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Turn on the Bright Lights

The Effects of Lobbying Transparency Reforms on Investment Outcomes in OECD Countries, 2000 to 2024

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Why Should I Read This Research?

Who lobbies whom, about what, and for how much? These are questions that lobbying registers are designed to answer, because in transparent representative democracies, citizens have a right to know who shapes the decisions made on their behalf. Beyond this accountability imperative, international organisations have begun increasingly framing lobbying transparency reforms as instruments of economic governance that can reduce market uncertainty, stabilise regulatory environments, and attract investment (OECD, 2026; World Bank, 2024; Council of the European Union, 2026). Placing mandatory, legally enforceable lobbying registers at the centre of an ‘integrity dividend’ narrative naturally raises the question: does the greater transparency in lobbying activities truly improve business confidence and attract investment?

This study addresses this research gap in the public integrity policy debate and political economy literature, by providing the first cross-country causal test of whether lobbying register adoption generates measurable improvements in business confidence and foreign direct investment across 24 OECD countries between 2000 and 2024. Building on signalling and credible commitment theory, this study examines whether lobbying transparency operates as a credible, costly signal of constrained elite discretion that influences firms and investors to reallocate their capital to reforming countries (Spence, 1973; North and Weingast, 1989). It employs a staggered difference-in-differences panel design, complemented by robustness and sensitivity diagnostics.

The empirical analysis returns null findings, which are robust, consistent, and consequential for both the public integrity reform agenda and institutional signalling literature. For policymakers, these findings suggest the economic case for lobbying transparency has been advanced more confidently than the evidence warrants, and that registers designed primarily to signal institutional grade governance are unlikely to deliver on that promise. For scholars, the findings impose a boundary condition, as registers that fail to meet minimum standards cannot function as costly signals, and the perceptual mechanism through which credible commitment effects would need to operate does not activate. These results notwithstanding, the absence of an economic dividend does not diminish the case for lobbying transparency, rather it clarifies it, and in doing so, sharpens the terms on which these reforms should be justified, designed, and held to account.

1. INTRODUCTION

Public integrity has long been considered fundamental to open and accountable governments, expressing the intuition that those bestowed with public power should be held to high standards of fairness, service, and accountability. However, recent publications from international organisations extend the basis of these policies beyond normative and democratic grounds, increasingly framing lobbying transparency as an ‘integrity advantage’ that delivers greater trust in institutions, fairer competition, and a more attractive investment climate. Whether that institutional narrative survives empirical scrutiny is the central question this study addresses.

Notably, the OECD, World Bank, and European Union have recently positioned lobbying transparency reforms within a broader macroeconomic rationale, advancing the argument that well-designed and properly implemented governance reforms reduce transaction costs, stabilise regulatory environments, and increase investment attractiveness (OECD, 2026; World Bank, 2024; Council of the European Union, 2026). Specifically, the OECD’s *Anti-Corruption and Integrity Outlook* (2026) posits that well-implemented integrity reforms “reduce governance costs, stabilise markets, and attract investment”; the World Bank Enterprise Surveys assert that these reforms “attract investment, encouraging sustainable economic growth” (World Bank, 2025); and the EU’s Anti-Corruption Directive invokes the “foreseeable positive economic outcomes” of comprehensive integrity reforms (Council of the European Union, 2026).

The implication is that three of the leading standard-setting organisations in public governance are staking considerable reform credibility on an economic proposition. This policy convergence raises an evident but surprisingly unasked question, which forms the central puzzle of the study: if integrity reforms carry the economic weight that these organisations claim, their downstream effects on business confidence and foreign direct investment should be empirically detectable. Yet in the absence of causal evidence, the economic case for integrity reforms rests on presumption rather than proof, which forms a gap the existing literature has not yet closed.

The literature on institutional reform and economic governance provides the theoretical context within which the integrity dividend is most plausibly situated. Drawing on signalling theory and

credible commitment frameworks, a substantial body of research argues that governance reforms generate economic returns by reducing informational asymmetries between private sector actors and government, by constraining the discretion of elites and making commitments to rule-bound behaviour observable and costly to reverse (Spence, 1973; North and Weingast, 1989; Andrews, 2013). The intuition is well established: when governments demonstrate credible commitment to transparency, rational firms and investors should revise their assessments of institutional quality upward, reducing the risk premium they attach to investments in that jurisdiction.

Within this literature, however, lobbying transparency occupies a curiously underexamined position. While the democratic rationale for disclosure has been extensively developed, grounded in the right of citizens to know who shapes decisions made on their behalf, the proposition that lobbying registers also generate measurable macroeconomic returns has been advanced through policy advocacy and theoretical inference rather than causal identification (Chari, Hogan and Murphy, 2010; Laboutková, Šimral and Vymětal, 2025). The cross-country quasi-experimental evidence that would be required to substantiate or refute that proposition is yet to be established, which is the research gap this study seeks to address.

Notwithstanding this institutional momentum, recent OECD evidence presents a tension at the core of the rationale for the ‘integrity advantage.’ In the biennial flagship OECD publication, the *Anti-Corruption and Integrity Outlook 2026*, member countries are shown to meet a mere 63 percent of anti-corruption and integrity criteria in de jure regulation, and only 44 percent in de facto implementation, denoting an ‘implementation gap’ of 19 percentage points between the comprehensiveness of legislative frameworks and their operationalisation in practice (OECD, 2026).

Looking at lobbying specifically, only 43 percent of criteria is met on regulation, and 38 percent on implementation, indicating a 5 percentage point implementation gap; also noting the composite lobbying regulation score fell from 44 to 43 percent between 2022 and 2025, showing stagnation even in the de jure dimension. Beyond technical limitations, these implementation gaps undermine the very mechanism through which lobbying transparency is claimed to deliver economic returns, because weakly designed and implemented registers cannot function as

credible signals of constrained elite discretion, and rational investors have no basis on which to revise their assessments of institutional quality accordingly (Spence, 1973; North and Weingast, 1989).

There are two reasons for which lobbying transparency offers the most direct empirical test of the integrity dividend hypothesis. First, lobbying, the organised exercise of influence by private interests over public policy, is the central mechanism through which undue influence operates, while being the most frequently cited driver of institutional distrust. (OECD, 2026; Chari, Hogan and Murphy, 2010). This makes it the reform category most directly implicated for economic and investment outcomes. Second, it is also the reform category in which the gap between formal adoption and substantive implementation is most acute; thus, the integrity dividend, if detectable anywhere, would be apparent in this context. Governments responding to the democratic and reputational costs of poorly regulated lobbying activity have adopted mandatory registers at an accelerating rate, with the share of OECD member countries implementing lobbying regulatory frameworks rising to 67 percent as of 2025 (OECD, 2026).

The evidentiary problem, however, is that adoption has substantially outpaced disclosure quality. Only 12 OECD member countries with an operational register require lobbyists to identify the specific legislative or regulatory initiatives they are targeting, which is the most basic condition for a register to carry genuine informational content, while beneficial ownership transparency has fallen from 42 to 17 percent of registers since 2022 (OECD, 2026). A register that fails to disclose what legislation lobbyists intend to influence, or who ultimately benefits from their activity, cannot credibly constrain elite behaviour or provide investors with a meaningful signal of improved governance quality. Whether registers this informationally thin can deliver the business confidence and investment returns their advocates promise is an empirical question, which this study addresses through two research questions:

1. *To what extent do mandatory lobbying transparency reforms affect business confidence and foreign direct investment (FDI) inflows in OECD democracies?*
2. *Does the economic impact of these reforms persist over the medium-term, suggesting structural improvements in investment climates, or is it limited to short-term confidence shocks?*

To answer these questions, a staggered difference-in-differences panel design is used across 24 OECD countries between 2000 and 2024. The analysis employs three estimators: a Two-Way Fixed Effects model as a baseline, and the Callaway and Sant’Anna heterogeneity-robust specifications to address cohort-specific treatment effect heterogeneity in the staggered adoption, with dynamic event-study designs, instrumental variable mechanism tests, and Rambachan and Roth sensitivity tests applied throughout. The analysis separates 24 countries into three groups: Non-reformers, which provide the counterfactual baseline; Reformers, which adopted mandatory, legally enforceable registers during the observation period, constituting the treatment group; and Established countries, which entered the observation period with registers already operational and are analysed separately to avoid conflating adoption effects with maturation effects. Two primary outcomes are examined: the Business Confidence Index and FDI as a share of GDP, alongside four expanded indicators: Greenfield FDI, gross fixed capital formation, ten-year sovereign yield spreads, and the ICRG Political Risk Index.

The headline result is that lobbying transparency reforms generate no statistically significant or correctly signed improvement in business confidence or FDI across any specification, estimator, or time horizon examined. This null pattern holds across all six outcome variables and all post-adoption horizons from $t + 0$ to $t + 10$, and survives the full battery of robustness checks. Mechanism decomposition confirms that the causal chain fails at its first link: mandatory register adoption does not produce a consistent positive shift in the perception- and risk-based indicators through which any investment response would need to be transmitted. These findings are not marginal or dependent on the specification; they are robust, consistent, and directly at odds with the economic proposition that international organisations have advanced with growing institutional confidence.

International organisations are currently directing reform advocacy, technical assistance, and institutional credibility toward an economic case this study finds unsupported by causal evidence, which carries consequences. Registers designed primarily to signal institutional credibility are unlikely to deliver on the promised integrity dividend, and reforms justified on economic grounds are rendered institutionally fragile when those returns fail to materialise. Yet, the normative democratic case for lobbying transparency, grounded in citizens' right to know who shapes decisions made on their behalf, remains undiminished. This study recommends that national governments and international organisations decouple reform advocacy from macroeconomic rationales unsustained by the evidence base, and that registers are redesigned to meet the disclosure standards that democratic accountability, rather than investment signalling, actually requires.

The study takes the following structure. Section 2 builds the theoretical framework, establishing why lobbying transparency should generate economic returns under certain conditions and why existing registers may fall short of them. Section 3 describes the empirical strategy used to test that framework across 24 OECD countries between 2000 and 2024. Section 4 presents the results, which are null across all specifications, estimators, and outcome variables. Section 5 diagnoses why, tracing the failure to the first link in the causal chain, where register adoption fails to shift investor perceptions of governance quality. Section 6 draws the conclusions, situates the findings within the broader literature, and identifies the conditions under which future reforms might produce detectable effects. Section 7 translates the diagnosis into three policy recommendations on justification, design, and institutional architecture, addressed to international organisations and to the three national government case studies of Chile, Sweden, and Canada.

2. LITERATURE REVIEW

This section reviews the literature to develop the theoretical framework for this study's empirical expectations. It covers five substantive fields to theorise the perceptual mechanisms through which governance reforms have the potential to shift the beliefs of private sector actors. Each field is explored insofar as it contributes to a coherent two-link framework, where lobbying transparency reforms influence business confidence as part of Link 1, and by means of those shifts in perceptions, foreign direct investment is influenced in Link 2. To synthesise, the section concludes by addressing a critical research gap from which the theoretical framework is derived.

2.1 Institutions, Credibility, and Investment

New Institutional Economics (NIE), defined as “the rules of the game in a society,” contends that institutions determine the incentive structure within which economic actors operate, whether it be through formal constraints or informal norms (North, 1990, p. 3; Rodrik, Subramanian and Trebbi, 2004). Since institutions reduce uncertainty and lower transaction costs, they ensure that economic exchange is feasible at scale. Under these conditions, NIE holds that the quality of institutions, beyond their mere existence, has an immediate and profound influence on the investment, innovation, and growth that an economy experiences. Institutions that are weak, ambiguous, or limited in their enforcement tend to increase transaction costs, which undermines property rights and diminishes the returns to productive investment relative to rent-seeking alternatives (North, 1990; Acemoglu, Johnson and Robinson, 2001; Rodrik, Subramanian and Trebbi, 2004). The implication is that countries with stronger institutional frameworks should, all else equal, attract more investment.

Within the broader framework of NIE, credible commitment connects directly to the signalling mechanism analysed in this thesis. North and Weingast (1989) argue that it is not only the quality of formal rules, but also the credibility with which governments are believed to honour them, which influences investment. This historical-institutional case study claims that before 1688, the English Crown's discretion to revise property arrangements, default on sovereign debt, and override contract rights consistently caused market failures. Only investors would discount the

value of government commitments for the fact that no institutional mechanisms prevented their reversal. Pushed to reform, the English Crown established parliamentary sovereignty, independent judicial enforcement, and limited royal prerogative. These constitutional amendments delivered results not merely by changing the formal rules, but by making elite discretion politically costly to exercise. As a result, structural improvements in sovereign creditworthiness and domestic capital market activity followed (North and Weingast, 1989, pp. 826–829). This mechanism is generalisable: when political elites constrain their own discretion in verifiable ways, such as embedding institutions with sufficient authority, the credibility of their commitments increase, sending signals of institutional self-restraint that reduces perceived political risk.

The logic behind the problem of credible commitment is extended by Acemoglu, Johnson and Robinson (2005) to assert that the variations in the quality of economic institutions, particularly their security of property and rule of law provisions, explain their cross-country income divergences in the long-run. This instrumental variable study sought to exploit the phenomenon of European colonisation to construct an instrument for institutional quality, which in turn provides compelling evidence that the relationship between institutions and economic growth is not merely correlational, but also causal (Acemoglu, Johnson and Robinson, 2005, pp. 391–412). The present implication is that institutional quality is an active determinant of investment decisions, rather than merely a contextual condition, and operates through the rational expectations of firms and investors about the security and enforceability of their claims.

