heterogeneity of the impact under selected key structural characteristics of the economy at the time of reform implementation. We find that the investment response to structural reforms is significantly stronger in economies with higher institutional quality and access to ample external financing, which together offer the private sector the economic stability and financial resources to confidently take long-term investment decisions.

Our paper contributes to the literature in two main ways. First, by studying the macro-level impact of major structural reforms on private investment using the narrative structural reform database developed by Duval et al. (2018) and updated by Wiese, Jalles and de Haan (2024),

combined with local projections and an augmented inverse probability weighting, we provide new empirical evidence on the dynamic effects of structural reforms on investment based on recent methodological advances. Second, by examining how the investment response to reforms varies with key structural characteristics, the paper sheds light on the conditions under which such reforms are most effective.

The remainder of the paper is organised as follows. Section 2 provides an overview of the relevant literature and outlines channels through which structural reforms may stimulate private investment. Section 3 explains the data and methodology used in our econometric analysis. Section 4 presents the results of our baseline regressions and explores the conditions under which structural reforms are particularly effective. Section 5 discusses various robustness checks. Finally, Section 6 summarises our key findings and outlines policy implications.

2 Literature overview

Economic theory provides different channels through which structural reforms may stimulate private investment. Product market reforms, for instance, can encourage investment by lowering entry barriers, reducing markups and decreasing administrative costs associated with firm entry and expansion (Alesina et al., 2005). This is because in less competitive markets, firms may limit investment in additional capacity in order to preserve monopoly rents on existing production. Moreover, barriers to entry and expansion may discourage investment and firm creation, while stronger competition may encourage incumbent firms—particularly those close to the technological frontier—to invest and innovate in order to maintain their competitive advantage (Aghion et al., 2005).

Labour market reforms may likewise support investment by reducing labour adjustment costs and increasing wage flexibility, thereby lowering the expected costs and risks associated with expanding production capacity. More broadly, labour market institutions can influence human capital accumulation, technological adoption and the reallocation of resources across firms, all of which can raise the marginal product of capital and thus encourage investment (Bentolila and Bertola, 1990; Barro and Sala-i-Martin, 1997).

A growing strand of literature analyses the macroeconomic impact of structural reforms from an empirical viewpoint. Related reduced-form studies have predominantly focused on output (e.g. Mavrogiannis and Tagkalakis, 2025*b*), productivity (e.g. Nicoletti and Scarpetta,

2014; Duran-Franch et al., 2026), employment (e.g. Rünstler, 2024), income distribution (e.g. Wiese, Jalles and de Haan, 2024), fiscal policy (e.g. Chrysanthakopoulos, Mavrogiannis and Tagkalakis, 2025) and external balances (e.g. Duval, Furceri and Tovar Jalles, 2022; Mavrogiannis and Tagkalakis, 2025*a*).

By contrast, the empirical literature studying the impact of structural reforms on private investment remains relatively scarce. In an early contribution, Alesina et al. (2005) exploit OECD sectoral regulation indicators and show that tighter product market regulation—particularly barriers to entry—is associated with significantly lower investment, while liberalisation (including entry deregulation and privatisation) raises investment rates in regulated industries. More recently, Gal and Hijzen (2016) used local projections in a firm-level framework to show that product market reforms immediately boost firm-level investment, with the effects reaching around 3% within two years.

Beyond these contributions, a small number of papers report estimates for private investment as a secondary outcome while the main focus of the analysis lies elsewhere. Most notably, combining local projections with a narrative structural reform database, Duval, Furceri and Tovar Jalles (2022) find that product market reforms lead to a statistically significant increase in the investment-to-GDP ratio of about 1 percentage point after two years. Labour market reforms, by contrast, are associated with a decline in the investment-to-GDP ratio of about 0.2 percentage points three years after the reform, although the estimate is not statistically significant. In addition, using a calibrated DSGE model, Cacciatore and Fiori (2016) provide state-dependent estimates under different business-cycle conditions and external borrowing constraints, generally predicting an increase in private investment over the medium term following structural reforms, while the short-run impact may be negative. Finally, Pfeiffer, Varga and in't Veld (2024) show within a structural model with endogenous growth that closing half of the gap vis-à-vis the EU's best-performing Member States across several structural indicators could boost total EU investment by 9% in the long run, while Vogel (2017) finds that structural reforms can increase investment even at the zero lower bound.

While closely related, our analysis differs from these studies in several important respects. Alesina et al. (2005) study the relationship between product market regulation and investment using sector-level regulation indicators and cross-industry variation, focusing exclusively on product market reforms. By contrast, our approach examines the macroeconomic investment response to identified reform episodes based on a narrative structural reform database and

considers both product and labour market reforms.

Compared with Duval, Furceri and Tovar Jalles (2022), who employ the same database to analyse the effects of structural reforms primarily on the current account, our paper places private investment at the centre of the analysis. Methodologically, we combine local projections with augmented inverse probability weighting, which explicitly addresses the potentially non-random occurrence of reforms and allows us to obtain doubly robust estimates of their dynamic effects. In addition, we extend the analysis by examining how the investment response to structural reforms depends on key structural characteristics of the economy. This allows us to shed light not only on whether structural reforms stimulate private investment, but also on the economic environments in which such reforms are most effective. The remaining studies differ more fundamentally from our approach, either in terms of the outcome variables considered or the empirical framework employed.

3 Empirical approach

3.1 Data

Our primary estimation sample consists of annual data for 26 OECD countries spanning the period 1975–2020.[1] Although the reform dataset by Wiese, Jalles and de Haan (2024) begins in 1970, we exclude the first five years from our analysis due to the limited number of reforms and incomplete availability of private investment data for several countries during this period. Data on nominal private investment – measured as gross fixed capital formation (GFCF) in the private sector – are sourced from the IMF and deflated using the GFCF deflator from the AMECO database. Investment figures are expressed in (constant price) local currency terms to abstract from exchange rate effects.

For the measurement of structural reforms, we rely on the narrative structural reform database developed by Duval et al. (2018) and subsequently updated by Wiese, Jalles and de Haan (2024).[2] This dataset focuses exclusively on "major" policy changes, identified based on legislative and regulatory actions that are documented in OECD Economic Surveys and meet at least one of three criteria: (i) the Economic Survey employs strong normative language to

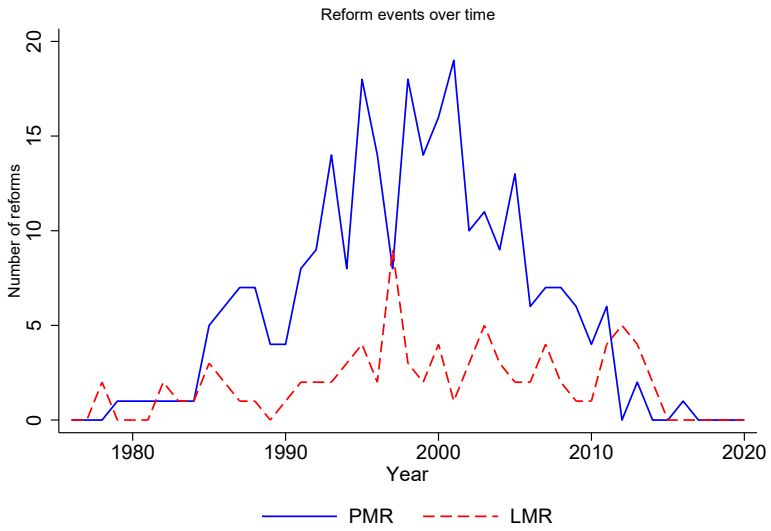
[1] Further details are provided in Appendix A.

[2] Aumond, Di Tommaso and Rünstler (2022) construct a similar quarterly dataset, whose scope is however narrower in terms of country coverage, time horizon and reform categories.

describe the action at the time it was implemented; (ii) the measure is repeatedly highlighted across multiple Economic Surveys; (iii) the corresponding OECD indicator exhibits a substantial change, defined as falling within the 5th percentile of cumulative changes over a three-year horizon.[3] When only the third criterion is met, additional sources are used to verify the policy action.

The narrative approach offers several advantages over methods that identify reforms based solely on large changes in specific indicators. Most importantly, it reduces the risk of misinterpreting noise in the data as a reform, provides greater contextual detail on the nature and timing of policy actions, and captures reforms with gradual effects on policy indicators. A key limitation of this database is however that the reform indicators are binary and thus do not offer quantitative information on the intensity of major reforms. This is a source of treatment heterogeneity in our analysis, although the focus on "major" reforms alleviates this concern.

Figure 2: Major structural reforms by policy area



Sources: Database developed by Duval et al. (2018) and updated by Wiese, Jalles and de Haan (2024), own illustration.

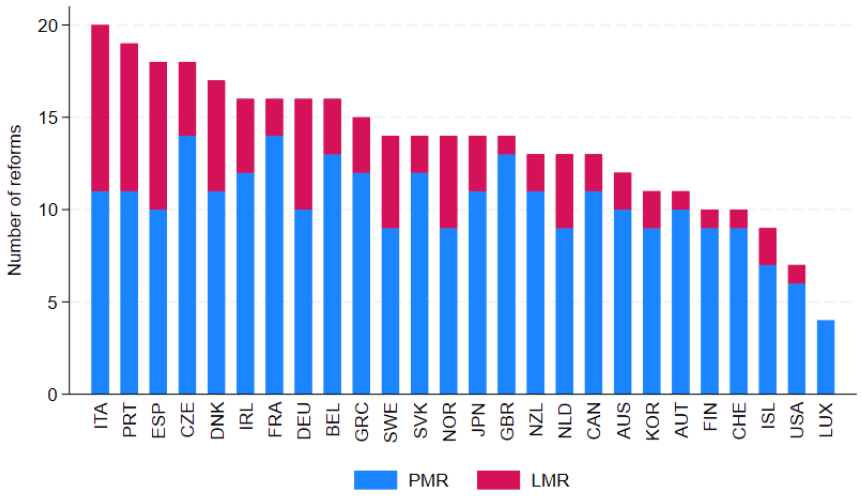
The database covers two primary categories of structural reforms: labour market reforms (LMRs) and product market reforms (PMRs). Labour market reforms include changes to employment protection legislation for regular and temporary workers, the replacement rate and the duration of unemployment benefits. Product market reforms focus on regulatory

[3]The OECD indicators include measures of product market regulation in key industries, employment protection legislation indices for regular and temporary workers, and the average replacement rate of unemployment benefits.

changes in key network industries, including air transport, electricity, gas, postal services, rail transport, road transport and telecommunications. The number of reforms in individual sub-categories is generally too small to allow for a meaningful econometric analysis at a disaggregated level. Therefore, we focus on two aggregate types of reform, i.e. labour and product market reforms, which adds another source of treatment heterogeneity to our analysis.

Our sample includes 354 major structural reforms, of which 267 are related to product markets and 87 to labour markets. As documented by Duval et al. (2018), the majority of these reforms were implemented during the 1990s and 2000s (see Figure 2). This concentration is primarily driven by product market reforms, whereas labour market reforms are more evenly distributed across the sample period. Strikingly, there is a persistent decline in reform activity over the most recent years, with only one major reform recorded between 2015 and 2020. It is also worth noting that our sample includes 49 instances of counter reforms, where previously implemented major reforms were rolled back. Most of these reversals are related to labour market reforms.

Figure 3: Major structural reforms by country and policy area



Sources: Dataset developed by Duval et al. (2018) and updated by Wiese, Jalles and de Haan (2024).

At the country level, the majority of reforms recorded in our sample were implemented by countries that are currently part of the European Union (see Figure 3). However, the overall frequency of reforms–defined as the percentage of country-year observations in which a reform is recorded–is only slightly higher among EU countries compared to non-EU countries. The

differences are more pronounced for labour market reforms, whose frequency is nearly twice as high in EU Member States as in non-EU countries. This disparity may partly reflect greater scope for deregulation among EU countries (Duval et al., 2018). It should also be noted that some product market reforms in EU countries were influenced by the deepening of the Single Market and preparations for accession to the EU (or its predecessors).

Figure 11 in Appendix A provides a detailed overview of reform incidence by country over time, alongside investment dynamics. Additional information on the remaining data sources used in our econometric analysis is also provided in the appendix.

3.2 Methodology

This paper investigates the macro-level effects of major structural reforms on investment. To this end, we employ the Local Projections (LP) method introduced by Jordà (2005), which estimates dynamic treatment effects by directly recovering impulse responses for each time horizon.[4] Our empirical specification is informed by the flexible neoclassical investment model of Hall and Jorgenson (1967), which implies that investment depends on expected demand conditions and the user cost of capital, and by its empirical operationalisation in Lim (2014). Specifically, the baseline specification we use is:

$$\begin{aligned}\Delta i_{j,t+h} \equiv i_{j,t+h} - i_{j,t-1} =& \alpha^h_{1,j} + \alpha^h_{2,t} T_t + \sum_{k=1}^{3} \beta^h_{1,k} \left(i_{j,t-k} - i_{j,t-1-k}\right) + \sum_{l=1}^{6} \beta^h_{2,l} R^{r=r_1}_{j,t+1-l} \\ &+ \beta^h_{3,r} R^{r\neq r_1}_{j,t} + \beta^h_4 X_{j,t-1} + \varepsilon^h_{j,t}.\end{aligned} \tag{1}$$

In this model, $i_{j,t+h} - i_{j,t-1}$ represents the cumulative change in the log of real private investment in country j over time horizon $h = 0, \ldots, 6$. $R^r_{j,t}$ is a binary variable indicating major structural reforms in policy area $r \in \{\text{LMR}, \text{PMR}\}$. To isolate the effect of reforms in a given policy domain from contemporaneous changes in the other, we control for labour market reforms when estimating the impact of product market reforms and vice versa. The model also includes three lags of investment growth which absorb persistence in investment dynamics[5], five lags

[4] Compared to VAR-based approaches, local projection estimators exhibit lower bias in finite samples, although they may result in higher variance at intermediate and longer horizons (Li, Plagborg-Møller and Wolf, 2024; Montiel Olea et al., 2025).

[5] In our baseline estimations, the sum of the coefficients of the lagged dependent variable is below one. Moreover, Piger and Stockwell (2025) show that even for highly persistent processes the long-differenced LP specification substantially reduces, and in many cases even eliminates, the estimation bias from persistence. This is important,

of the treatment variable to control for delayed effects of reforms[6], country fixed effects ($\alpha^h_{1,j}$) and a time trend (T_t). For all lagged variables, the lag length is selected based on the Akaike criterion. The vector of lagged control variables ($X_{j,t-1}$) includes GDP growth, the cost of capital measured by the long-term real interest rate, the output gap (based on an HP-filter), inflation, a global financial crisis dummy, trade openness and a rule of law index. $\varepsilon^h_{j,t}$ represents the error term. We use the Spatial Correlation Consistent (SCC) standard errors as per Driscoll and Kraay (1998) for the simple local projections, as recommended by Inoue, Jordà and Kuersteiner (2026) for a panel setting with $T >> N$.

There is ample evidence that the incidence of reforms is non-random (e.g. Pitlik and Wirth, 2003; Agnello et al., 2015; Dias da Silva, Givone and Sondermann, 2017; de Haan and Parlevliet, 2018; Duval, Furceri and Miethe, 2021). In particular, structural reforms are often introduced in response to severe economic crises or external pressures, factors that may inherently influence investment outcomes. A balance test confirms that reform and non-reform observations in our sample differ systematically in observable characteristics (see Table 2 in the Appendix). Reform episodes are associated with higher real interest rates, more negative output gaps, elevated inflation rates, and – specifically for labour market reforms – lower GDP growth rates. Moreover, these covariates are potentially correlated with investment.

Hence, in our application causal identification cannot rely on reforms being exogenous. In such a case, the literature often resorts to the "selection on observables" assumption to achieve causal identification (Jordà and Taylor, 2025). Conditional on a set of observed covariates, the treatment assignment is assumed to be as good as random with respect to outcomes: $\Delta i_{j,t+h}(R^r_{j,t}) \perp R^r_{j,t}|\widetilde{X}_{j,t}$. Here, $\widetilde{X}_{j,t-1}$ captures all observable covariates from equation (1), including reform-related controls. Depending on the application, including the confounders as controls in a simple local projection can help to plausibly invoke this assumption. However, we cannot exclude that the confounders affect the incidence of reforms non-linearly. Moreover, the functional form of this potential non-linear relationship is unknown.

Therefore, we employ the augmented inverse probability weighting (LP-AIPW) estimator developed by Jordà and Taylor (2016). This approach has increasingly been applied in the recent literature on structural reforms (see de Haan and Wiese, 2022; Wiese, Jalles and de

since Herbst and Johannsen (2024) demonstrate that level-based LP specifications suffer from estimation bias when a relatively short time dimension is coupled with high persistence.

[6] As an additional robustness check, we re-estimated equation (1) using leads of the treatment variable to account for potential future reform shocks. The results remain robust.

Haan, 2024; Mavrogiannis and Tagkalakis, 2025*a*,*b*). It aims to construct a pseudo-population in which (reform) treatment is unrelated to the observed covariates – even in the presence of non-linearities – so that the estimated average treatment effect (ATE) is open to causal interpretation. More specifically, the LP-AIPW estimator conditions on propensity scores that are designed to summarise all relevant confounding information operating through observables. Of course, unobservable factors might still confound our analysis. In practice, anticipation effects are a particular concern in this regard. In our estimation, we therefore control for such anticipation effects through standard proxies while also including fixed effects to capture other unobservables.

In more detail, the LP-AIPW estimator rests on a multi-step correction procedure. First, we estimate propensity scores using a probit model to capture the likelihood of structural reform implementation based on various predictors identified in the literature. Second, the propensity scores are used to assign weights to individual observations in the calculation of the average treatment effect (ATE) of major structural reforms. As noted, this weighting process aims to approximate a quasi-random assignment of reform treatments and control observations.

The first step involves estimating a probit model that predicts the likelihood of major structural reforms – our treatment variable – using a set of economic and political covariates. The model is specified as follows:

$$p_{j,t} = \gamma_{1,j} + \gamma_{2,t} T_t + \sum_{m=1}^{3} \delta_{1,m} g_{j,t-m} + \sum_{n=1}^{3} \delta_{2,n} R^r_{j,t-n} + \delta_3 W_{j,t-1} + \delta_4 Z_{j,t-1} + \zeta_{j,t}. \tag{2}$$

Here, $p_{j,t}$ represents the probability of implementing a major structural reform in policy area $r \in \{\text{LMR}, \text{PMR}\}$ in country j at time t. Following Duval, Furceri and Miethe (2021), we include the following predictors of reforms: country fixed effects to account for unobserved heterogeneity ($\gamma_{1,j}$), a time trend (T_t), three lags of GDP growth ($g_{j,t-m}$) to capture business cycle fluctuations and three lags of the treatment variable ($R^r_{j,t-n}$) to account for extended reform implementation programmes.

The vector $W_{j,t-1}$ comprises lagged macroeconomic and structural variables essential in anticipating reform implementation (see e.g., Tommasi and Velasco, 1996; Rodrik, 1996), including the real interest rate, the output gap, the employment rate and inflation. Motivated by the crisis-induced reform hypothesis of Drazen and Grilli (1993) and Drazen and Easterly (2001), we also incorporate a global financial crisis dummy. Further, we use trade openness

and the KOF economic globalisation index in order to capture whether an economy is well integrated in international markets (see e.g., Dreher, 2006), and also account for institutional quality by including a rule of law index. The vector $Z_{j,t-1}$ includes a variable to track the duration of a government's tenure, motivated by the notion that governments are less inclined to implement reforms as their time in office increases, along with an election dummy variable (see Alesina et al., 2023). Finally, $\zeta_{j,t}$ denotes the error term.

The predicted propensity scores ($\hat{p}_{j,t}$) derived from this model are subsequently used to re-weight observations, simulating a pseudo-random assignment of reform treatments.[7] After re-weighting the observations, the post-balance test in Table 2 suggests that the covariate imbalance between treated episodes and untreated observations is effectively addressed.