The corruption literature investigates the responses of firms and investors to institutional consolidation and decline. Studies using bilateral FDI data demonstrate that host-country corruption has a quantitatively negative effect on investment inflows (Busse and Hefeker, 2007), noting an increase in corruption from the level of Singapore to that of Mexico is equivalent in deterrence effect to raising the corporate tax rate by 50 percentage points (Wei, 2000). This finding is reinforced at the firm level by Smarzynska and Wei (2002) and Egger and Winner (2005), providing empirical support for the claim that corruption not only reduces FDI, but distorts its composition, which compels foreign investing firms to partner with a local company

instead of investing alone. Beyond deciding whether to invest, this means corruption changes how investors do so.

These empirical findings on the relationship between reforms and investment are consolidated by Méon and Sekkat's cross-country analysis (2005), advancing that corruption consistently hinders, rather than facilitates, the wheels of growth and investment, and that this effect is intensified when the quality of institutions is low. Aidt (2009) then reviews this literature comprehensively, coming to the conclusion that the evidence that corruption enhances efficiency is weak, while it has a robust negative effect on sustainable development, by means of the decline in institutional quality and confidence in the capital market. An important qualification is raised by Mungiu-Pippidi, who argues that in contexts where particularism and clientelism are the prevailing norms, the introduction of formal anti-corruption rules tends to produce cosmetic compliance rather than genuine behavioural change, unless broader normative and enforcement equilibria are transformed. From this perspective, the effectiveness of integrity reforms as credibility signals depends not only on their legal design, but also on whether societies have already moved towards an impersonal, rule-based equilibrium in which such rules are expected to be enforced.

In framing the approach of this study, the literature on institutions, credibility, and investment demonstrates that lobbying transparency reforms can function as a credibility signal that both reduce investor uncertainty and encourage capital inflows. It has been shown that when the relationship between political elites and organised interests is non-transparent, the perception of institutional risk increases. Although, reforms that render the relationship more legible and constrained ought to, in principle, reduce the aforementioned risks. This raises a further question: does adopting disclosure registers that mandate lobbying transparency constitute a sufficiently credible and visible signal to generate shifts in the perceptions of private sector actors? In the following section, the theoretical mechanism through which such a signal would operate is developed.

2.2 Signalling Theory and Credible Commitment

Originally outlined by Spence (1973) in the context of labour markets, signalling theory is the theoretical mechanism through which the effects of lobbying transparency reforms on investor behaviour are best understood. This foundational contribution demonstrated that under conditions of informational asymmetry, or when one party holds information that is private and relevant but unobservable to another, observable intentions or actions can convey informative signals. Importantly, this theory only holds true under the condition that the cost of producing the signal is negatively correlated with the underlying quality being communicated. If the signal can be produced by all actors at equal cost, it carries no informative value and rational observers will discount it accordingly (Spence, 1973, pp. 358–360). A credible signal must therefore be prohibitively costly to mimic, such that only actors with a genuine underlying commitment, or governments genuinely committed to constraining elite discretion, find it worthwhile to send.

As applied to governance, the logic behind signalling theory connects directly to the credible commitment framework developed by North and Weingast (1989). It was established in Section 2.1 that beyond the immediate issue that institutional quality is uncertain, prospective investors must also grapple with governments having ex post incentives to renege on commitments made ex ante. Once investment is sunk and political pressures shift, a government that initially promised to protect property rights, enforce contracts, and limit the influence of vested interests may nonetheless abandon those commitments. This time-inconsistency problem, when perceived by rational investors, leads to the discounting of the value of governmental promises, unless they are backed by institutional arrangements that impose real costs on reneging. The adoption of constitutions, independent judiciaries, and in particular mandatory disclosure requirements, can constrain the discretion of the very actors whose promises would otherwise be unverifiable. In this framework, mandatory lobbying registers install a de jure constraint on opacity through which undue influence is exercised. By imposing consistent and verifiable disclosure obligations for political actors, the reversal of integrity commitments becomes politically costly.

The literature on the application of signalling theory to the institutions-investment intersection is well-developed. Jensen (2003) uses cross-sectional and time-series data for over one hundred

countries to demonstrate that democratic political systems attract significantly higher FDI inflows than non-democratic counterparts. This finding is attributable to the credibility that democratic constraints on political elites provide to foreign investors (Jensen, 2003, pp. 587-616). Analogous to the signalling mechanism, democratic institutions protect the property rights that follow investments, but also signal ahead of time the limited ability of the government to expropriate or alter the terms of investment unilaterally (Büthe and Milner, 2008). Jensen (2008) then extends this analysis to demonstrate that political risk is a core determinant of FDI allocation decisions. The risks taken by host governments, particularly those acted on arbitrarily or capriciously, can be reduced by embodying democratic institutions with the faculties to implement policy reforms that are transparent, predictable, and more costly to reverse.

This argument is carried further by Fang and Owen (2011) into the domain of international institutions, arguing that the membership bids for non-democratic countries to join international organisations represent a costly commitment device. Insofar as these countries remain members of these international organisations, foreign investors receive signals that governance standards will be maintained, at the cost of reputational and material consequences from peer countries (Hallward-Driemeier, 2003). Zheng (2011) then provides cross-country evidence to show a nonlinear relationship between institutional constraints and FDI in developing countries. This suggests there is an optimum between too few veto players generating policy uncertainty, and too many generating policy rigidity, at which credibility and flexibility are jointly maximised. A boundary condition on this mechanism is established by Hallward-Driemeier, who shows that bilateral investment treaties, or formal commitment devices that are structurally analogous to mandatory lobbying registers, function as complements to, rather than substitutes for, underlying institutional quality; only generating significant FDI gains where domestic governance frameworks are already reasonably robust (Hallward-Driemeier, 2003). These studies on the applications of signalling theory establish that in addition to their mere existence, the credibility of government institutions is the mechanism through which political systems influence foreign investment outcomes.

However, the literature on signalling theory also advances the conditions under which the intended informational effects are not produced by credibility signals. Synthesising the literature

across strategic management and political economy, Connelly et al. (2011) identify three modes of signals that fail: signals that do not cost enough to reverse; signals that do not reach the intended audience; and signals whose informational value is diminished by signal inflation. The latter implies the proliferation of the same across actors of varying quality drives its equilibrium informativeness towards zero (Connelly et al., 2011, pp. 39–67). Relevant to mandatory lobbying registers, these signal failures are built on by Thyne (2006), who distinguishes costly from cheap signals by their potential to durably alter the beliefs of an audience of rational actors. Where signals can be generated without genuine sacrifice, they are dismissed as noise rather than received as information, which dissolves the intended responses to the signal. This applies directly to lobbying transparency, because registers that only disclose the identities of lobbyists, and do not mandate the disclosure of legislative targets, financial volumes, or beneficial ownership, do not constitute costly signals as outlined by Spence (1973) as the informational content of the reform is too thin. There is no basis for investors to revise their risk assessments if they cannot verify the quality or enforcement of lobbying register commitments.

In sum, the literature on signalling theory and credible commitment naturally generates the two-link causal chain for the empirical design of this study. First, perceptions of firms and investors should shift to the extent that mandatory lobbying registers represent costly signals of institutional commitment (Link 1). If that perceptual shift takes place and is of sufficient magnitude, investment and capital allocation decisions should be revised in favour of the host country (Link 2). Thus, the literature produces clear and sequential empirical expectations, in addition to the conditions under which those expectations fail.

2.3 Lobbying, Undue Influence, and Capture

Having established the theoretical framework for institutional signals to credibly reduce informational asymmetry for investors, the literature as it applies to the specific domain of lobbying regulation will be examined in three arguments. First, the intention of firms and business associations to pursue political influence is structurally embedded in democratic governance, as opposed to a market imperfection amenable to limitation. Second, competition between interest groups systematically creates unbalanced outcomes that fosters the conditions

for regulatory capture. Third, although regulatory capture produces market distortions, these are only secondary to the perceptions of undue influence, connecting this literature to the signalling mechanism under question.

The foundation of interest group activity in democratic politics is Truman's (1951) pluralist framework, which holds that claims made by interest groups are a normal feature of contestation on governmental institutions. In turn, the competitive interaction between interest groups in the market of legislative influence approximates a form of democratic representation. This claim was systematically questioned by Olson (1965), advancing his own deductive framework of collective action that privileges small, concentrated interest groups with clear and immediate interests over large, diffuse publics whose welfare is affected, but who face prohibitive organisational costs. It follows that systematic misrepresentation of broader interests occurs, as interest groups driven by economic motives, such as large firms and industry associations, become structurally advantaged in their capacity to mobilise lobbying resources relative to unorganised groups or the general public. Through a deductive model, Becker (1983) confirms that due to competition among pressure groups, political equilibrium does not reflect the aggregate preferences of citizens, but rather the relative efficiency of competing interest groups in generating political pressure. Denzau and Munger (1986) formalise this argument, showing that unorganised interests receive legislative representation only when it is electorally convenient for legislators to provide it, which means that making lobbying activities visible, precisely what lobbying registers do, is a precondition for unorganised constituencies to hold legislators accountable at all.

Extending the political economy of lobbying to trade policy, the literature shows that interest groups can secure regulatory outcomes that are directly contrary to aggregate welfare. A seminal model proposed by Grossman and Helpman (1994) demonstrates that by maximising a welfare function that weights both aggregate welfare and contributions from organised interests, governments adopt regulation schedules that systematically favour politically organised industries above consumers and unorganised industries. This model applies directly to trade policy, as domains that will translate private interests through financial and organisations' resources into political access will diverge public policy from outcomes that maximise broader

welfare; with the extent to which these outcomes diverge mirroring the advantages held by the interest groups. Formally, this constitutes the theoretical substrate of regulatory capture, or the transformation of public authority into a vehicle for private interests. Helpman and Persson (1998) extend this logic to legislative bargaining, showing that the degree to which lobbying distorts policy depends on institutional features such as veto players, electoral rules, and agenda control, which explains why the effect of transparency reforms should be expected to vary across the 24 OECD democracies examined in this study.

In comparative politics literature, regulatory capture is not a remote theoretical concern, but a widely perceived risk among public sector actors in advanced democracies. This is substantiated by Chari, Murphy and Hogan (2007), showing substantial cross-country variation in the legal frameworks that govern lobbying activities; evidence that the concept that regulatory design is itself contested terrain. It follows that the definition of lobbying, the threshold for disclosure, and the enforcement mechanisms adopted, all reflect those competing pressures. In the European context, Chalmers and Santos Macedo (2021) show that lobbying activity is positively associated with firm profitability, confirming that the economic rents at stake in OECD democracies are substantial enough that reforms altering the disclosure environment should plausibly register at the aggregate level. Their framework is then extended globally (Chari, Hogan and Murphy, 2010), reporting the lack of lobbying regulation, or the maintenance of legal frameworks that are too thin, for most democratic systems to provide meaningful accountability. Consistent with this concern, Bertrand, Bombardini, and Trebbi (2014) demonstrate that lobbyists' value derives more from whom they know than from what they know, suggesting that mandatory disclosure of meetings and client portfolios directly erodes the rents attached to opaque political access, which is the channel on which the costly-signal mechanism depends.

For this study, the critical conclusion of this literature is not the material consequences of lobbying per se, but rather the perceptual consequences of its opacity. These unobservable interactions between interest groups and government officials raise the risk that policy commitments reflect private negotiations rather than enforceable public rules. In the technical sense, the opacity differs from corruption, but as it relates to policy processes, it remains functionally analogous in its consequences for investor confidence (OECD, 2024). For example,

as 77 percent of firms globally perceive that close relationships between private sector and public sector actors distort market competition (World Bank, 2025), investors place a premium on governance frameworks that visibly constrain elite discretion. These premiums, in particular, are the normative basis for which reforms of lobbying frameworks are adopted, and whose reduction constitutes the theoretical basis for the signalling expectation developed in Section 2.2.

2.4 Lobbying Transparency Regulation

Since the turn of the century, the regulation of lobbying transparency has emerged as a well-defined field of governance studies from North America to encompass most OECD democracies and increasingly into emerging and transitional democracies.

Chari, Murphy and Hogan (2007, 2010) conducted the foundational comparative study, demonstrating that the regulatory frameworks of lobbying vary across countries on four dimensions: the definition of the act of lobbying; the scope of actors required to disclose their activities; the breadth of information required for these disclosures; and the sanctions that result from non-compliance, including the enforcement mechanisms to dissuade non-compliance. From transitional states to established democracies, the analysis shows that frameworks suffer most from their definitional narrowness and enforcement weakness, which tend to be characteristics that uphold the formal appearance of regulation, while preserving the substantive opacity of the relationship between interest groups and governments. Laboutková, Šimral and Vymětal (2025) extend this diagnosis, arguing that a lobbying register adopted as a standalone legislative act constitutes a category error. For transparency to deliver genuine informational value, a broader architecture of governance instruments must be adopted and operate in mutually reinforcing relation, including FOI provisions, legislative targeting disclosures, conflict of interest disclosures, and open government data standards. The theoretical stakes of correctly designing these architectures are considerable: Cotton and Déllis (2016) show that lobbyists who possess private and relevant information can use that informational advantage to skew the legislative agenda, and that mandatory disclosure reduces this advantage by making the information partially visible to rival interests and voters.