The area under the ROC curve (AUC) from our probit models is 0.812 for product market reforms and 0.780 for labour market reforms, indicating a high degree of discrimination between treated and control units.[8] The smooth kernel densities displayed in Figure 12 in the Appendix illustrate the distribution of propensity scores for both treatment and control units. The figure suggests that there is sufficient overlap between the treated and control units.

As a final step we use the propensity scores ($\hat{p}_{j,t}$) from the probit model and the conditional means obtained from the initial LP model for the treated group (m_1^h) and the control group (m_0^h) to calculate the average treatment effect (ATE) of major structural reforms on investment by means of the LP-AIPW estimator:

$$\begin{aligned}\hat{\Lambda}^h_{LP-AIPW} =& \frac{1}{N}\sum_j\sum_t\Bigg\{\left[\frac{R_{j,t}(i_{j,t+h}-i_{j,t})}{\hat{p}_{j,t}} - \frac{(1-R_{j,t})(i_{j,t+h}-i_{j,t-1})}{1-\hat{p}_{j,t-1}}\right] \\ &- \frac{R_{j,t}-\hat{p}_{j,t}}{\hat{p}_{j,t}(1-\hat{p}_{j,t})}\left[(1-\hat{p}_{j,t})m_1^h + \hat{p}_{j,t}m_0^h\right]\Bigg\}.\end{aligned} \tag{3}$$

The LP-AIPW estimator combines two components: the standard Inverse Probability Weighting (IPW) estimator and an efficiency-enhancing adjustment term. The IPW component – the first term in equation (3) – calculates the ATE by re-weighting the difference in average cumulative changes in investment between the treated and control groups using the inverse of the propensity scores ($\hat{p}_{j,t}$) from the first-stage probit model. Observations with reform events and high (low) propensity scores are assigned smaller (larger) weights, while non-reform

[7] See Table 3 of the Appendix for the marginal effect of the predictors.

[8] While some observations are still likely to receive high weights, the augmentation term in the LP-AIPW estimator helps to stabilise the estimator, as shown below.

observations with a high (low) propensity of reform are assigned larger (smaller) weights. The second term in equation (3) stabilises the estimator by correcting for extreme propensity scores, using the weighted conditional means from the local projections for treated (m_1^h) and control (m_0^h) groups. Notably, the "doubly robust" property of the LP-AIPW estimator ensures its consistency even if either the propensity score model or the local projection model is misspecified.

To mitigate the imported uncertainty stemming from the first-stage propensity score estimation, we use block-bootstrapped standard errors with 500 repetitions in our AIPW estimations (see de Haan and Wiese, 2022; Wiese, Jalles and de Haan, 2024).

4 Results and interpretation

4.1 Baseline results

Next, we turn to the results of our baseline LP-AIPW estimation. Figure 4 plots the impulse responses from the baseline analysis. To put our estimates for private investment into context, we also report the impulse responses of total and public investment.

Several findings stand out. First, both product and labour market reforms have a positive impact on private investment over longer time horizons. This suggests that both types of reform are effective policy instruments for fostering private investment on a sustainable basis.

Second, the average long-term effect of labour market reforms on private investment is larger than that of product market reforms. More specifically, labour market reforms raise the level of real private investment by around 5% after six years (and even 6% after five years), while this effect is around 3% for product market reforms. To put this result into perspective, it should be noted that the product market reforms in our sample have a relatively narrow sectoral focus on individual network industries. Insofar, they might be less likely to exert strong macroeconomic effects than economy-wide labour market reforms. To recall, the average treatment effect for each policy area (PMR and LMR, respectively) may mask within-group differences across different types of reform due to the treatment heterogeneity discussed in Section 3. The number of reforms within each sub-category is however too small to allow for a meaningful econometric analysis at a disaggregated level.

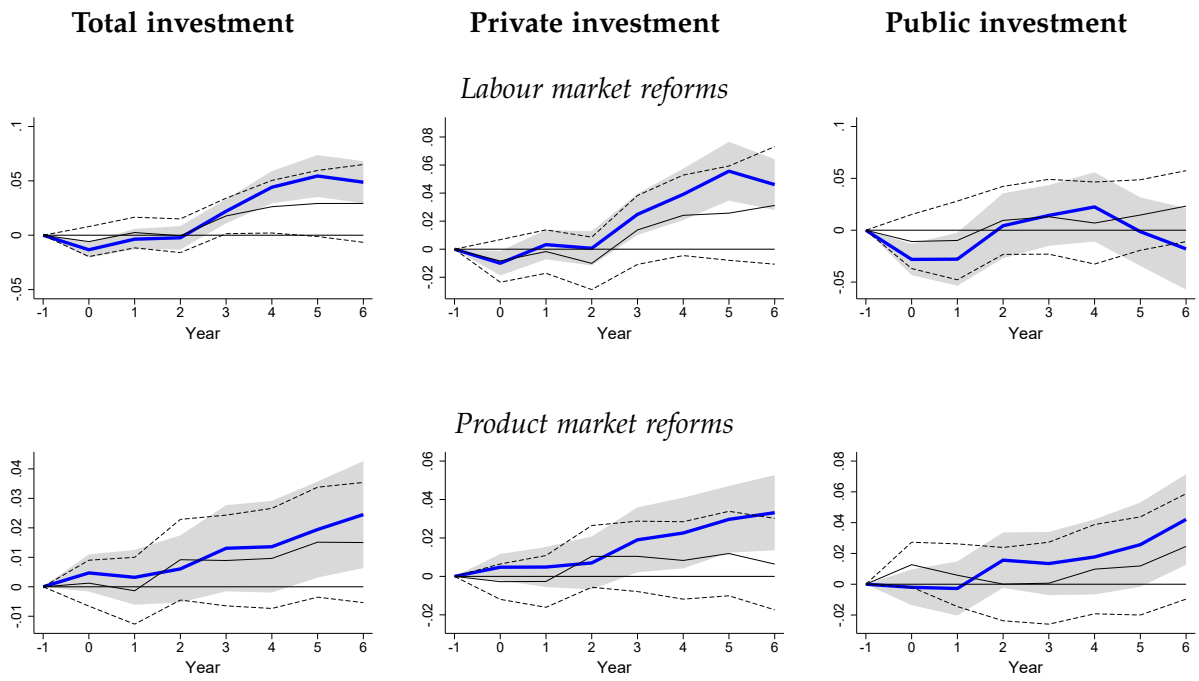
Third, the positive effects of both labour and product market reforms on private investment

become statistically significant at the 90% level only after three years. This finding is consistent with the existing literature, which typically finds that it takes time for the macroeconomic effects of structural reforms to materialise.

Fourth, the impulse response of private investment to labour market reforms resembles a J-curve, with an initial decline in the impact period followed by subsequent increases. However, the negative impact effect is modest and only borderline significant at the 90% confidence level. Therefore, we caution against overinterpreting this result.

Fifth, accounting for selection bias is crucial for uncovering the benign effects of structural reforms on private investment in our sample. In fact, simple local projections would point to insignificant results of both labour and product market reforms over all horizons, as indicated by the dashed lines in Figure 4. The failure to account for selection bias may thus help explain why the existing literature on the macroeconomic effects of structural reforms sometimes comes to inconclusive findings.

Figure 4: Impact of structural reforms on investment (baseline results)



Note: The black solid lines show the impact of structural reforms on log real investment estimated using local projections. The black dashed lines represent the corresponding 90% confidence interval based on Driscoll–Kraay standard errors. The blue line shows the average treatment effect (ATE) of structural reforms estimated using the LP-AIPW method. The shaded area indicates the corresponding 90% confidence interval based on bootstrap standard errors with 500 repetitions. Year zero marks the start of reform implementation (treatment).

Finally, the impulse responses of total investment are similar to those of private investment. This should not be surprising, given that private investment accounts for the bulk of total investment. That said, there is – weak – evidence that structural reforms also affect public investment. In response to product market reforms, public investment seems to increase over longer horizons, possibly reflecting an indirect channel operating through increased economic activity and, thus, public revenue in the aftermath of such reforms. By contrast, our estimates point to a negative but temporary effect of labour market reforms on public investment. Again, this finding could be driven by an indirect revenue channel given that the literature on labour market reforms sometimes finds adverse short-term output effects. Overall, however, structural reforms influence total investment mainly through the private sector. Therefore, the remainder of this paper will focus on private investment.

Our main findings on the impact of product market reforms are overall consistent with the existing literature, most notably Alesina et al. (2005) and Cacciatore and Fiori (2016). However, the results for labour market reforms are more benign than those of the existing literature (e.g. Cacciatore and Fiori, 2016). This could indicate that controlling for selection bias is particularly important for these reforms.

4.2 Preconditions for effective reforms

For policymakers, a crucial question is under which conditions structural reforms are particularly effective in boosting private investment. The answer to this question matters for the optimal timing of reforms and their design, including possible flanking policies. Therefore, this section explores the heterogeneous responses of private investment to structural reforms depending on different starting positions. Technically, we replace equation (1) by state-dependent local projections (e.g. Auerbach and Gorodnichenko, 2012; Ramey and Zubairy, 2018):

$$\begin{aligned} i_{j,t+h} - i_{j,t-1} =& D_{j,t-1}\Big[\alpha^h_{1,j} + \alpha^h_{2,t}T_t + \sum_{k=1}^{3}\beta^h_{1,k}\left(i_{j,t-k} - i_{j,t-1-k}\right) + \sum_{l=1}^{6}\beta^h_{2,l}R^{r=r_1}_{j,t+1-l} + \beta^h_{3,r}R^{r\neq r_1}_{j,t} \\ &+ \beta^h_4 X_{j,t-1}\Big] + \left(1 - D_{j,t-1}\right)\Big[\alpha^h_{1,j} + \alpha^h_{2,t}T_t + \sum_{k=1}^{3}\beta^h_{1,k}\left(i_{j,t-k} - i_{j,t-1-k}\right) \\ &+ \sum_{l=1}^{6}\beta^h_{2,l}R^{r=r_1}_{j,t+1-l} + \beta^h_{3,r}R^{r\neq r_1}_{j,t} + \beta^h_4 X_{j,t-1}\Big] + \varepsilon^h_{j,t}. \end{aligned} \tag{4}$$

Here, $D_{j,t-1}$ is a dummy variable capturing a specific state of the economy in country j at time $t-1$. $X_{j,t-1}$ contains the same control variables as in equation (1).