Globally, the OECD has been established as the principal standard-setter of anti-corruption and integrity policy frameworks. In 2010, the *Recommendation on Principles for Transparency and Integrity in Lobbying* was adopted, denoting the first international instrument to outline normative guidelines on the governance of lobbying frameworks for member countries. These guidelines were supported by the release of the *Lobbying in the 21st Century Report* in 2021, measuring implementation progress and identifying recurring deficits. Looking ahead, guidelines on how firms can lobby responsibly will be published in 2027. While member countries have improved stakeholder access and certain disclosure mechanisms, transparency measures have fallen sharply relative to the increase in the sophistication of lobbying activities, noting a lack of institutional infrastructure for enforcement (OECD, 2021). The *Anti-Corruption and Integrity Outlook 2026* reinforces this assessment: 67 percent of OECD member countries maintain lobbying registers, and 61 percent provide a definition of lobbying in the primary legislation, although there remains a 19 percentage point gap between de jure adoption and de facto implementation. Disclosure of the specific legislation being targeted is required by less than one-third of registers, with disclosure of lobbying budgets at 18 percent, and beneficial ownership transparency declining from 42 to 17 percent of member countries since 2022 (OECD, 2026).

To account for these deficiencies, a systematic comparative analysis of sixteen lobbying registers across three dimensions is conducted by Katsaitis (2024), noting most registers underperforming in terms of transparency, regulatory capacity, and interoperability, with the greatest shortcomings in regulatory capacity and cross-referencing with related governance data. This is supported by the OECD Public Integrity Indicators, offering a cross-national baseline on regulations and practices against which adequate disclosure frameworks can be assessed, including lobbying, conflict-of-interest management, and political finance. This research is corroborated by Lobbying in the Sunlight consortium's scoping review, which develops fifty indicators and a Framework for Open, Comprehensive, and Accessible Lobbying (FOCAL) on the accessibility of disclosure frameworks. The results show that disclosure requirements typically obscure the financial, relational, and legislative dimensions that are relevant to accountability, highlighting only the most visible and least politically sensitive elements.

Drawing on this literature, two implications are relevant for this study. First, variations across the OECD in the quality of member country registers is directly relevant to the signalling mechanism developed in Section 2.2. The costly signal is not effective when mandatory registers are narrow in scope, weakly enforced, or de facto non-operational, because they do not impose real constraints on political elites and by extension do not credibly communicate the commitment of institutions to prospective investors. These findings provide the empirical basis for the signalling mechanism investigated in this study. Second, the absence of previous cross-country causal studies evaluating the investment effects of lobbying transparency reforms forms the research gap that is addressed in this study.

Thus far, the closest precedent in the broader literature is Drabek and Payne's (2002) quantitative cross-country analysis showing that the lack of transparent institutions, a composite between policy uncertainty and informational opacity, reduces FDI inflows to a great extent for a broad global sample of countries; noting a one-unit improvement in transparency corresponding with an average of 40 percent increase in FDI inflows. A parallel line of evidence from finance establishes the converse mechanism: Fisman (2001) finds that firm value falls sharply when political connections weaken, and Faccio (2006) documents systematic premia on connected firms across 47 countries, together demonstrating that opacity in political access is actively priced by markets and that reforms compressing that opacity should, in principle, register in aggregate investment and risk indicators. However, by measuring broad governance transparency, rather than the narrow and policy relevant signal produced by mandatory lobbying registers, the signalling effect is not isolated, which to date, remains an untested empirical proposition – and the gap this study seeks to fill.

2.5 Governance, Investment, and Institutions

The development of the theoretical mechanism in Sections 2.1 and 2.2 endows a critical role to the perceptions of private sector actors; for reforms to generate investment outcomes, they must first alter assessments of institutional credibility. This intermediate perceptual mechanism, or the process by which signals are received, assessed, and translated into revised expectations, falls into the rich literature on political trust. In addition to providing the micro-foundations for Link 1

of this study's causal chain, this body of literature contains the most direct empirical evidence on whether lobbying transparency shifts the attitudes of these actors. Before turning to that evidence, Carstens (2023) provides useful conceptual scaffolding by theorising political trust as a multidimensional construct spanning process, institution, and output legitimacy, which clarifies why movements in the WGI sub-indicators used here, as Control of Corruption and Rule of Law, can reasonably be interpreted as trust-based responses to lobbying-transparency reforms rather than as direct measures of underlying institutional behaviour.

Theories of the determinants of trust in political institutions are broadly divided into two competing traditions: cultural and institutional theories (Mishler and Rose, 2001). Cultural theories delineate trust of political institutions as exogenous. Through early socialisation, a stable disposition of trust is transmitted across generations, and largely impervious to short- to medium-term changes in policy platforms or government behaviours (Mishler and Rose, 2001). Lobbying transparency reforms would thus be inconsequential for trust formation, as the cultural mechanism disregards discrete policy interventions within the timeframes relevant to investment decisions. On the other hand, institutional theories interpret trust endogenously, in the sense that a rational evaluation of government performance, integrity, and responsiveness updates continuously in response to observable institutional behaviour. Because this framework treats governance quality as something citizens and market actors actively infer from observable signals, it provides the theoretical foundation for the signalling hypothesis: that private sector actors update their rational assessments of governance quality when they observe credible institutional constraints being imposed on political elites, which increases their trust in public institutions. While it remains contested which policy interventions and their timeframes are capable of generating responses in trust, the empirical balance of evidence favours the institutional theory (Mishler and Rose, 2001; Norris, 2011; Rothstein and Teorell, 2008).

The literature on political trust in public institutions is operationalised by the OECD's analytical framework, which distinguishes two broad categories of drivers. First, competence-based drivers determine the quality, reliability, and responsiveness of the delivery of public service. Second, values-based drivers determine the perceptions of government integrity, fairness, and openness. Only 39 percent of OECD countries achieved high or moderately high trust in their national

government, displaying an annual decline of 2.4 percentage points annually since 2021 (OECD, 2024). To mitigate the associated risks, the OECD identifies perceived government integrity, defined as the absence of or low corruption, capture, and preferential treatment, as the most consistent and robust treatment to increase trust in national government (OECD, 2024); even out performing satisfaction with public services, as a predictor of institutional confidence. The theoretical mechanism is therefore established, from reforms that adopt lobbying transparency measures to the integrity dimension of trust, insofar as the reforms render the relationship between interest groups and governments more legible and constrained. Whether this targeting is sufficient remains to be seen.

However, the literature on governmental transparency and political trust outlines a paradox central to the theoretical mechanism: that the relationship between disclosure and trust is weaker, more conditional, and less linear than policy advocates assume. Grimmelikhuijsen et al. (2012) use experimental research methods to show that, contrary to intuition, greater transparency can reduce the perception of government competence by exposing the complexity and disorder of actual decision-making processes. In turn, ideal expectations of rational and orderly governance are confounded for private sector actors. This claim is further developed by Grimmelikhuijsen et al. (2014) to demonstrate that trust is affected by transparency through a modulation of national cultural context, and that power-distance values condition how disclosure is perceived. This scholarship evidences the structural limitation that transparency by itself does not automatically generate trust, as the interpretation of disclosed information depends on pre-conceived beliefs. The same disclosure may be perceived by actors with deep-rooted institutional skepticism as confirming, as opposed to refuting, prior assessments of elite capture. This limitation is consistent with Andrews' account of isomorphic mimicry (2013), which argues that reforms often generate the outward appearance of compliance with international good-governance standards without altering underlying incentive structures, meaning that transparency reforms can be observed, yet rationally discounted by audiences if they are not accompanied by credible enforcement.

Most directly relevant to this paradox is the experimental evidence provided by Crepaz and Arikan (2024), who test the effects of three separate transparency interventions on political trust

and the perceptions of corruption. This study centres around an agency-theoretic framework, where information asymmetry is reduced between private sector actors (principals) and political actors (agents) with the introduction of transparency measures, which updates assessments of trustworthiness and integrity. Administering three treatments on political financing, asset declarations, and interest group disclosures, the authors make differentiated and theoretically significant findings: when political donations are transparent, trust in political parties improves; when MPs declare their assets, perceived corruption decreases; and most critically, when interest groups disclose their lobbying activities, attitudes towards these groups did not change. This null result is interpreted by Crepaz and Arikan through the lens of audience effects: that private sector actors' perceptions of interest groups and their lobbying activities have a deeply embedded skepticism that is not responsive to informational interventions of this reform type. Instead, the disclosure of information that lobbyists are active may entrench negative assessments held previously, rather than dispel them.

The literature on the relationship between governance, investment, and institutions carries substantial theoretical weight for this study. Notionally, the perceptual mechanism that connects reform to firms and investors (Link 1) faces a structural challenge even before attaining the investment decisions of Link 2, if these actors do not update their assessments of institutional integrity in response to direct and experimental exposure to information about lobbying transparency regulation. Acknowledging the findings of Crepaz and Arikan (2024) does not preclude the signalling mechanism from applying to sophisticated private sector actors. Although, the micro-level evidence for the argument that lobbying transparency is distinct from other accountability reforms in its ability to alter trust is compelling. At the aggregate level, this is also consistent with the evidence of declining institutional trust across OECD democracies, in spite of the proliferation of lobbying registers (OECD, 2024). Therefore, should the empirical analysis of this study reveal a null result, it may not reflect a failure of the research design, but a feature of lobbying transparency as a category of reforms: the signal it produces, although credible, is insufficient to shift the pre-determined beliefs of rational private sector actors.

2.6 Synthesis: Research Gap and Hypotheses

Altogether, the five bodies of literature converge on a two-link theoretical framework that explains the conditions necessary for lobbying transparency reforms to produce observable effects on investment outcomes.

Under the broader framework of NIE, transaction costs and uncertainty can be reduced by institutions and their credible commitments to governance, beyond their mere formal existence, to shape investment behaviour (North, 1990; North and Weingast, 1989). Signalling theory specifies the three necessary conditions for governance reforms to function as a credible commitment device: that reforms impose real and observable constraints on the discretion of the signalling actor; that costs are imposed that cannot be mimicked by actors without genuine commitment; and that reforms are perceived as informative by the relevant audience (Spence, 1973; Connelly et al., 2011).

For lobbying transparency reforms specifically, the first condition is met by the mandatory nature of the register, upheld with legal enforcement and meaningful disclosure obligations. Meeting the second and third conditions poses an empirical question that is yet to be answered in the existing literature. The body of literature on lobbying regulation asserts that most OECD registers do not meet the costly signal threshold by falling short on the constraints imposed on elite behaviour, due to their weak scope and narrow enforcement (OECD, 2026; Katsaitis, 2024). Whereas, the political trust literature culminates with the experimental findings of Crepaz and Arian (2024) to state that reforming lobbying transparency, even when implemented properly, fails to shift the perceptions of relevant audiences because the signal may not be perceived as credible.

Building on this theoretical foundation, the signalling mechanism produces two sequential and empirical hypotheses. If the costly signal condition is met and interpreted as credible evidence of constrained elite behaviour by firms and investors, the perceptions held by investors on political risk and institutional quality should improve after reforms, denoting Link 1 of the theoretical framework. The determinants of trust set out by the OECD identifies perceptions of government

integrity as the central determinant of institutional confidence (OECD, 2024), and Jensen asserts that constraints on elite discretion that are politically observable reduce the likelihood of investors choosing a risk premium. Therefore, a lobbying register that credibly constrains the relationship between interest groups and governments has a plausible theoretical mechanism to measurably improve investor confidence. This means investment outcomes should respond in the medium-term if Link 1 operates as theorised, because less institutional uncertainty lowers the risk-adjusted cost of investment in the reforming country, attracting a structural increase in FDI inflows (Jensen, 2003; Zheng, 2011), which denotes Link 2 of the theoretical framework.

Following these expectations, the two hypotheses for this study are generated:

H1: The adoption of a mandatory, legally enforceable lobbying register will cause an immediate and statistically significant increase in Business Confidence Indices (BCI) in reforming countries, relative to the non-reforming control group.

H2: Countries with sound implementation of these reforms will sustain a higher rate of FDI inflows (as a percentage of GDP) in the post-reform period ($t + 2$ to $t + 5$), affirming structural shifts as opposed to a temporary shock.

Critically, these hypotheses are defined by a theoretically conservative reading of the literature. The reason for this is that the signalling framework alone does not accommodate for a structural qualification raised in the political science literature: that lobbying transparency reforms are categorically distinct from other accountability reforms due to their potential to credibly shift institutional perceptions. Specifically, the conduct of actors nominally constrained by these reforms, of interest groups and governments alike, reinforces a skepticism among observers that is deeply embedded and resistant to informational correction (Crepaz and Arikan, 2024; Mishler and Rose, 2001; Crepaz, 2024). In cases where lobbying registers are thin, unenforced, or perceived as performative, which holds in most instances per the OECD's implementation gap data, the reforms generate cheap rather than costly signals, and rational private sector actors have no basis for revising their priors. Therefore, the theoretical framework anticipates the possibility of null results at both links in the causal mechanism.

3. DATA, SOURCES, AND METHODOLOGY

3.1 Research Design Overview

In this section, the theoretical framework and hypotheses developed in the literature review will be operationalised into a tractable empirical strategy. The research design seeks to evaluate whether the adoption of mandatory lobbying registers functions as a credible institutional signal that shifts business confidence (H1) and subsequently attracts FDI inflows (H2). Drawing on the theoretical traditions of Spence's signalling theory (1973) and North and Weingast's credible commitment framework (1989), sequential empirical predictions are generated: the adoption of reforms should first shift business confidence (Link 1), which then should produce a structural increase in FDI inflows (Link 2). To design an empirical test of these hypotheses, there must be a causal attribution of outcome changes to a policy intervention, which accounts for the confounding influence of macroeconomic conditions and pre-existing institutional trajectories.