Rule of law. First, we shed light on the importance of the institutional environment for the effectiveness of structural reforms. The literature suggests that the credibility of legal institutions and the reliability of contract enforcement play a central role in shaping how economic agents respond to structural reforms (e.g. Acemoglu and Robinson, 2012). When firms and investors operate within a predictable legal framework, they might be more willing to commit capital in response to new opportunities created by policy changes. Strong rule-of-law environments mitigate uncertainty, reduce the scope for corruption or selective enforcement and enhance policy credibility. Conversely, in weak institutional settings, reforms may be distorted by governance deficiencies, limiting their capacity to stimulate sustained private investment.

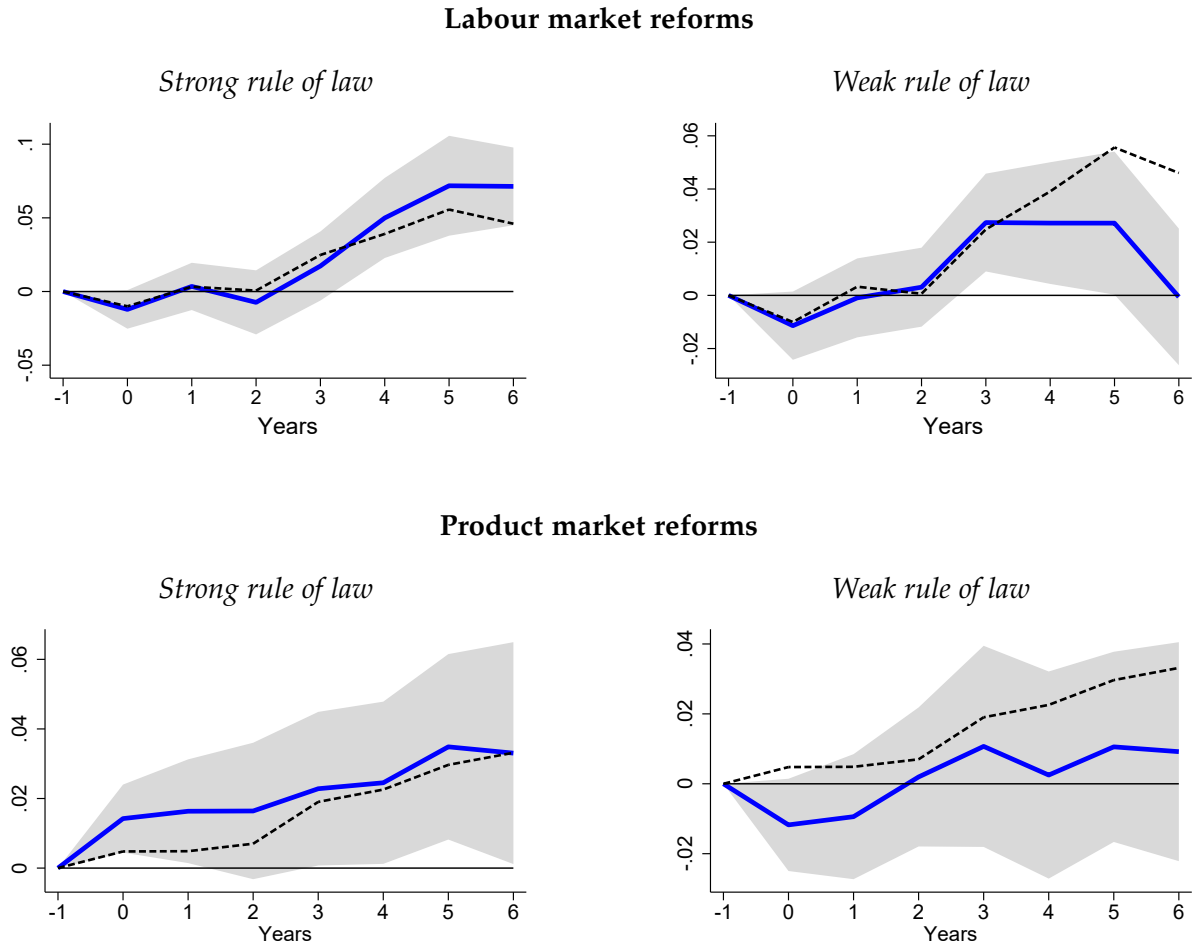
To capture the quality of institutions, we use the rule of law index from the Varieties of Democracy (V-Dem) dataset (Coppedge et al., 2026). The index estimates the extent to which the government complies with the law, courts are independent, laws transparent, justice accessible, corruption absent, and the bureaucracy is impartial. The dummy $D_{j,t-1}$ equals 1 if the rule-of-law index is above the sample median, and 0 otherwise. In our sample, structural reforms are almost equally distributed across the two states: country-year observations with a relatively high rule-of-law index account for 47% of product market reforms and 51% of labour market reforms. The (lagged) rule of law indicator is arguably exogenous to the structural reform shock. Hence, our state-dependent local projections should asymptotically recover the population response of private investment regardless of the shock size (Gonçalves et al., 2024).

We find that labour market reforms implemented in an environment characterised by stronger institutional quality, as measured by the rule-of-law index, typically have a more pronounced and persistent impact on private investment (see Figure 5). In countries with a relatively high rule-of-law-index, the boost to the level of private investment generated by labour market reforms increases steadily over time, reaching 7% at the end of our estimation horizon. By contrast, countries with relatively weak rule of law record a hump-shaped impulse response. Having reached a peak three years after the implementation of the reform 2.7%, the effect becomes statistically insignificant towards the end of our estimation horizon.

The impulse responses for product market reforms are surrounded by high uncertainty (see Figure 5). In countries with relatively weak rule of law, the estimated coefficients for

private investment are statistically insignificant at the 90% level throughout the estimation horizon. In countries with stronger rule of law, the coefficients are significant in some years, although sometimes marginally. Notwithstanding this, the estimated average treatment effect in this group increases over time, almost reaching 4% at its peak. Overall, there is at least weak evidence that the rule of law also enhances the effectiveness of product market reforms.

Figure 5: The impact of product and labour market reforms on private investment depending on the rule of law

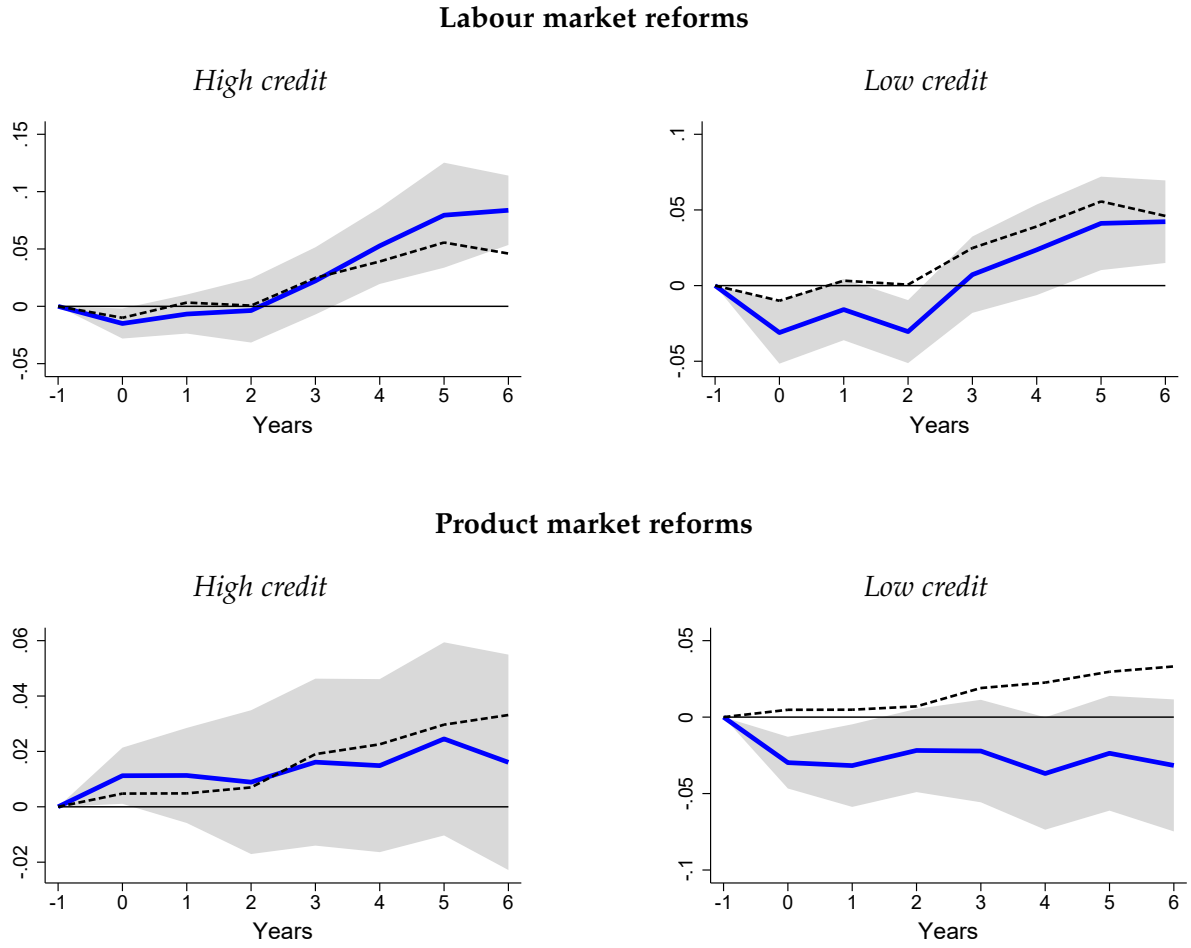


Note: The solid blue line represents the (ATE-AIPW) cumulative impulse response of private investment following a product or labour market reform for country-year observations with strong and weak rule of law (compared to the sample median), respectively, based on equation (4). The shaded area indicates the 90% confidence interval using bootstrapped standard errors. The dashed line is the baseline (ATE-AIPW) cumulative impulse response from equation (3).