This raises a fundamental identification challenge: that lobbying transparency reforms are not assigned arbitrarily, and countries the pre-existing and systematic differences between countries adopting these reforms from those that do not are correlated with the outcomes of interest, as shown in the balance table (Table 1). Using a cross-sectional regression would thus confound institutional selection with reform effects. To mitigate this, this study employs a staggered difference-in-differences panel design that exploits natural variation in the timing of the adoption of reforms, across 24 OECD democracies between 2000 and 2024. This approach is consistent with that of the broader comparative political economy literature on governance and investment (Jensen, 2003; Zheng, 2011), and compares within-country changes before and after treatment against the counterfactual estimates from non-reforming countries to identify reform effects; with country fixed effects absorbing time-invariant heterogeneity, and year fixed effects absorbing common temporal shocks.

3.2 Sample Construction and Panel Design

3.2.1 Country Selection and Inclusion Criteria

The sample includes 24 OECD member countries from 2000 to 2024 observed annually, creating a total of 600 country-year observations, excepting certain instances of missing data. To be specific, three criteria form the basis for the country selection. First, the countries must have sustained OECD membership throughout the observation window for there to be institutional comparability. This means each of the countries share foundational standards of democratic governance, rule of law, and macroeconomic data collection that would not hold in a globally representative sample (OECD, 2021). Second, the countries must be included in the OECD BCI dataset for the entire study period, as this forms the primary dependent variable. Third, the data for all the outcome and control variables, as shown in Table A1.1, must be adequately covered to ensure that the estimation is not compromised by systematic gaps in one country group relative to another.

Restricting the sample to OECD member countries was a deliberate design choice that would serve as a theoretical boundary condition, not merely a data constraint. As the signalling theory mechanism requires that the discretion of elites is restricted for governments to be credible, precisely because accountability structures make reversal politically costly, having non-democratic or transitional states in the sample would conflate the signalling channel with broader regime-type effects. This would introduce endogeneity that cannot be resolved through fixed-effects controls alone. Thus, the sample consists of cases for which the theoretical mechanism is most plausible; a necessary condition for a valid test of the signalling hypothesis.

3.2.2 Country Groups and Treatment Classification

Within the sample, the 24 countries are partitioned into three mutually exclusive groups of eight based on their relationship to the treatment window between 2000 and 2024:

“Reformers” refer to OECD member countries that have adopted mandatory, legally enforceable lobbying registers during the observation period, as defined by the OECD (2024). In the staggered design, these represent treated units, with the timing of their adoption serving as the identifying variation. Table A2.1 and Figure A2.1 show country names and reform years.

“Non-reformers” refer to OECD member countries that have not adopted a mandatory, legally enforceable lobbying register as of 2024. These countries represent the untreated control group, providing the counterfactual for the staggered estimator.

“Established” refers to OECD member countries that have adopted mandatory, legally enforceable lobbying registers before 2000, making them right-censored with respect to the treatment window. These countries are incorporated to serve as a secondary control group for the robustness analyses (see Section 3.7.3), determining the sensitivity of the results to the composition of the control groups.

3.2.3 Observation Window and Panel Balance

The observation window was set from 2000 to 2024 for two central reasons: first, to maximise post-treatment observation years for countries adopting reforms during the study period; second, to capture the pre-reform developments necessary for the parallel trends assessment. Two major macroeconomic shocks that took place throughout this window that warrant attention, including the Global Financial Crisis (2007 to 2009) and the COVID-19 recession (2020 to 2021). While the estimation strategy absorbs these events as fixed year effects, there are also some data unavailability constraints in a small number of country-years. Where a required variable is missing, observations are excluded listwise. As shown in Table A1.1, there are 574 total observations for country-years for the BCI specification, and 576 for FDI as a share of GDP specification.

3.3 Treatment Variable

3.3.1 Definition and Coding Protocol

The definition of the treatment variable is the adoption of mandatory, legally enforceable lobbying registers in primary legislation, that require the disclosure of the identity of the lobbyists, their clients, or both, as a precondition for lawful lobbying activity (OECD, 2021). The distinction between mandatory lobbying registers that are upheld by legal enforcement, from voluntary disclosure arrangements, is analytically significant. For the theoretical signalling mechanism to operate, a costly signal must impose real constraints on the signalling actor and cannot be costlessly mimicked by actors without genuine commitment (Spence, 1973; Grittersová, 2014). Voluntary registers with binding obligation and no sanction for non-compliance do not constitute a credible commitment, in the sense of North and Weingast (1989). This implies no sacrifice of discretion is required, which fails to distinguish governments committed to integrity from those engaging in reputational window-dressing. Consistent with standards applied in the OECD's comparative assessments of lobbying frameworks, the coding threshold for this study is thus set at mandatory and enforceable provisions in the primary legislation (OECD, 2021; Transparency International, 2024).

To code the reform adoption dates, two sources are used: the *Lobbying in the 21st Century Report* (2021) and the *Anti-Corruption and Integrity Outlook* (2026), both published by the OECD. These publications systematically catalogue the lobbying regulations of all OECD member countries, updating and cross-validating with the cooperation of national agencies. The year in which the mandatory register legislation took legal effect is defined as the treatment year E_i , not the year of parliamentary approval, as this aligns the treatment date with the point at which the institutional signal could first be received and processed by private sector actors.

3.3.2 Binary Treatment Indicator and Absorbing Treatment Assumption

The treatment variable is coded as a binary indicator D_{it} . It is equal to 1 in total years from when the reform took place and onwards, and 0 in total preceding years.

$$D_{it} = 1[t \geq E_i]$$

The treatment specification is absorbing, in the sense that once a mandatory register is adopted, it remains active for all subsequent periods. As no country removed their lobbying register during the observation window, the absorbing assumption reflects the institutional reality.

This binary measure, as opposed to the intensity-based measure, is adopted at the primary specification level for two reasons. First, it provides data on register quality that is consistent across national legal systems, including disclosure depth scores and enforcement capacity indices, that are not available at annual frequency for all countries across the entire observation window. Regrettably, the OECD only reports implementation indicators at survey intervals and with significant data gaps in earlier years. Second, to employ a first-pass test of the signalling mechanism, the binary indicator is theoretically appropriate. This holds true because even if mandatory register adoption is not detected by private sector actors, an intensity-based approach would improbably improve those results. Understanding whether the depth of reforms conditions the effect is assessed and shown in the heterogeneity analysis (Section 4.4), which tests these differential effects by cohort and by baseline institutional quality.

3.3.3 Staggered Adoption and Identification

The staggered adoption structure is enabled by the eight Reformers adopting mandatory lobbying registers at different points throughout the observation window. Hence, the identifying variation of the causal estimates is temporal. In this staggered design, the Two-Way Fixed Effects estimator constructs a weighted average of all possible 2×2 difference-in-difference comparisons. The comparisons occur between the Reformer cohort and the control groups at the time of each adoption event. According to Goodman and Bacon, if treatment effects are heterogeneous across

cohorts or time, negative weights may be assigned to some comparisons, which creates Two-Way Fixed Effect estimates that do not correspond to any interpretable average treatment effect (2021). In the staggered case, controls may constitute the units that have already been treated, introducing ‘forbidden comparisons’ (Callaway and Sant’Anna, 2021; de Chaisemartin and D’Haultfoeuille, 2020). The extent to which these comparisons corrupt the baseline estimates are evaluated in the Goodman-Bacon decompositions (Figure B2.1; Figure B2.2), and further addressed in the Callaway and Sant’Anna group-time average treatment effect estimator, implemented as robustness estimators (Figure 5; Figure F6).

Additionally, the staggered structure enables an analysis that would not be possible under a single-cohort design, which is the assessment of whether treatment effects (or their absence) are consistent across all Reformers, or whether they are specific to a particular cohort or adoption year. This consistency check across cohorts is further developed in the heterogeneity analysis of Section 4.4 and in the leave-one-out sensitivity tests of Section 4.3 (Table D1.1; Table D2.1; Figure 3; Figure 4).

3.4 Outcome Variables

Business confidence and foreign direct investment as a share of GDP are the two outcome variables used in the analysis, corresponding directly to the two links of the theoretical causal chain. The outcome set is then expanded to include four additional variables, with the objective to triangulate the primary findings and to stress-test the null results against alternative measures of the same constructs.

3.4.1 Primary Outcomes

The primary dependent variable used to test H1 is the OECD Standardised Business Confidence Indicator (BCI). As the outcome variable for Link 1, connecting reforms to business confidence, the BCI is developed from opinion surveys on future developments in production, order books, and finished goods inventories. In a four step process, it is standardised across countries with period conversion, smoothing, normalisation, and amplitude adjustment (OECD, 2021), and

aligns each country's confidence series with the fluctuations of the Composite Leading Indicator (CLI) and de-trended GDP. This ensures comparability across countries, placing all country series on a common cyclical scale, which is the main reason for which BCI is preferred as a perception proxy over raw survey balances and producer price expectations. It enables meaningful comparisons both over time and between countries, which is a prerequisite for difference-in-differences estimations within a panel framework.

A limitation of the BCI is that it aggregates manufacturing-sector conditions and does not isolate governance-related confidence from general macroeconomic sentiment. The implication for the theorised signalling mechanism is that any reform signal affecting business perceptions of institutional quality may be diluted by business cycle fluctuations. To address this limitation, the outcome set is expanded to include the ICRG Political Risk Index and ten-year sovereign yield spreads, which are more directly targeted at governance risk assessments.

To test H2, the outcome variable of annual net FDI inflows as a percentage of GDP is used, sourced from the World Bank Development Indicators (World Bank, 2024). This data is then cross-validated against OECD FDI statistics and the UNCTAD World Investment Report (UNCTAD, 2024). By expressing FDI in proportion to GDP, rather than in absolute terms, the differences in scale between the economies in the sample is controlled for. This ensures the measure is stationary over time, which is a prerequisite for valid panel estimation. In terms of the theorised signalling mechanism, FDI inflows capture the capital reallocation response that is expected to follow a credible institutional signal. As these signals decrease informational asymmetry between host-country governments and investors, risk premiums are expected to fall and capital should be reallocated towards the Reformer (Jensen, 2003; Zheng, 2011).

A limitation of the use of FDI inflows as a share of GDP that is well-documented in the literature is that structural outliers, such as Ireland and Luxembourg, record extreme values that are driven by the activities of special purpose entities and multinational profit-shifting (UNCTAD, 2024). To address this limitation, the leave-one-out sensitivity analysis conducted in Section 4.3 demonstrates that the results are not driven by these outlier-driven biases (Figure 4).

3.4.2 Expanded Outcome Set

Following null results across alternative measures of business confidence, political risk, and capital allocation, four additional outcomes are estimated in the same Two-Way Fixed Effects specification to establish robustness. Results for the six additional indicators are reported in Tables F1.1 and F1.2.

Variable	Source	Theory Link	Rationale
Greenfield FDI (USD bn)	fDi Markets, Financial Times	Link 2	Tracks cross-border investment in new projects and expansions, explicitly excluding mergers and acquisitions; provides cleanest measure of productive investment commitment uncontaminated by financial flows (fDi Markets, 2024)
Gross Fixed Capital Formation (GFCF/GDP)	World Bank WDI	Link 2	Captures total investment in fixed assets, including domestic capital formation; tests whether any reform effect extends beyond internationally mobile capital to domestic investment decisions
10-year Government Bond Yield Spread	OECD / Refinitiv	Link 1	Market-priced sovereign risk premium; directly captures how financial markets price institutional credibility and governance risk in government borrowing costs; more forward-looking than BCI
ICRG Political Risk Index	PRS Group	Link 1	Expert-assessed composite of 12 political risk components, scored on a 100-point scale where higher values denote lower risk (PRS Group, 2022); constructed specifically to reflect governance and political stability perceptions rather than business cycle conditions like BCI; more direct test of whether reforms shift risk assessments

3.5 Control Variables

The selection of the four control variables are based on two criteria. First, the variables must constitute theoretically plausible confounders of both reform adoption and investment outcomes. Second, they must be available at annual frequency across the observation window for all countries in the sample. These control variables will isolate the effects of lobbying transparency reforms from the macroeconomic and institutional factors that could drive changes in business confidence and FDI inflows independently of any reform. This avoids over-specification: post-treatment bias that compromises the validity of the treatment coefficient is negated by excluding post-treatment variables or variables that lie on the causal chain between reform and outcomes (Angrist and Pischke, 2009).

The first and most consequential control is annual real GDP growth as a percentage, sourced from the World Bank Development Indicators (World Bank, 2024). As economies experience rapid growth, investment may be attracted independently of institutional reforms, creating conditions more or less conducive to legislative change. The literature substantiates this risk, where economic performance is shown to be the strongest cross-national predictor of FDI inflows (Jensen, 2003). This is further supported by the balance table results, which show statistically significant pre-treatment differences between the GDP growth of Reformers and Non-reformers (Table 1).

The second control is trade openness, calculated as a sum of total exports and imports as a share of GDP, also sourced from the World Bank World Development Indicators (World Bank, 2024). As more open economies have greater exposure to foreign capital flows, there are stronger incentives to adopt international governance norms to maintain access to global markets (Rodrik, 1998). In the balance table, it is shown that Reformers have significantly higher trade openness in the pre-treatment period, necessitating the inclusion of this variable to avoid omitted variable bias (Table 1).