Financial development. Financial development is likely to be another important determinant shaping the impact of structural reforms on private investment as firms' ability to invest critically depends on access to finance. Against this backdrop, the private credit-to-GDP ratio

and the stock market capitalisation-to-GDP ratio provide natural and widely used proxies for the depth of financial intermediation and the availability of financing to the private sector (Levine, Loayza and Beck, 2000; Levine, 2005). A higher private credit-to-GDP ratio captures greater financial development and, more broadly, easier access to bank-based finance for firms, reflecting more efficient banking systems, deeper financial intermediation and lower borrowing constraints. Likewise, a higher stock market capitalisation-to-GDP ratio captures greater size and depth of equity markets, reflecting better access to stock market-based finance.

Figure 6: The impact of product and labour market reforms on private investment depending on private credit-to-GDP ratio

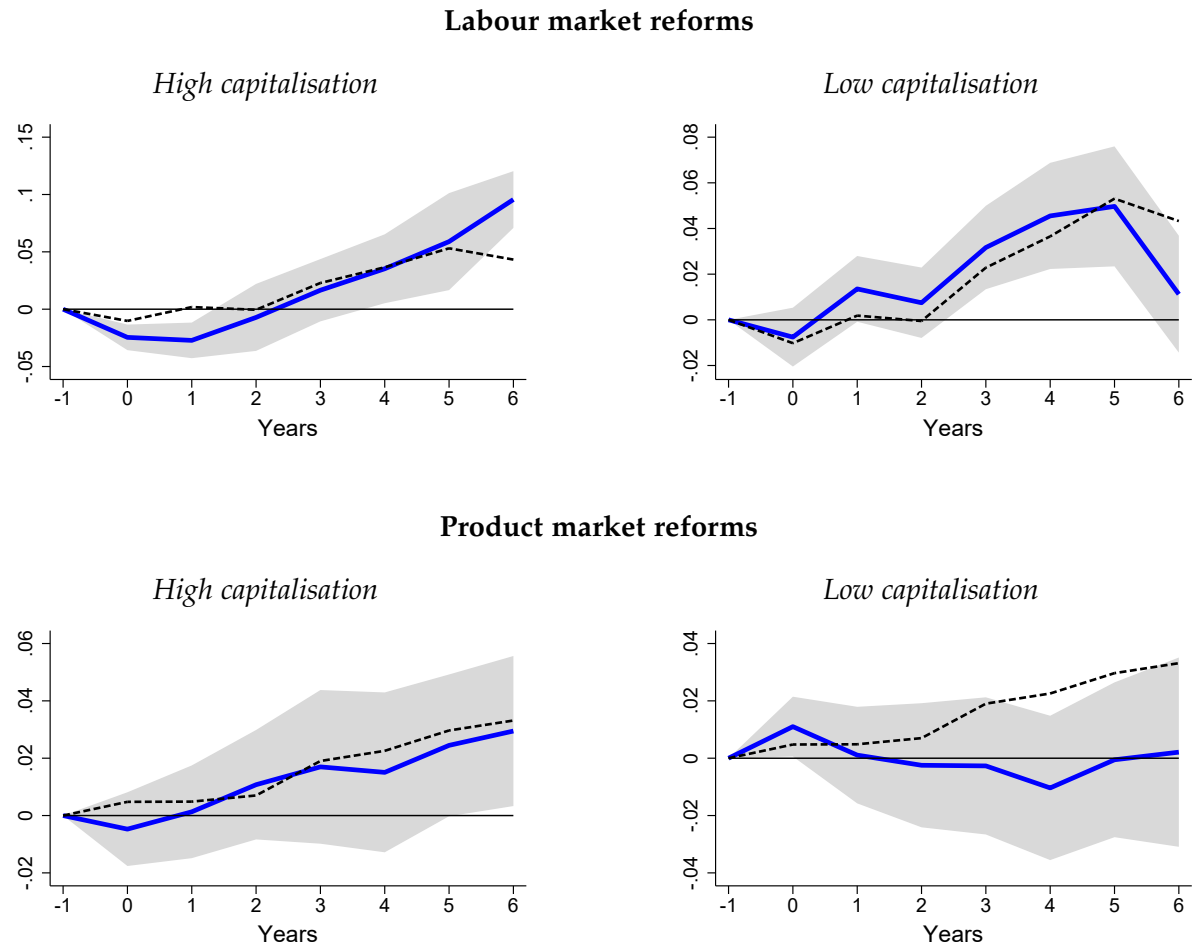


Note: The solid blue line represents the (ATE-AIPW) cumulative impulse response of private investment following a product or labour market reform for countries with a high and low credit-to-GDP ratio (compared to the sample median), respectively, based on equation (4). The shaded area indicates the 90% confidence interval using bootstrapped standard errors. The dashed line is the baseline (ATE-AIPW) cumulative impulse response from equation (3).

We study both dimensions separately. In both cases, the empirical implementation mirrors

that used for the rule of law above by resting on a dummy variable that equals one when the respective measure of financial development exceeds the sample median, and zero otherwise.

Figure 7: The impact of product and labour market reforms on private investment depending on stock market capitalisation-to-GDP ratio



Note: The solid blue line represents the (ATE-AIPW) cumulative impulse response of private investment following a product or labour market reform for countries with a high and low stock market capitalisation-to-GDP ratio (compared to the sample median), respectively, based on equation (4). The shaded area indicates the 90% confidence interval using bootstrapped standard errors. The dashed line is the baseline (ATE-AIPW) cumulative impulse response from equation (3).

We first present results for the *private credit-to-GDP ratio*. The results shown in Figure 6 provide evidence that a high private credit-to-GDP ratio is associated with a more favourable impact of structural reforms on private investment. This pattern is most evident for labour market reforms, where a reform typically raises the level of real private investment by approximately 10% cumulatively over six years when the private credit-to-GDP ratio is high. When the ratio is low, the impact remains positive and statistically significant, but its magnitude is halved.

For product market reforms, the evidence is not decisive. A high private credit-to-GDP ratio is associated with a more favourable investment response, the medium-term effect is however not statistically significant. In low-credit environments, the estimated response turns negative, but is again not statistically different from zero. Notably, for product market reforms, the estimated medium-term effects in both high- and low-credit environments lie below the baseline estimate and become less precise, which may reflect the fact that allowing for heterogeneity across financial conditions reduces estimation precision and alters the weighting of observations, leading to smaller and less precisely estimated within-regime effects.

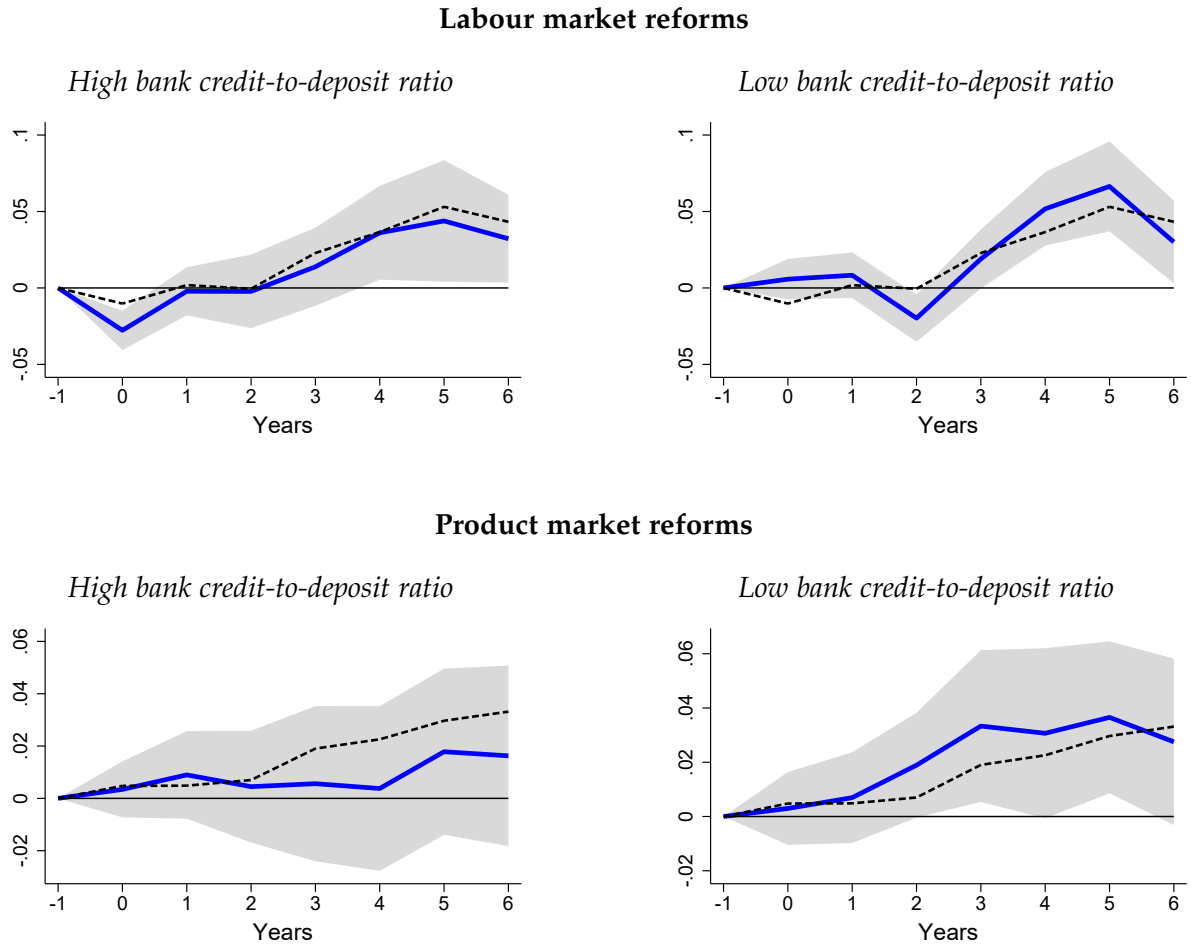
Turning to the *stock market capitalisation-to-GDP ratio*, Figure 7 suggests that for both product and labour market reforms, high stock market capitalisation is associated with a stronger private investment response. For labour market reforms, the impact under high capitalisation reaches 10% after six years and is statistically significant while under low capitalisation it peaks at 5% after five years and the confidence interval includes zero after six years. Likewise, for product market reforms the impact is positive and statistically significant at around 4% after six years under high market capitalisation whereas under low capitalisation the estimates remain centred around zero and are not statistically significant.

Overall, the evidence suggests that financial development plays an important role in shaping the investment effects of structural reforms. This is in line with the view that better-developed financial systems ease financing constraints and facilitate firms' investment responses to reform-induced changes in incentives.

Bank lending conditions. While standard measures of financial development capture the overall availability of finance, they do not necessarily reflect banks' ability and willingness to expand lending at the margin. To capture this dimension, we consider the bank credit-to-deposit ratio, which reflects banks' lending stance and balance-sheet conditions.[9] A higher ratio may indicate that banks are already heavily exposed and have limited scope to expand lending further, especially to higher-risk borrowers.

[9]The bank credit-to-deposit ratio corresponds to the financial resources provided to the private sector by domestic money banks as a share of total deposits. Domestic money banks comprise commercial banks and other financial institutions that accept transferable deposits, such as demand deposits. Total deposits include demand, time and saving deposits in deposit money banks.

Figure 8: The impact of product and labour market reforms on private investment depending on bank credit-to-deposit ratio



Note: The solid blue line represents the (ATE-AIPW) cumulative impulse response of private investment following a product or labour market reform for countries with high and low bank credit-to-deposit ratio (compared to the sample median), respectively, based on equation (4). The shaded area indicates the 90% confidence interval using bootstrapped standard errors. The dashed line is the baseline (ATE-AIPW) cumulative impulse response from equation (3).

The empirical implementation again mirrors that used above. We construct a dummy variable that equals one when the bank credit-to-deposit ratio exceeds the sample median, and zero otherwise. The results in Figure 8 suggest that bank lending conditions matter primarily for product market reforms. In low credit-to-deposit environments, product market reforms are associated with a positive increase in private investment over the medium term. The effect, however, hovers between being statistically different from zero and not statistically distinguishable from zero over the horizon. By contrast, when the ratio is high, the response is weaker and clearly not statistically different from zero. This pattern is consistent with

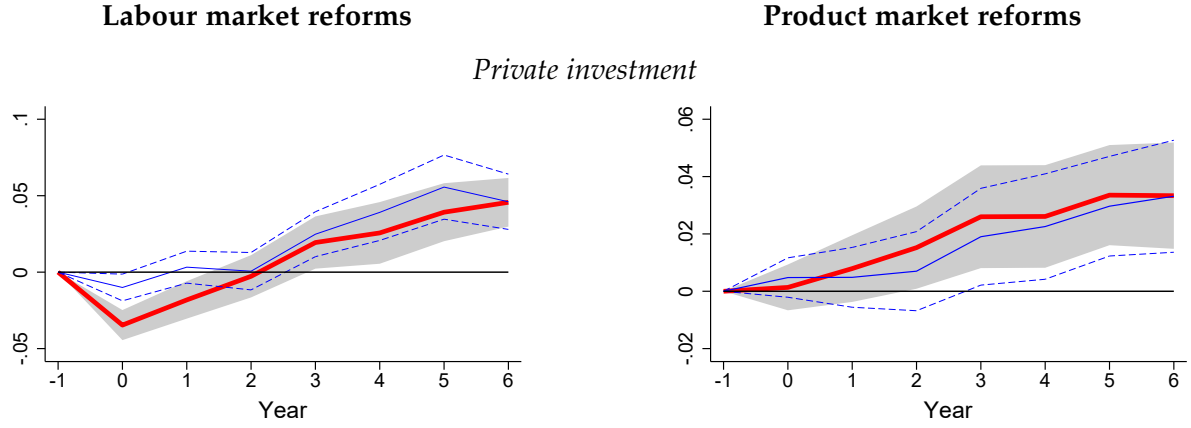
the view that product market reforms require financing for riskier investment opportunities, such as firm entry, and that banks may be less able or willing to extend additional credit in such environments when they are already highly exposed. For labour market reforms, the differences across regimes are less pronounced, suggesting that their investment effects are less sensitive to this particular dimension of financial conditions.

5 Robustness checks

To ensure the robustness of our results, we conduct several sensitivity checks. Most importantly, we control for compositional effects and look at both counter reforms and placebos. Notably, our main results also go through if we exclude financial centres from the sample or include leads of the treatment variable in our local projections to account for anticipation effects.

Compositional effects. In our empirical setting, local projections (LPs) are not estimated on a fixed set of observations across horizons. By construction, the LP framework typically leads to a loss of observations as the horizon length increases, which may induce compositional effects if the set of countries contributing to the estimates changes over time. Such changes could potentially distort the estimated responses.

Figure 9: Robustness check: compositional effects in baseline specification



Note: The blue solid line shows the ATE of structural reforms on real private investment estimated using the LP-AIPW method. The red line shows the ATE of structural reforms estimated using the LP-AIPW method using only the observations available in year 6. The shaded area indicates the 90% confidence interval based on bootstrap standard errors with 500 repetitions.

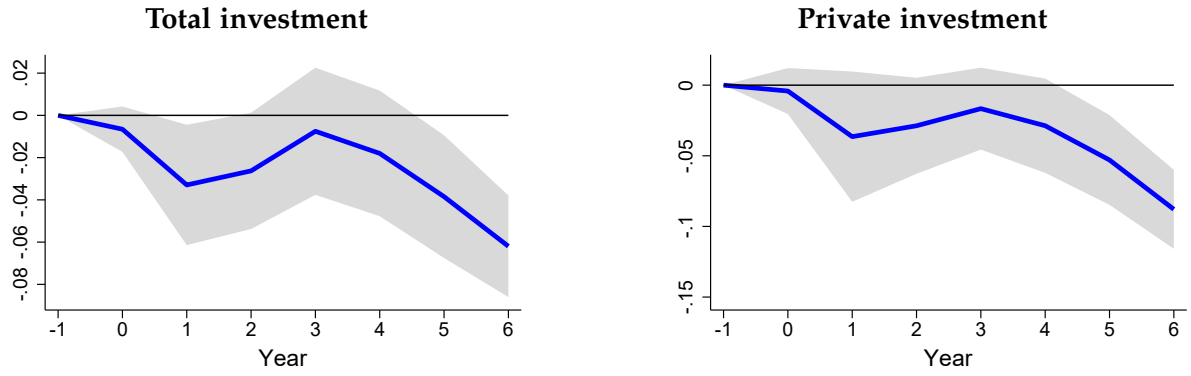
To address this concern, we first look at the number of countries included at each projection horizon. As it turns out, the country coverage remains broadly stable across horizons, suggest-

ing that compositional shifts are unlikely to drive our results. To verify this, we re-estimate the baseline ATEs using a fixed sample of observations that is available at the longest time horizon, $h = 6$. The resulting estimates are very similar to those obtained from the baseline specification (see Figure 9), providing further reassurance that our findings are not driven by changes in the underlying sample composition over time.

The effect of counter reforms. The narrative dataset used in this paper allows us to examine the plausibility of our baseline results by looking at the impact of counter reforms, i.e. episodes in which major structural reforms were rolled back. Intuitively, the impulse responses of counter reforms should be a mirror image of our baseline results.

Given the scarcity of counter reforms in our sample (49 in total), we need to pool all these events regardless of whether they are related to labour or product markets. With this caveat, Figure 10 illustrates that counter reforms are indeed estimated to have a negative impact on private and total investment over longer horizons in contrast with the positive effects obtained in the deregulation case. The effects on public investment are again insignificant. The results are thus consistent with our baseline estimation for major structural reforms.

Figure 10: Impact of counter reforms on investment



Note: The blue line shows the impact of counter reforms on real investment estimated using local projections. The shaded area indicates the corresponding 90% confidence interval based on Driscoll-Kraay standard errors.

Placebo reforms. To further assess the robustness of our baseline results, we perform placebo reform simulations. This exercise is designed to detect spurious results driven by the estimator or our dataset properties rather than genuine reform effects. Specifically, we construct a distribution of unconditional ATE estimates based on randomly generated reforms. Coefficients obtained from placebo reforms are expected to follow a normal distribution centred around zero. By contrast, the coefficients associated with actual reforms are expected to be

located closer to the tail of the distribution when the baseline coefficients are statistically significant. We run 10,000 simulation replications. Placebo reforms are drawn from a binomial distribution, with the probability of success calibrated to match the frequency of product and labour market reforms, respectively, observed in our sample.

In the presence of spurious results, the coefficient distributions from placebo reforms would mirror those obtained from actual reforms. Instead, our placebo coefficients are tightly centred around zero while the estimated effects of actual reforms are consistently located closer to the tail of the distribution, at least in years in which our baseline specification yields statistically significant coefficients (see Figures 13-14 in the Appendix). Overall, our results seem to capture meaningful economic effects of reforms rather than being artefacts of the estimation procedure.

6 Conclusion

This paper examines the dynamic effects of major structural reforms on private investment in advanced economies. To enhance the robustness of our findings, our empirical approach combines narrative reform identification with the local projections augmented inverse probability weighting (LP-AIPW) estimator proposed by Jordà and Taylor (2016).