The third and fourth control variables are Control of Corruption and Rule of Law, sourced from the World Bank Worldwide Governance Indicators (WGI). These composite variables are constructed with several hundred individual variables sourced from expert assessments, survey respondents, and NGO data. Using an Unobserved Components Model, they generate margins of error for each country estimate to account for measurement uncertainty (Kaufmann et al., 2010; Kaufmann and Kraay, 2024). Specifically, they control for countries that both attract higher investment and are more likely to adopt reforms, which would otherwise confound the relationship if underlying governance trends are not held constant. To confirm that the baseline results are not sensitive to their inclusion, a robustness test that excludes these controls is used (Table E1.1). Importantly, these variables are first used as controls in the primary specification stage and then outcomes in the expanded analysis, as they are partially endogenous to the treatment, lobbying registers may shift perceptions of the control of corruption (Table E1.1).

3.6 Estimation Strategy

3.6.1 Baseline Two-Way Fixed Effects Model

The baseline Two-Way Fixed Effects regression uses the following estimating equation:

$$Y_{it} = \alpha + \beta D_{it} + \gamma' X_{it} + \mu_i + \lambda_t + \varepsilon_{it}$$

where:

Y_{it} is the outcome (BCI or FDI as a share of GDP) for country i in year t ;

D_{it} is the binary treatment indicator, equal to 1 in all years from the reform year onward;

X_{it} is a vector of time-varying controls (GDP growth, trade openness, WGI Control of Corruption, WGI Rule of Law);

μ_i are country fixed effects;

λ_t are year fixed effects;

ε_{it} is the idiosyncratic error term;

$\beta^\wedge$ is the parameter of interest, which measures the average change in the outcome following reform adoption within countries, relative to the counterfactual trend from the control group.

Country year fixed effects absorb all characteristics of countries that remain fixed over time that could confound cross-sectional comparisons, including legal traditions and structural economic features. Year fixed effects absorb global shocks that affect all countries in each year to ensure that $\beta^\wedge$ reflects the variation within countries that is attributable to the adoption of reforms, rather than contemporaneous global trends (Imai and Kim, 2021). Some of these global shocks include the global financial crisis, the COVID-19 recession, and implementation of governance norms across OECD member countries.

To account for serial correlation within countries, standard errors are clustered at the country level throughout the specification. Given the sample, there can only be 24 clusters, which means country-level clustering may lead to insufficient rejection of the null hypothesis in finite samples. In light of this, two robustness checks using alternative standard error approaches are conducted, including heteroskedasticity-robust and Driscoll-Kraay standard errors, which account for both serial correlation within countries and spatial cross-sectional dependence between them (Table 7) (Driscoll and Kraay, 1998).

3.6.2 Lagged Specification

As the static Two-Way Fixed Effects estimates an average treatment effect pooled across all post-reform years, H2 cannot be tested, which expects structural investment effects to materialise in the medium-term. To address this, the treatment effect is decomposed by years since reform adoption using a distributed lag specification, estimating dynamic effects from $t + 1$ to $t + 5$:

$$Y_{it} = \alpha + \sum_{k=1}^5 \beta_k D_{it}(k) + \gamma' X_{it} + \mu_i + \lambda_t + \varepsilon_{it}$$

where:

$D_{it}(k)$ is an indicator equal to 1, if year t is exactly k years after country i 's reform;
 β^k are the coefficients tracing the treatment effect at each post-reform horizon.

This specification enables a test of whether effects accumulate monotonically over time, as the theorised signalling framework expects (Table 3). According to Jensen (2003) and Zheng (2011), this approach is recommended to capture the delayed response of foreign direct investment allocation in cross-country studies, which is consistent with the theoretical distinction between Link 1 (observing from $t = 0$ to $t + 1$) and Link 2 (observing from $t + 2$ to $t + 5$).

3.6.3 Event-Study Specification

The event-study specification is the central test of the parallel trends assumption pre-reform, which has the following estimating equation:

$$Y_{it} = \alpha + \sum_{k=-2}^{10} \delta_k \cdot 1[t - E_i = k] + \gamma' X_{it} + \mu_i + \lambda_t + \varepsilon_{it}$$

where:

E_i is the reform year for country i ;

k is the event time, indexing years relative to the reform;

$k = -1$ is the omitted reference period (year immediately before reform);

δ^k are the event-time coefficients, estimated relative to $k = -1$.

Whether treated and control countries had parallel trends before the reform is shown by the pre-reform coefficients ($k < 0$), while the dynamic trajectory of any treatment effect is shown by post-reform coefficients ($k \geq 0$). For the theorised signalling framework, two conditions should be met: pre-reform coefficients should be statistically indistinguishable from zero, and post-reform coefficients should exhibit positive shifts beginning at or very shortly after $k = 0$. As a counterpart to the event-study specifications (Figure 1; Figure 2), the joint F-test on pre-treatment coefficients tests the null finding that all δ^k for $k < 0$ are jointly equal to zero (Table 4), which in fact rejects the null. This divergence in pre-reform BCI trends establishes an identification concern that is addressed in the Rambachan-Roth sensitivity analysis (Figure C1.1; Table C1.1).

3.7 Identification, Validity, and Robustness Diagnostics

For the causal estimates to be valid, the parallel trends assumption requires that, in the absence of treatment, both treated and control countries would have followed parallel trajectories throughout

the observation window. Due to the fact that, by definition, this counterfactual is unobservable in the post-treatment period, a number of complementary diagnostic tests are made to assess the identification, each targeting a distinct risk to causal inference.

3.7.1 Parallel Trends Assumption

The event-study plots for BCI and FDI as a share of GDP are the main tests of parallel trends, which show point estimates and 95 percent confidence intervals for the two years pre-reform, relative to the year immediately preceding the reform, $k = -1$ (Figure 1; Figure 2). In addition, a joint F-test on the pre-treatment event-study coefficients is conducted to see if the null hypothesis that pre-reforms are jointly equal. With a result of $F = 9.07$, the null hypothesis is rejected, proving these BCI trends between treated and control countries diverge pre-reform. This limitation is reported transparently in Section 3.9 and addressed further in the Conclusions as a real identification concern.

Considering this limitation, it is important to note that standard pre-trend tests have limitations themselves, specifically that they are sensitive to statistical power to the extent that a failure to reject may reflect insufficient precision, as opposed to the absence of a violation (Roth, 2022). In response to this, the analysis includes a sensitivity framework proposed by Rambachan and Roth, reframing the question from whether pre-trends test failed, to how large a violation of the parallel trends is required to overturn the main finding (2022). Seeing that the parallel trends assumption may not be statistically exact, the approach proposed by Rambachan and Roth restricts the degree to which post-treatment violations of the parallel trends can be distanced from differences in the pre-treatment (2022). This generates honest confidence sets that are valid under a range of plausible violations. In the sensitivity plots, it can be deduced that the null findings are robust even under moderate assumed violations of parallel trends (Figure C1.1). Critically, this indicates that no large hidden positive effect is hidden by the pre-trend divergences. As the direction of the pre-trend bias is upward (or that Reformers had structurally higher GDP growth and trade openness pre-treatment), any bias would be conceived to drive the estimated treatment effects in a positive direction (or that lobbying transparency reforms increase business confidence), making the null finding and weakly adverse results all the more striking.

3.7.2 Placebo Tests

Placebo tests are conducted, which replace the true reform indicator with a randomly assigned pseudo-treatment that maintains the sample structure but assigns treatment dates at random, with the objective of ensuring that the null results are not mechanically driven by the research design. By re-estimating the Two-Way Fixed Effects model under this placebo treatment, it is shown that coefficients for both BCI and FDI as a share of GDP are statistically indistinguishable from zero (Figure C2.1; Table 5). The estimated coefficients are clustered around zero for all randomisation draws, which confirms that the estimation framework does not mechanically generate spurious results, and that the null results are a genuine empirical finding.

3.7.3 Alternative Control Groups

Three alternative configurations of the baseline specification are estimated to ensure the compositional choice of treated and control groups does not drive the results by fault of their design. To be specific, the alternative configurations are Reformers against Non-reformers only, Reformers against Established countries only, and then Reformers against Non-reformers and Established countries. Across all the configurations, the treatment coefficients for BCI are negative, comparable in magnitude, and statistically consistent, which demonstrates that the null finding is not a result of which countries constitute the counterfactual, and is a robust finding (Table 6; Table C3.1).

3.7.4 Leave-One-Out Influence Analysis

To assess whether any single country has a disproportional leverage on the result, each of the 24 countries are removed from the sample in turn and the Two-Way Fixed Effects model is re-estimated on the remaining 23 observations. Particular attention is paid to Ireland and Luxembourg, whose FDI as a share of GDP are structurally inflated by the activities of their special purpose entities and the tax optimisation made by multinational corporations (BIS, 2024; IMF, 2020); also noting Australia, with the longest post-treatment observation window. The exclusion of any particular country from the analysis, as shown in the leave-one-out distributions

for BCI and FDI as a share of GDP, does not alter the magnitude or significance of the treatment coefficient. This means the null finding is distributed across the full cross-section, and is not anchored to any observation more than another.

3.7.5 Heterogeneity-Robust Estimators

The Goodman-Bacon decomposition is implemented to evaluate the degree to which the Two-Way Fixed Effects estimate is corrupted by forbidden comparisons; that is, comparisons in which countries that have already been treated serve as controls for countries that have not yet been treated, which may allot negative implicit weights to certain sub-group comparisons (Goodman-Bacon, 2021). To address this, the decomposition plots identify the relative contribution of clean versus corrupted comparisons to the overall estimates (Figure B2.1; Figure B2.2). In addition, the Callaway and Sant'Anna group-time average treatment effect (ATT) estimator, as a robustness check that avoids forbidden comparisons by construction, restricts the comparison group to units that have not yet been treated or never will be for each event time, and confirms the null findings across cohorts (Figure F2.1; F2.2).

3.8 Mechanism Decomposition: The Two-Link Architecture

The null average treatment effects resulting from both primary outcomes, while substantively significant in themselves, do not by default reveal where the failure lies in the theoretical causal chain. When tested separately, the two-link framework from Link 1 (reforms to business confidence) to Link 2 (business confidence to investment outcomes) can generate diagnostically informative results. If both links fail, a global null is produced; if Link 1 fails in the absence of Link 2 failing, the perceptions channel functions but cannot be activated by the reform signaling as designed.

3.8.1 Link 1: Reform and Perceptions

The first decomposition analysis tests Link 1 through a re-estimation of the Two-Way Fixed Effects model that uses four perception and risk indicators as dependent variables, including

BCI, ICRG Political Risk Index, sovereign yield spread, and WGI Rule of Law; where the reform dummy is the treatment variable and the same control set (Table 8). Should any of these indicators exhibit a statistically significant and correctly-signed coefficient, Link 1 is confirmed. Although, a null result across all four indicators would indicate that the mechanism fails at Link 1, before the perception stage.

3.8.2 Link 2: Perceptions and Capital Allocation (IV Strategy)

The second decomposition analysis tests Link 2 through an instrumental variables strategy. It uses the reform dummy D_{it} in a regression of Greenfield FDI that is instrumented on BCI. This means the reform dummy only affects Greenfield FDI through its effect on BCI and not through any direct channel, which is a restriction necessitated by the two-link mechanism. The strength of the instrument is tested by the first-stage F-statistic, where a value below the conventional threshold of 10 would signify that the reform dummy is a weak instrument for BCI. This test is substantively informative because it may confirm that reforms fail to move the perception variable that is supposed to transmit their effect onto investment outcomes (Table 9).

3.9 Data and Measurement Limitations

All empirical research designs have limitations, and addressing them transparently is a prerequisite for credible causal inference. This study is subject to a number of data and measurement limitations that bear on the interpretation of the results. These concern the binary coding of the treatment variable, the temporal aggregation of annual outcome data, structural outliers in FDI measurement, and the violation of the parallel trends assumption. As each limitation is best understood in relation to the specific estimates they qualify, a full discussion is deferred to Section 6.3, following the presentation of results.

4. ANALYSIS

The staggered difference-in-differences panel analysis presented in this section evaluates whether lobbying transparency reforms affect BCI and FDI as a share of GDP across 24 countries between 2000 and 2024. This analysis proceeds in four subsections. Before any causal estimations, Section 4.1 first establishes the comparability of the pre-treatment group and the structural features of the sample. Section 4.2 presents the main causal estimates, including the static Two-Way Fixed Effects model, lagged specification, and the event-study, to test H1 and H2. Then, Section 4.3 evaluates the identification validity and robustness of the analysis with parallel assumption tests. Finally, Section 4.4 decomposes the causal mechanism, testing at which point the theorised signalling mechanism fails. In Section 5, the results will be interpreted in view of the theoretical signalling framework and the research questions.

All supplementary analyses are provided Appendices A to F, including summary statistics, treatment timing details, further robustness checks, extended estimates, and heterogeneity analyses. All tables report standard errors in parentheses and the statistical significance of figures use the following notation: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

4.1 Pre-Treatment Structure

Table 1. Balance table, pre-treatment

Variable	Non-Reformers	Reformers	Difference	Obs
BCI	10011.77 (132.15)	9294.05 (2466.55)	717.73***	300
FDI (% GDP)	233.59 (526.86)	867.87 (1243.93)	-634.29***	303
GDP Growth	169.04 (234.93)	254.62 (275.08)	-85.58***	303
Trade Openness	7544.47 (2677.89)	9236.39 (4310.82)	-1691.92***	303
Control of Corruption	7,041,717 (3,674,849)	5,023,865 (3,512,787)	2,017,852***	287
Rule of Law	8,667,844 (568,478)	7,264,307 (2,640,242)	1,403,537***	287

Table 1 demonstrates that the eight Reformer and sixteen control countries were not structurally comparable prior to the adoption of reforms. Differences exist across all variables included in the analysis. Reformers had lower BCI, Control of Corruption, and Rule of Law scores than Non-reformers, while Reformers had significantly higher FDI inflows as a share of GDP, GDP growth, and trade openness than Non-reformers. These results imply that Reformers have greater economic dynamism but weaker institutions than Non-reformers. A further implication may be that the adoption of reforms were not arbitrary, as countries may have done so in response to this noticeable deficit in the credibility of their governance. Crucially, these pre-existing differences are controlled for in the later analysis with country fixed effects and time-varying covariates.

4.2 Main Estimates

Table 2. Static Two-Way Fixed Effects regression results

Dependent Variable	Treated	GDP Growth	Trade Openness	Control of Corruption	Rule of Law	Obs.	R² (Within)
BCI	-1063.87*	0.79***	0.16	0.0007	0.0005	574	0.106
FDI/GDP	-313.84	0.71	-0.04	0.00009	-0.0002	576	0.115

Table 2 shows that the treatment coefficient for BCI is negative and marginally significant, and that the treatment coefficient for FDI as a share of GDP is both negative indistinguishable from zero. These results suggest that neither H1 nor H2 are supported. Looking at the controls, Control of Corruption and Rule of Law are near zero and insignificant for both outcome specifications, while GDP growth maintains a positive association with BCI. Already unfavourable for the hypotheses, the within-group R² results show great volatility within countries for both outcomes, which suggests that lobbying registers, even if properly implemented, only represent one of many interventions that influence business confidence and investment outcomes.

Table 3. Lagged effects of reform

Dependent Variable	L1	L2	L3	L4	L5	GDP Growth	Trade Openness	Control of Corruption	Rule of Law	Obs.	R ² (Within)
BCI	-711.27	-1098.28	1202.41	989.38	-1452.93**	0.75***	0.22	0.001	0.001	480	0.114
FDI/GDP	34.02	-466.27	323.88	-204.92	-690.89	0.79	-0.08	0	-0.001	480	0.155

Table 3 shows that the only coefficient with a statistically significant result, across ten combinations of lagged outcomes, is BCI at $t + 5$. However, even this estimate follows inconsistent results between $t + 1$ and $t + 4$ and is not preceded by a directional trend, not to mention the result is negative. Taken with optimism, this coefficient indicates that, only five years after reform adoption, the initial spike in business confidence dissipated, which runs contrary to H1. Therefore, this specification reveals the absence of a consistent and positive trend for either outcome for any time period, bar BCI at $t + 5$, confirming the signalling effects are neither real nor delayed.

Figure 1. Event-study plot, BCI

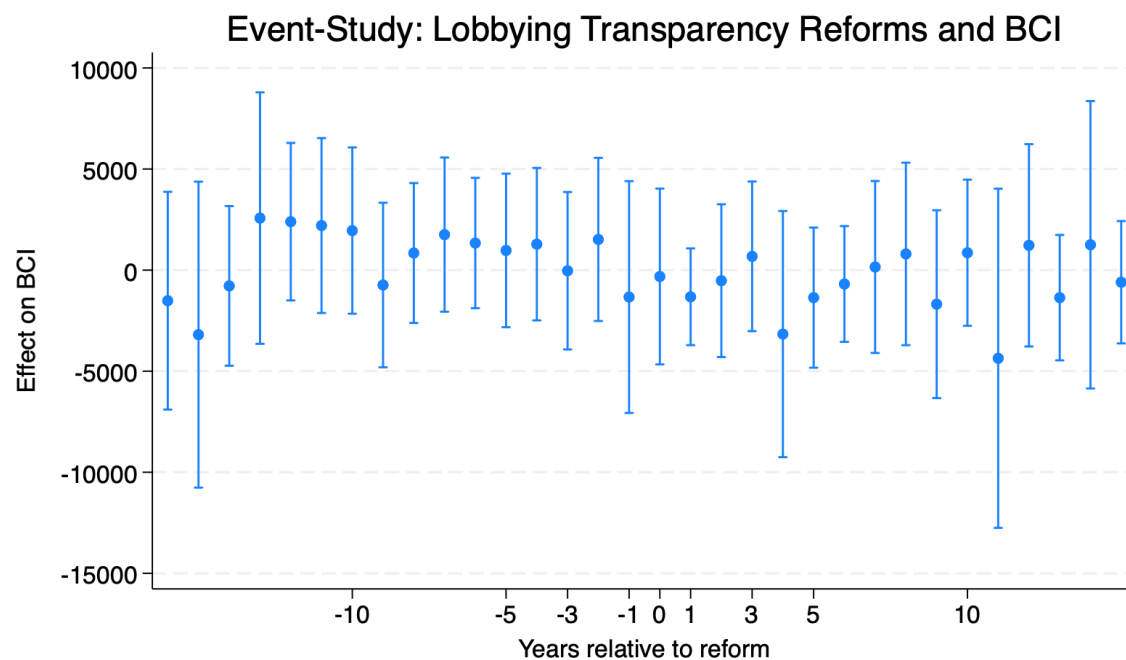
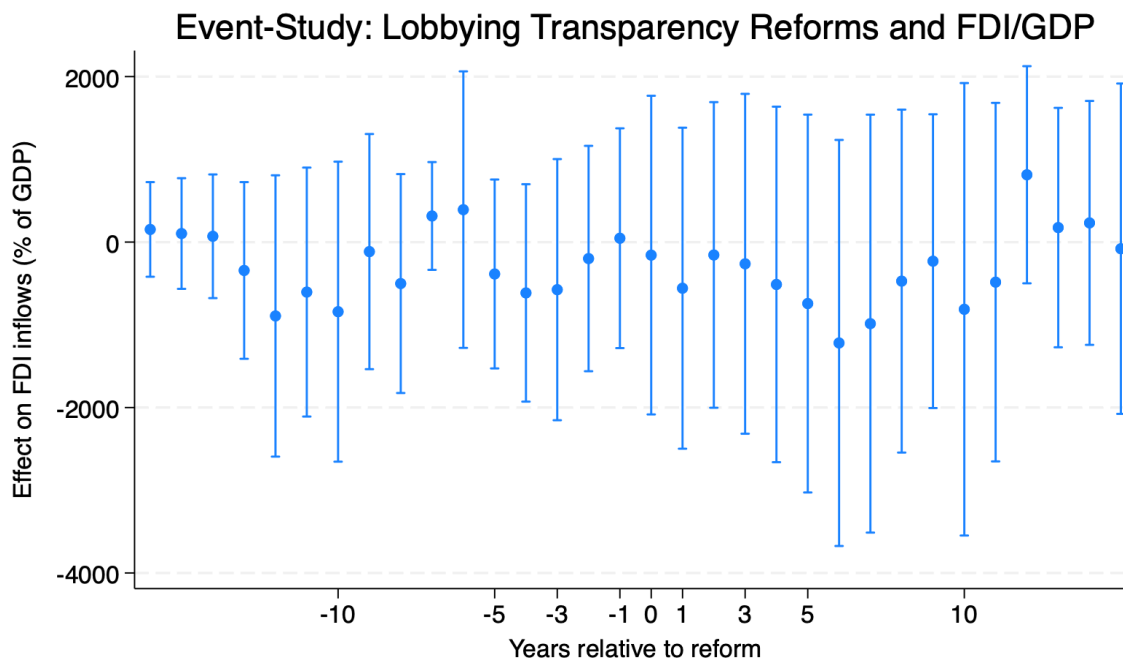


Figure 1 features two results that are significant to the analysis. First, the pre-reform coefficients at $t = -2$ and $t = -1$ are both large in absolute magnitude and negative direction, which reinforces the fact that Reformers had lower levels of BCI before reforms were adopted, in contrast to the control group. Although imprecisely estimated and not individually significant, the size and direction of these pre-reform coefficients suggest that the treated and control groups may not have been on parallel trajectories. Second, since the post-reform coefficients fluctuate around zero through $t + 4$, and the confidence intervals count zero across all years post-reform, Figure 1 indicates there is no sustained positive shift in BCI post-reform, which refutes H1.

Figure 2. Event-study plot, FDI as a share of GDP



While it reinforces the null results from Figure 1, Figure 2 suggests that there are no differential pre-trends in capital flows, with the pre-reform coefficients of FDI as a share of GDP at $t = -2$ and $t = -1$ sitting close to zero. Overall, the flatness of this event-study, centred near zero with wide symmetric uncertainty bounds, indicates that reforms are not associated with an increase in investment outcomes from the reform year through to a decade afterwards. Thus, Figures 1 and 2 suggest that neither the business confidence channel nor the capital allocation outcome support the predictions made by H1 and H2.

4.3 Validity and Robustness

Table 4. Joint F-test for pre-trends

Test	F-statistic	df	p-value
Pre-trends	9.07	(7, 7)	0.0047

Table 4 demonstrates that the pre-reform event-study coefficients are not jointly equal to zero, which denotes a violation of the parallel trends assumption. This indicates that there were divergences between treated and control countries before the reforms were adopted, implying some post-reform differences are attributable to a continuation of pre-reform differences, separate from the effects of reforms. To contextualise this finding, two observations are relevant. First, from the balance table (Table 1), it can be deduced that pre-trend bias is likely upwards because Reformers had higher GDP growth and trade openness before treatment, which strikingly, moved estimated treatment effects in a positive direction. Second, the Rambachan-Roth sensitivity analysis (Figure C1.1, Appendix C) shows that in spite of the parallel trends violation, the null findings survive, which implies that pre-treatment trend differences do not hide a meaningful positive effect on the outcome variables.

Table 5. Placebo test results

Outcome	Coefficient	Standard Error	Observations	Countries
BCI	144.749	294.523	574	24
FDI/GDP	30.105	155.377	576	24

Table 5 returns placebo coefficients that are both statistically insignificant and close to zero, which demonstrates that the negative and null findings exhibited in Tables 2 and 3 are not generated by the DiD design structure, the sample composition, or any mechanical feature of the estimation procedure. The consequence is that the null result is genuine. This is also confirmed by Figure C2.1 of Appendix C, which shows coefficients tightly clustered around zero across all randomisation draws.

Table 6. Alternative control group results

Outcome	Control Group Comparison	Treated Coefficient	Standard Error	t	P> t	95% CI Lower	95% CI Upper
BCI	Reformers vs Non-reformer	-1216.513	620.247	-1.96	0.069	-2538.538	105.511
BCI	Reformers vs Established	-1105.942	615	-1.8	0.092	-2416.784	204.9
BCI	Reformers vs All Controls	-1063.871	581.989	-1.83	0.081	-2267.806	140.064
FDI/GDP	Reformers vs Non-reformer	-190.321	220.419	-0.86	0.401	-660.132	279.49
FDI/GDP	Reformers vs Established	-328.198	210.804	-1.56	0.14	-777.514	121.117
FDI/GDP	Reformers vs All Controls	-313.842	221.381	-1.42	0.17	-771.803	144.119

Table 6 displays negative treatment coefficients for both BCI and FDI as a share of GDP across all three alternative control configurations, all of which have comparable magnitude and are below the 5 percent threshold. This directional consistency across all comparisons suggests that the null and weak adverse effects are not steered by the composition of the control group, and that the difference-in-difference research design does not manufacture the results.

Table 7. Alternative clustering results

Dependent Variable	Cluster / Robust SE	Treated	Standard Error	t	P> t	95% CI Lower	95% CI Upper	Obs.	Groups
BCI	Country cluster	-1063.871	581.9886	-1.83	0.081	-2267.806	140.0641	574	24
BCI	Robust	-1063.871	581.9886	-1.83	0.081	-2267.806	140.0641	574	24
BCI	Driscoll-K	-1063.871	390.0338	-2.73	0.012	-1870.717	-257.0245	574	24
FDI/GDP	Country cluster	-313.842	221.3806	-1.42	0.17	-771.8027	144.1188	576	24
FDI/GDP	Robust	-313.842	221.3806	-1.42	0.17	-771.8027	144.1188	576	24
FDI/GDP	Driscoll-K	-313.842	225.2837	-1.39	0.177	-779.8767	152.1928	576	24

Table 7 employs three standard error approaches to re-estimate the baseline specification, including country-level clustering (the baseline), heteroskedasticity-robust, and Driscoll-Kraay, which account for both serial correlation and spatial autocorrelation across countries. It produces identical results for BCI in the country-clustered and robust approaches, with a tightened standard error in the Driscoll-Kraay correction that makes the BCI statistically significant at the 5 percent level. For FDI as a share of GDP, all three approaches yield insignificant estimates with

stable point values and standard errors. Notably, the Driscoll-Kraay BCI result suggests that the negative relationship between the adoption of reforms and business confidence is estimated with greater precision when cross-country correlation is taken into account. Although this result does not imply a causal effect, it strengthens the finding that reforms are not associated with a positive shift in business confidence under all standard error assumptions.