In a sample covering 26 OECD countries over the period 1975–2020, we find that major labour and product market reforms can significantly boost private investment over the medium term, while their effects vary by reform type and country context. Labour market reforms are typically more effective in boosting private investment than product market reforms, possibly due to the industry-specific focus of most product market reforms in our sample. However, both types of reform have in common that the positive effects on private investment only become statistically significant after around three years. We also find that the effectiveness of structural reforms is amplified in countries offering strong institutional frameworks and access to ample external finance for the private sector.

These results have important policy implications. First, our estimates underscore the potential of well-designed labour and product market reforms to revive private investment and enhance long-term growth prospects in advanced economies. Unfolding this potential will however require a step-up in reform efforts, considering the weak reform momentum highlighted by the database used in this paper. Second, our findings emphasise the critical role of institutional quality and access to finance in shaping reform outcomes. Insofar, efforts to

improve institutional settings and the workings of financial markets – including through further cross-border integration – should not be seen as substitutes for, but rather as complements and critical enablers of successful labour and product market reforms.

We see several promising avenues for future research. First, future studies could delve deeper into the transmission channels through which reforms influence investment, such as firm-level productivity growth or allocative efficiency. Second, the analysis could be expanded to include additional reform types. Third, more granular data capturing the intensity of reforms would enhance the precision of future analyses. By addressing these gaps, future research can build on the findings presented in this paper to deepen our understanding of the role structural reforms play in shaping investment dynamics and, thereby, long-term economic growth.

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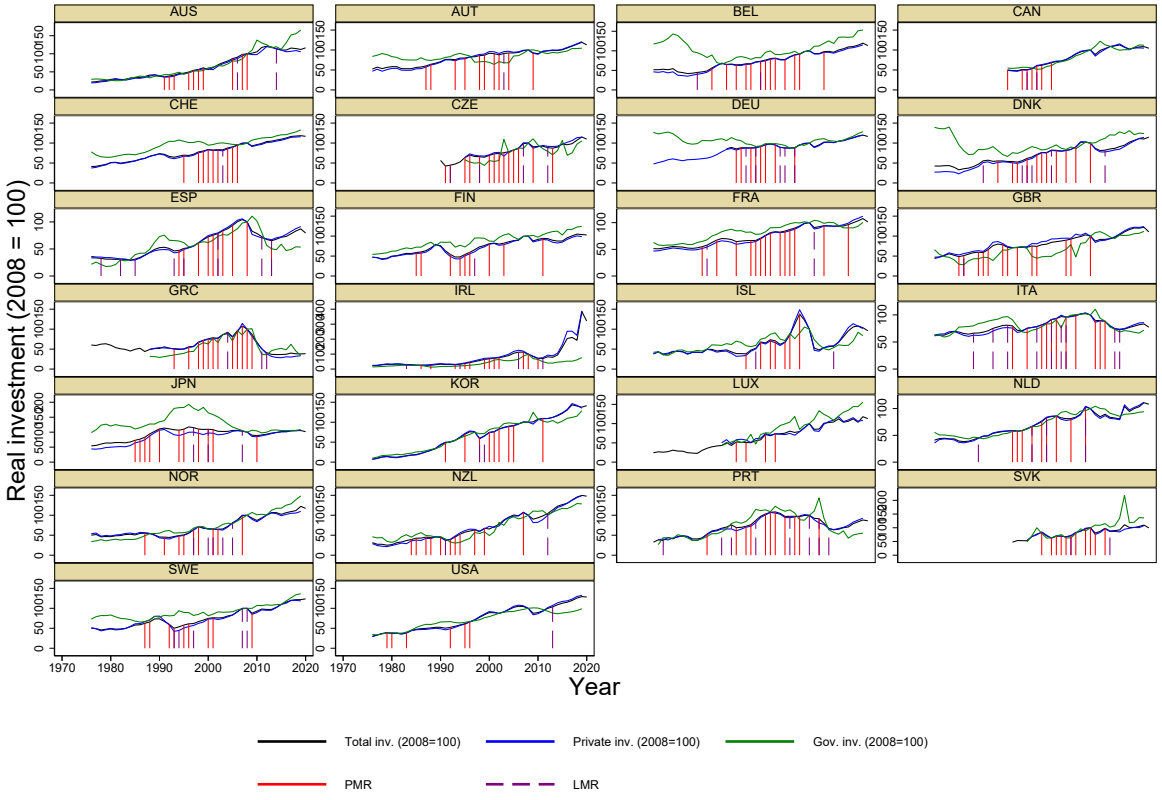
A Appendix

Data

Our baseline sample covers 26 OECD countries over the period 1975-2020: Australia, Austria, Belgium, Canada, Czechia, Denmark, Finland, France, Germany, Greece, Iceland, Ireland, Italy, Japan, Korea, Luxembourg, the Netherlands, New Zealand, Norway, Portugal, Slovakia, Spain, Sweden, Switzerland, United Kingdom and the United States.

Figure 11: Real total, private and public investment (constant local currency units, billions) and reform events by country



Sources: IMF, European Commission (AMECO), own illustration. *Note:* Each vertical red line marks a reform event as defined by Wiese, Jalles and de Haan (2024). PMR stands for product market reforms and LMR for labour market reforms.

Table 1: Descriptive statistics

Variable	N	Mean	Min	Max	Source
Log real private investment	1,173	26.1	22.0	33.9	IMF, AMECO
Log real GDP	1,286	27.6	23.1	35.2	World Bank
Real interest rate	1,126	2.4	-17.1	21.0	OECD
Output gap (% potential GDP)	1,286	0.0	-13.5	18.5	World Bank
Inflation	1,258	5.2	-4.5	84.0	World Bank
Trade openness (% GDP)	1,286	76.2	10.8	382.4	World Bank
Rule of law	1,283	1.0	0.3	1.0	V-DEM
KOF economic globalisation	1,280	66.5	21.7	93.6	ETH
Legislative elections	1,165	-2.3	0.0	1.0	DPI(2020)
Years in office	1,163	1.2	1.0	18.0	DPI(2020)
Employment rate	1,260	46.2	30.0	74.8	World Bank
Credit to private sector (% GDP)	754	99.5	23.4	301.0	World Bank
Stock market capitalisation (% GDP)	912	59.1	0.0	321.9	World Bank
Bank credit to deposit ratio	1,156	112.4	17.8	370.9	World Bank

Results

Table 2: Balance test for differences in means of covariates between reform episodes and other observations

	Pre-weighting		Post-weighting	
Control variable	**PMR**	**LMR**	**PMR**	**LMR**
GDP growth	-0.001	-0.011***	-0.004	-0.000
	(0.001)	(0.003)	(0.007)	(0.012)
Real interest rate	1.773***	1.086**	0.296	0.482
	(0.232)	(0.380)	(0.349)	(0.501)
Output gap	-0.194*	-0.649**	0.320	-0.233
	(0.110)	(0.306)	(0.297)	(0.432)
Inflation rate	2.597***	9.213**	0.018	-0.273
	(0.457)	(3.649)	(0.516)	(0.600)
GFC	-0.005	-0.020	0.019	0.022
	(0.014)	(0.022)	(0.031)	(0.065)
Trade openness	-3.000	-6.614	-1.306	-0.716
	(3.389)	(5.460)	(8.703)	(4.975)
Rule of law	0.010	0.008	0.002	0.000
	(0.007)	(0.011)	(0.005)	(0.006)
Observations	1,142	1,142	1,026	999

Note: Two-tailed t-test standard errors in parenthesis. ***/**/* Indicate p-value 0.01/0.05/0.10.

Table 3: Average marginal effects from probit models of product and labour market reforms

	PMR	LMR
GDP growth (t-1)	1.808***	−0.781*
	(0.657)	(0.453)
GDP growth (t-2)	−0.635	0.164
	(0.658)	(0.455)
GDP growth (t-3)	1.134*	−0.425
	(0.648)	(0.412)
Long-term interest rate (t-1)	0.0210***	0.00167
	(0.006)	(0.003)
Output gap % potential GDP (t-1)	−0.0164**	0.000840
	(0.007)	(0.005)
Inflation (t-1)	0.00268	0.00298**
	(0.002)	(0.001)
Global financial crisis (t-1)	0.0431	−0.0869*
	(0.062)	(0.053)
Trade openness (t-1)	−0.00506***	−0.00205**
	(0.001)	(0.001)
Rule of law index (t-1)	0.640	0.0341
	(0.524)	(0.262)
Legislative election held (t-1)	0.00139	0.00268
	(0.027)	(0.019)
Years in office (t-1)	0.00887*	−0.00736**
	(0.005)	(0.004)
Economic globalisation index (t-1)	0.0170***	0.00483**
	(0.004)	(0.002)
Employment rate (t-1)	0.0156**	−0.00753
	(0.007)	(0.005)
PMR (t-1)	0.0649**	
	(0.027)	
PMR (t-2)	0.0192	
	(0.027)	
PMR (t-3)	0.0964***	
	(0.026)	
LMR (t-1)		−0.0346
		(0.028)
LMR (t-2)		−0.00512
		(0.027)
LMR (t-3)		−0.0202
		(0.028)
Time trend	−0.0663***	−0.0494**
	(0.0195)	(0.0236)
FE	YES	YES
AUC	0.812	0.780
Observations	1,026	999

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

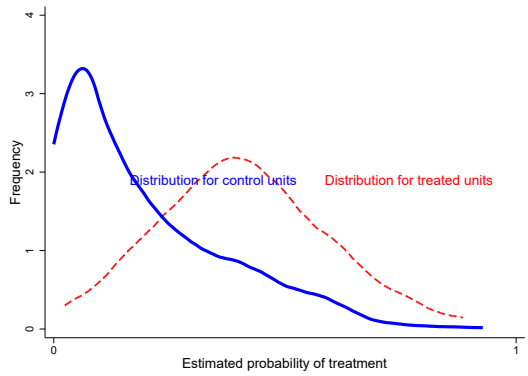
Table 4: Coefficient estimates (baseline): impact of product and labour market reforms on investment using the LP-AIPW method

Year	$t=0$	$t=1$	$t=2$	$t=3$	$t=4$	$t=5$	$t=6$
Total investment							
PMR	0.0047	0.0032	0.0061	0.0131	0.0136	0.0194**	0.0245**
	(0.0038)	(0.0057)	(0.0069)	(0.0089)	(0.0095)	(0.0099)	(0.0110)
Observations	1,004	978	952	926	900	874	849
LMR	-0.0134***	-0.0036	-0.0023	0.0221***	0.0441***	0.0545***	0.0460***
	(0.0045)	(0.0057)	(0.0066)	(0.0067)	(0.0090)	(0.0117)	(0.0118)
Observations	977	952	927	902	877	852	828
Private investment							
PMR	0.0048	0.0049	0.0070	0.0190*	0.0226**	0.0297***	0.0331***
	(0.0042)	(0.0063)	(0.0084)	(0.0103)	(0.0112)	(0.0106)	(0.0119)
Observations	993	967	941	915	889	863	838
LMR	-0.0100*	0.0033	0.0006	0.0248***	0.0391***	0.0556***	0.0488***
	(0.0053)	(0.0064)	(0.0074)	(0.0089)	(0.0111)	(0.0128)	(0.0110)
Observations	941	916	891	866	841	817	792

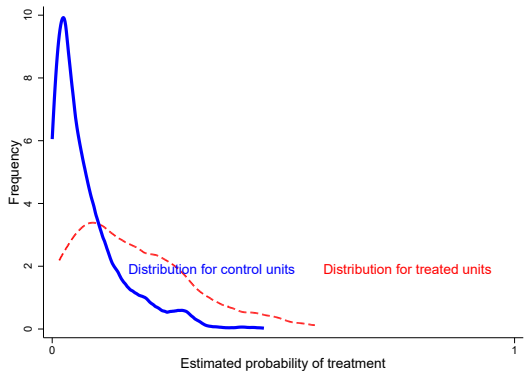
Note: Bootstrapped standard errors in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Figure 12: Overlap of propensity scores for treatment and control group

(a) Product market reforms

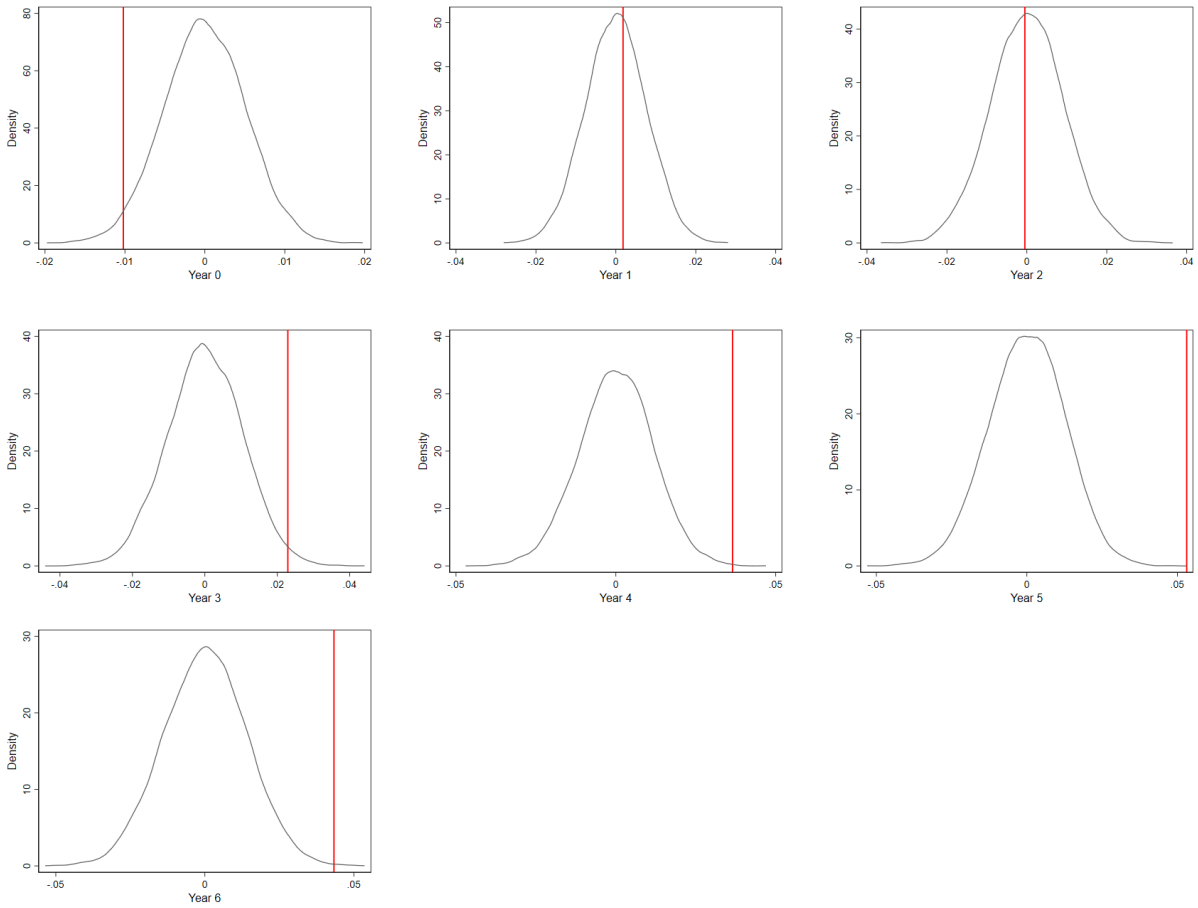


(b) Labour market reforms



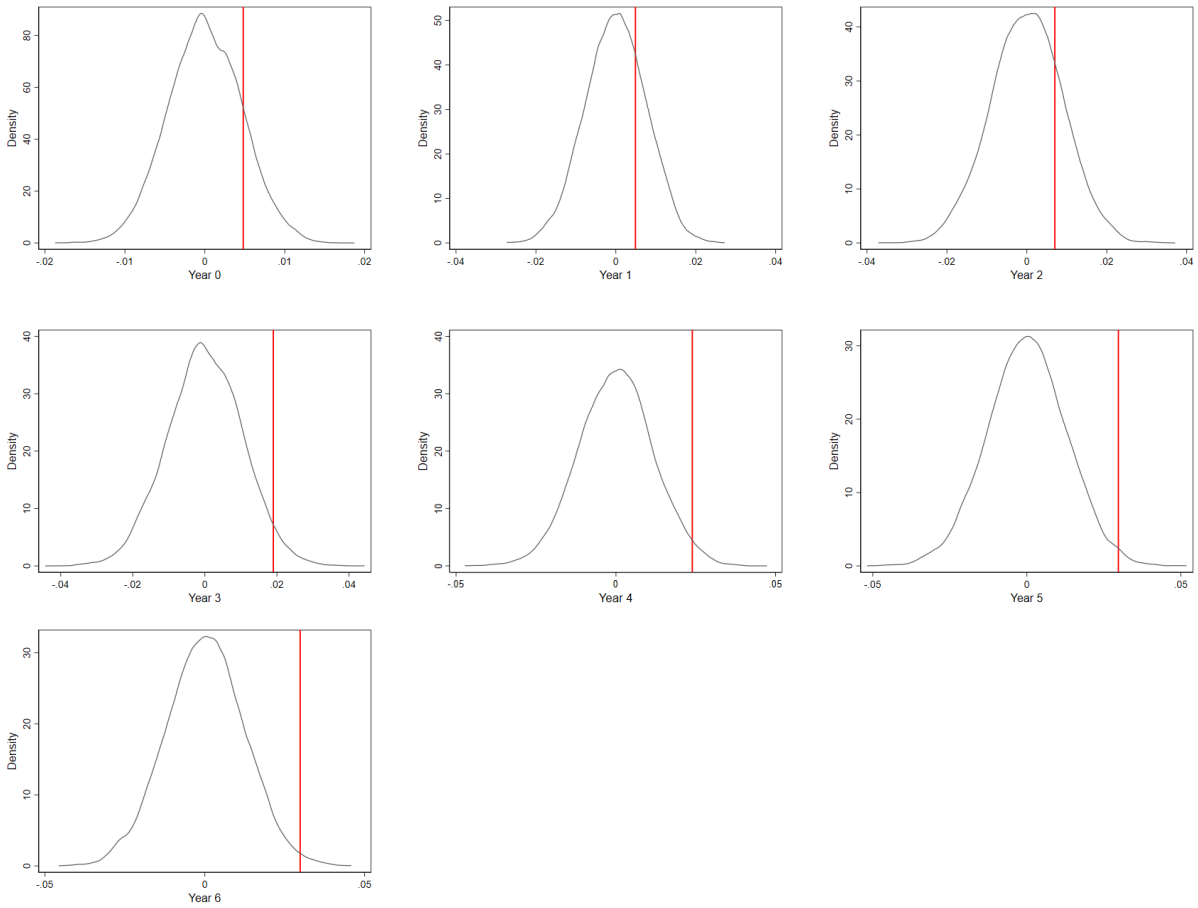
Note: Propensity scores derived from the probit model in equation (2).

Figure 13: Distribution of the coefficients of unconditional ATEs of placebo labour market reforms for private investment



Note: This figure illustrates the distribution of coefficients representing the unconditional Average Treatment Effects (ATEs) of randomly generated labour market reforms on private investment. The simulations rely on the augmented inverse probability weighting (LP-AIPW) approach. The thin vertical line denotes the ATE derived from the main analysis using actual reforms. All simulations are based on 10,000 replications. The placebo reforms are generated from a binomial distribution with a 10% probability of success, mirroring the proportion of actual reforms observed in our sample.

Figure 14: Distribution of the coefficients of unconditional ATEs of placebo product market reforms for private investment



Note: This figure illustrates the distribution of coefficients representing the unconditional Average Treatment Effects (ATEs) of randomly generated product market reforms on private investment. The simulations rely on the augmented inverse probability weighting (LP-AIPW) approach. The thin vertical line denotes the ATE derived from the main analysis using actual reforms. All simulations are based on 10,000 replications. The placebo reforms are generated from a binomial distribution with a 30% probability of success, mirroring the proportion of actual reforms observed in our sample.

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