Figure 3. Leave-one-out influence plot, BCI

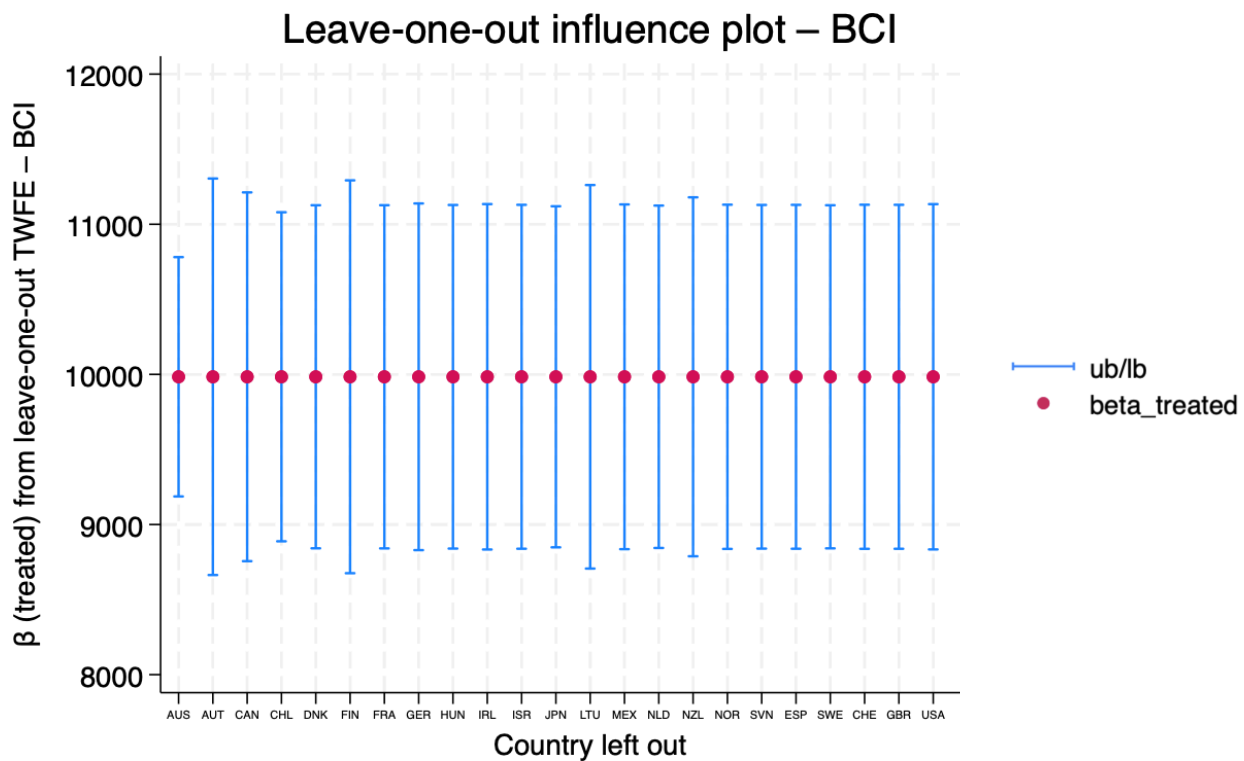
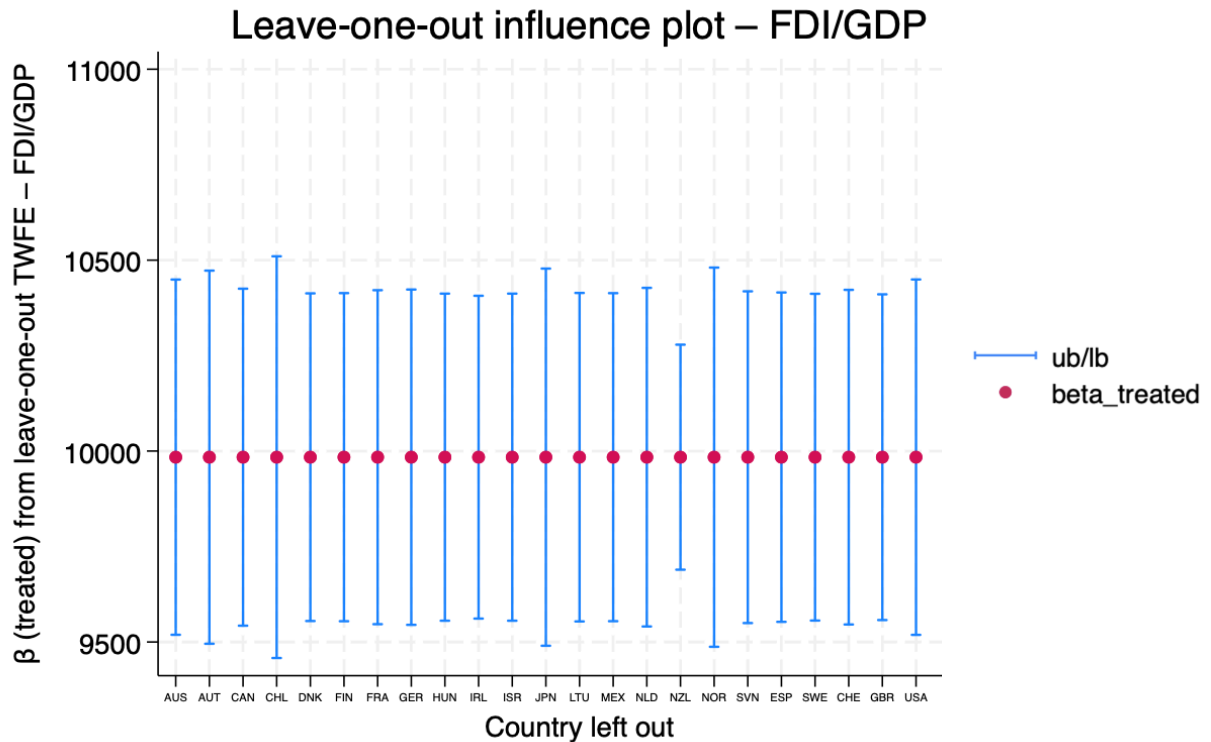


Figure 3 shows that the BCI treatment coefficient is very stable across all 24 iterations, and that the exclusion of any individual country from the treatment does not shift either the magnitude or the statistical significance of the estimate. Certain countries that may have had greater influence than others, such as Ireland with higher FDI as a share of GDP, or Australia with a longer post-treatment period, do not disproportionately affect the null finding.

Figure 4. Leave-one-out influence plot, FDI as a share of GDP



Similarly, Figure 4 reveals that none of the 24 country exclusion iterations produce estimates that are substantively or statistically different from the baseline result. This result is noteworthy because of the idiosyncratic composition of FDI as a share of GDP in certain countries, such as Ireland and Luxembourg, which have inflated FDI figures by virtue of their special purpose entity and tax optimisation activities that do not reflect their actual productive investment flows. As their exclusion does not change the baseline finding, Figures 3 and 4 reiterate that the null finding is a robust feature of the full panel.

4.4 Mechanism Test Results

This subsection intends to explain where and how the causal mechanism breaks down, given that lobbying transparency reforms have been shown to *not* causally affect BCI and FDI as a share of GDP. In the theorised signalling framework, Link 1 requires that reforms change the levels of business confidence in the quality of institutions, and Link 2 requires that these perceptual shifts translate into real investment outcomes.

Table 8. Link 1 test, reforms $\rightarrow$ confidence and risk indicators, $t = 0$ to $t + 3$

Variable	BCI	ICRG risk	Yield spread	WGI Rule of Law
L0	-0.451	0.22	0.134	-0.458
Standard error	(0.540)	(0.790)	(0.292)	(0.771)
L1	0.728	0.306	-0.277	-0.565
Standard error	(0.568)	(0.869)	(0.474)	(0.535)
L2	-0.883*	0.172	0.38	-0.355
Standard error	(0.439)	(0.508)	(0.224)	(0.35)
L3	0.412	-0.119	-0.425	-0.132
Standard error	(0.243)	(0.437)	(0.264)	(0.69)
Observations	528	480	526	528

Table 8 locates the failure of the causal mechanism at Link 1, in that lobbying transparency reforms do not generate a consistent, positive, and statistically robust shift in any of the four perception or risk indicators that the signalling mechanism requires. This holds true because the strong majority of coefficients, across all four indicators across and all four time horizons, are statistically insignificant and close to zero. The sole exception is BCI at $t + 2$, although this result is both negative and isolated to a single lag. On the whole, this Link 1 failure implies that the causal chain of Link 2 from reform to capital reallocation cannot operate.

Table 9. Link 2 independent variable regression, BCI/ICRG → Greenfield FDI, instrumented by reform dummy

Variable	Greenfield FDI – ICRG (2SLS FE)	Greenfield FDI – BCI (2SLS FE)
ICRG risk	-4603.2	–
	(6425.4)	–
BCI	–	27649.2
	–	(60647.6)
Controls		
GDP growth	Yes	Yes
Trade openness	Yes	Yes
WGI corruption	Yes	Yes
WGI rule of law	Yes	Yes
Year FE	Yes	Yes
Country FE	Yes	Yes
Observations	503	503
Countries	24	24
First-stage F (treated)	1.84	0.17
Kleibergen-Paap rk F	1.84	0.17
Stock-Yogo 10% critical	16.38	16.38

Table 9 uses a reform dummy as an instrumental variable for both BCI and the ICRG Political Risk index to estimate a two-stage least squares fixed effects regression, with Greenfield FDI as the outcome. It reveals that the second-stage coefficients for both Greenfield FDI and the ICRG Political Risk Index are statistically insignificant. Crucially, the first-stage diagnostic finds that the reform dummy is a weak instrument, suggesting that reforms do not change business confidence or political risk scores sufficiently enough to create the variation needed for the causal identification of Link 2. To substantiate these results, the first-stage F-statistics lie below the Stock-Yogo 10 percent critical value and the conventional minimum threshold, in addition to the Kleibergen-Paap rk F matching the values.

Figure 5. Callaway-Sant'Anna event study, BCI

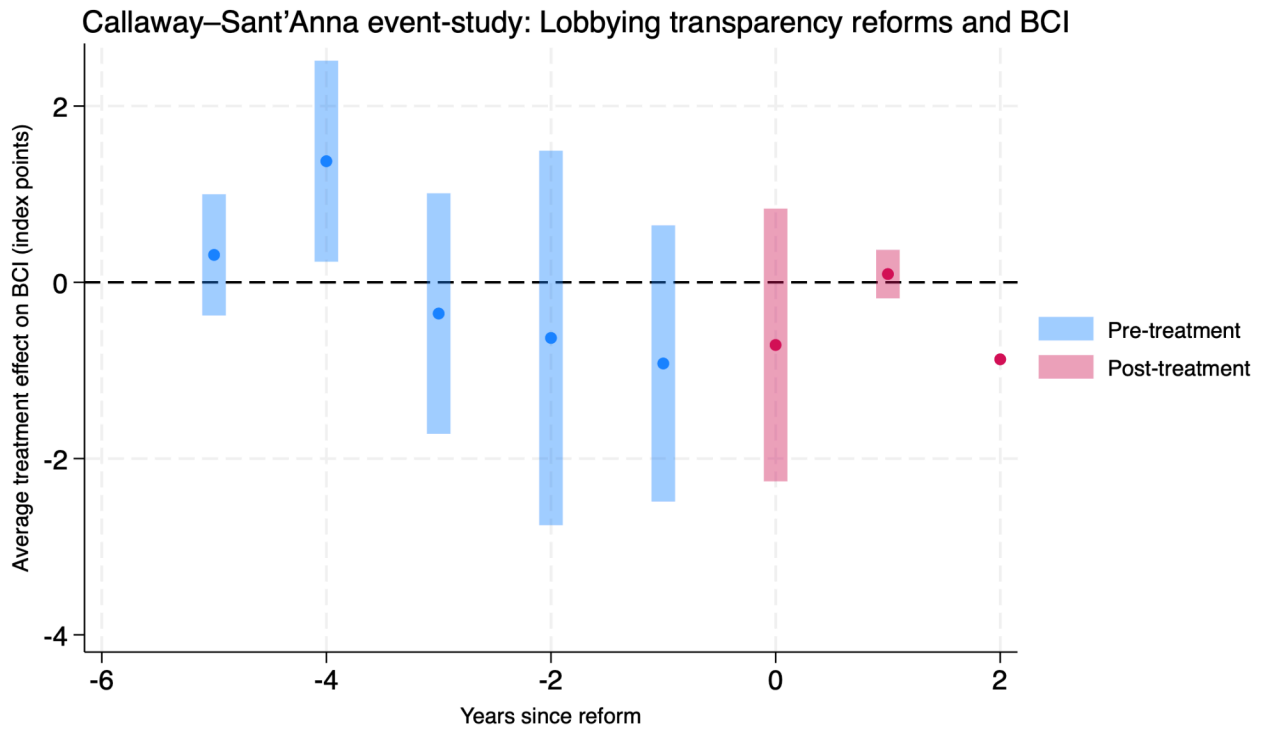


Figure 6. Callaway-Sant'Anna event study, Greenfield FDI



Figure 5 and 6 show that for both BCI and Greenfield FDI, the Two-Way Fixed Effects results were not biased by heterogeneous treatment effects across cohorts, in light of the estimator using already-treated units as implicit controls. To this end, no reform cohort sustains a positive post-reform shift, while the average treatment effect remains statistically indistinguishable from zero, fluctuating around zero with wide confidence intervals. For both BCI and Greenfield FDI, this confirms that the null results from the initial regressions with staggered treatment adoption are robust to treatment-timing heterogeneity.

5. FINDINGS

5.1 Evidentiary Standard

To ensure an objective and accurate interpretation of the empirical results, the evidentiary standard against which the two hypotheses are evaluated ought to be made explicit. Given null findings, the results are only meaningful insofar as the conditions under which they were produced could be capable of detecting the effects they failed to find.

For Hypothesis 1 to be confirmed, three conditions must be met. First, it requires a positive treatment coefficient on BCI that is statistically significant. Second, the pre-reform event-study coefficients would have to be indistinguishable from zero, establishing the comparable trajectories of both treated and control countries prior to reforms. Third, it requires a directionally consistent and positive dynamic pattern across the lag window from $t + 1$ to $t + 5$. For Hypothesis 2, confirmation requires a positive and statistically significant lag coefficient on FDI as a share of GDP across the medium-term window from $t + 2$ to $t + 5$.

None of these evidentiary standards are met by the results. This determination is robust across numerous estimators, alternative control group configurations, and varying approaches to standard error estimation; forming a pattern that is a substantive finding in its own right (Table 6 Table 7 Figure 3 Figure 4 Figure 5 Figure 6).

5.2 Hypothesis 1 on Business Confidence: Not Supported

5.2.1 The Static Estimate

The negative and marginally significant treatment coefficient from the Static Two-Way Fixed effects model is directionally adverse from Hypothesis 1, as opposed to merely indistinguishable from zero (Table 2). This directional character holds true across all three control group configurations, with negative treatment coefficients that are comparable in magnitude, and narrowly below the five percent threshold (Table 6). The fact that all control group compositions

converge demonstrates that the estimate's adverse direction is not a product of the research design choice, or who serves as the counterfactual for reforming countries, but is a substantive finding in itself.

The degree of robustness to this finding is shown when spatial dependence across countries is taken into account. For one, the BCI treatment coefficient achieves statistical significance at the five percent level in the Driscoll-Kraay standard errors (Table 7), which considers both serial correlation within countries and cross-sectional autocorrelation between them. Also, the negative relationship between the adoption of reforms and business confidence is estimated with greater precision than the baseline standard errors suggest, accounting for cross-country correlation, as the standard error tightens and the point estimate does not change. On the whole, none of the standard error assumptions, including country-clustered, heteroskedasticity-robust, or Driscoll-Kraay, generate positive estimates (Table 2; Table 7).

5.2.2 The Dynamic Estimates

The lagged specification enables the theorised signalling effect to materialise with a delay of up to five years following the adoption of reforms (Table 3; Table 5). For all combinations of lagged outcomes for BCI, the coefficients are insignificant up to $t + 4$. The only statistically significant lag estimate is at $t + 5$, which is both negative and not preceded by a consistent directional trend in earlier lags (Table 3). This excludes the potential for a delayed but genuine signalling effect to develop over the medium-term. More plausibly, this significant adverse finding emerging five years after reforms might be interpreted as a credibility decay, in which any optimism that remains from the initial adoption of the reforms fades once the constraints of implementation become visible to investors over time. Such an interpretation would also be consistent with the OECD's reporting of an implementation gap between the de jure adoption of lobbying registers and their de facto operational coverage (2026).

The event-study plot visualises the inconsistency of the post-reform coefficients from zero through $t + 4$ (Figure 1), with confidence intervals encompassing zero throughout the entire observation window. Noteworthy is the lack of discrete upward shift in BCI at or around the

reform adoption year, as is the lack of sustained departure from the pre-reform baseline at any subsequent period. To this end, Figure 1 confirms that the theorised signalling framework does not hold: the announcement and implementation of a lobbying register does not create a discrete, positive, and at least partially, sustained effect on business confidence.

5.2.3 The Leave-One-Out Test

The leave-one-out test addresses the potential concern that in a sample of 24 countries, a single observation may have had a disproportionate influence on the null finding (Figure 3). That is, a certain country with unusual structural characteristics may mask a genuine positive effect for the rest of the treated sample. To address this concern, the test systematically excludes one country at a time to estimate the static Two-Way Fixed Effects on the remaining 23 countries, which produced a strikingly stable distribution of coefficients across all the iterations (Figure 3; Figure 4). Countries with a plausibly disproportionate influence, such as Ireland with an inflated FDI as a share of GDP by virtue of their special purpose entity and tax optimisation activities, and Australia with the longest post-treatment observation window, do not affect the real distribution of the result.

5.3 Hypothesis 2 on Foreign Direct Investment: Not Supported

5.3.1 Static and Lagged Estimates

As shown in Table 6, the treatment coefficients for the static and lagged Two-Way Fixed Effects models for FDI as a share of GDP are consistent across all three alternative control group configurations as negative and statistically indistinguishable from zero. Unlike the BCI estimate, the standard error assumption for FDI as a share of GDP does not approach conventional significance thresholds, including the Driscoll-Kraay correction (Table 7). Similarly, the lagged specification reveals that across all five lag horizons, from $t + 1$ to $t + 5$, none of the FDI as a share of GDP coefficients reach statistical significance (Table 3). The result is unambiguous: the absence of any monotonically positive trend across the medium-term window rules out the possibility that a delayed reallocation is obscured by the bluntness of the static specification.

This finding is confirmed in the event-study plot for FDI as a share of GDP (Figure 2), which in contrast to the BCI event-study, displays pre-reform coefficients that sit near zero and removes any possibility of differential pre-trend in capital flows that might complicate post-reform causal inference. After reforms are adopted, coefficients are mostly negative through $t + 5$, but the confidence intervals encompass zero across more than a decade following reform adoption. The theorised signalling hypothesis is inconsistent with this flatness, as lobbying reforms should have produced a durable increase in investment outcomes for Reformers.

5.3.2 Greenfield FDI

The use of FDI as a share of GDP as an outcome measure assumes a recognised limitation: that countries with special purpose entity flows and tax optimisation strategies have FDI stocks that do not reflect actual productive investment flows. This holds true for countries such as Ireland and Luxembourg, characterised by their small and open economies. To avoid this distortion, Greenfield FDI, or investment in new physical productive capacity, represents a more theoretically appropriate test of whether lobbying transparency reforms attract real capital that enhances productivity.

Looking at the results for Greenfield FDI, the treatment coefficients for both the static Two-Way Fixed Effects model and the Callaway-Sant'Anna heterogeneity-robust estimator are statistically insignificant and negative in direction (Table F1.1; Figure 6). For Hypothesis 2, this means the null finding survives the removal of measurement noise that might have obscured a genuine productive investment effect, indicating it is not driven by data quality concerns of FDI as a share of GDP affecting the primary outcome specification.

5.3.3 The Expanded Outcome Set

In response to the null results, a further analysis was conducted, expanding the outcome set to six indicators, including BCI, FDI as a share of GDP, Gross Fixed Capital Formation as a share of GDP, sovereign yield spreads, the ICRG Political Risk Index, and Greenfield FDI. This enables

an assessment of whether the null results for the primary outcomes reflect an inaccuracy in that measure or a more general pattern.

Both the static Two-Way Fixed Effects model (Table F1.1) and the lagged specification (Table F1.2) show that the treatment coefficients of all six of the indicators across all horizons are both statistically insignificant and in the negative direction. The implication is that the null result is not outcome specific: the broadest plausible set of investment-relevant variables through which a signalling effect could be transmitted, from short-run investor perceptions to medium-term measures of real capital allocation, extends these findings uniformly (Table F1.1; Table F1.2; Figures F1.1–F1.4). This reinforces the finding that the absence of a detectable effect reflects a genuine property of the intervention, in contrast to a limitation of any single outcome or measurement.

5.4 Mechanism Diagnosis

5.4.1 The Diagnostic Logic

Having established that transparency reforms neither raise business confidence nor generate structural increases in FDI inflows across the panel, Hypothesis 1 and Hypothesis 2 are rejected. These null findings, in themselves, are a substantive empirical contribution. However, a null result at the level of the headline outcomes does not, by itself, identify why the expected effects failed to materialise.

From this standpoint, two separate theories might explain the same surface-level null result. First, firms and investors may perceive institutional quality differently in light of the reforms, but nonetheless choose not to reallocate capital. Second, firms and investors may not perceive institutional quality differently in light of the reforms, such that the causal mechanism is severed at its very first step. These two scenarios carry vastly different implications for theory, research, and policy designs, and by testing each link of the signalling chain independently, the causal mechanism can be decomposed to resolve the ambiguities that the headline outcome cannot clarify alone (Table 8; Table 9).

Crucial to this mechanism diagnosis is that the research design is sequential. First, Link 1 requires that the adoption of lobbying transparency reforms causes a positive and sustained change in the confidence in the institutional quality of the reforming government. This could be operationalised in a number of ways, including an improvement in business confidence, a reduction in political risk, a compression of sovereign yield spreads, or an improvement in perceptions relating to rule of law. Second, Link 2 requires that this change in perception translates into a reallocation of productive capital toward the Reformer.

The implication is that, if Link 1 holds but Link 2 does not, there would be an observable shift in confidence without the corresponding capital reallocation. Such a decoupling could theoretically be a result of portfolio inertia, other investment barriers, or measurement timing. However, if Link 1 itself fails, which has been shown to be the case, it logically follows that the capital reallocation will not take place, and the locus of policy intervention is the reform design instead of the investment environment more broadly.

5.4.2 Where Link 1 Fails

The Link 1 test (Table 8) isolates the effect of lobbying transparency reforms on four perception and risk indicators from the reform year to three years post-reform, including BCI, the ICRG Political Risk index, sovereign yield spreads, and the WGI Rule of Law. It provides an unambiguous result, which is that almost all coefficients across all four indicators and all four time horizons are statistically insignificant and close to zero. The sole exception is a decrease in BCI two years after the reform, which is isolated to a single lag and inconsistent with a genuine, if delayed, signalling response.

The pattern of WGI Rule of Law coefficients are consistently negative across all four lags. While individually insignificant, this directional pattern is at odds with the signalling hypothesis, and will be further unpacked in Section 5.7. On top of this, the WGI as intermediate outcome (Table E1.1) shows a treatment effect on WGI Rule of Law that is both statistically significant and negative at the five percent level, which across the entire empirical analysis, is the only

coefficient to achieve significance in this direction. Although the causal inference from this isolated result is limited, its adverse direction cannot be dismissed, which is developed in further detail in Section 6.

In short, the analysis provides the unambiguous result that the causal chain is severed at Link 1, meaning that lobbying transparency reforms in OECD countries do not produce a consistent, positive, and statistically robust change in any of the four perception or risk indicators that is required to confirm the signalling mechanism.

5.4.3 The Weak Instrument of Link 2

The Link 2 test (Table 9) uses the reform dummy as an instrumental variable for both BCI and the ICRG Political Risk Index, with Greenfield FDI as the outcome, in a two-stage least squares fixed-effects regression. It reveals weak instrument diagnoses across both specifications, with first-stage F-statistics that lie well below the Stock-Yogo ten percent critical value and the conventional minimum threshold of ten, confirmed by The Kleibergen-Paap rank F-statistics. Although these weak first-stage results cause the second-stage to be uninterpretable regardless of their magnitude or significance, it is worth noting the second-stage results are statistically insignificant for both outcome-instrument combinations.

However, this weak instrument result is substantively informative in its own right, as it confirms the failure of the Link 1 test (Table 8). The finding suggests the reform dummy does not alter business confidence or political risk with sufficient variation to generate to support causal identification, and that it cannot serve as a valid instrument. Otherwise put, the mechanism breaks down not because perceptions shift and fail to attract capital, but because perceptions do not measurably shift in the first place.

5.5 Explanations for Link 1 Failure

Since lobbying transparency reforms fail to generate detectable positive shifts in business confidence and investment outcomes, a second-order question arises: why do these reforms fail to shift business confidence? Three distinct theoretical explanations are examined, none of which are mutually exclusive.

5.5.1 Institutional Saturation

The first explanation holds that across OECD countries, the quality of institutional governance has surpassed the threshold of expectation for what sophisticated investors price into their risk assessments (Knack and Keefer, 1995; North, 1990). Embedded in robust institutional frameworks, these countries may already have established a credible baseline of rules-based governance, with WTO trade disciplines, OECD governance standards, EU regulatory directives, multilateral investment treaties, and national judicial systems with independent enforcement, and more. In this context, an additional transparency instrument such as a lobbying register, may be of marginal or negligible informational value to their decision-making, depicting a ceiling effect. It may also be difficult for firms and investors to meaningfully update assessments in response to incremental reforms, once their uncertainties about the institutional quality of a country have reached this ceiling.

The balance table (Table 1) provides support for this claim. Reformer countries had lower BCI and institutional quality indices than Non-reformers prior to the treatment, indicating structurally weaker governance indicators and less of a potential ceiling effect. Moreover, the entire sample, including weaker institutional performers, operate well above the global baseline at which signalling theory predicts maximum signal values (Table A1.1). Far from suggesting the panel has perfect governance frameworks, it characterises the OECD sample as sufficiently credible, such that the addition of a lobbying register does not provide a decision-relevant signal that would prompt an increase in confidence or revision of investments. This explanation is developed further in Section 5.7, which outlines the central boundary condition on signalling theory.

5.5.2 The Implementation Gap and Cheap Signalling

The second explanation holds that the quality of the actual reforms themselves are insufficient. In the costly signalling framework, a signal is credible only insofar as it is costly for a low-quality sender to imitate, which means it must impose real constraints on the behaviour of the signalling party (Spence, 1973). Therefore, a lobbying register that is limited to recording the identities of registered lobbyists, without mandating the disclosure of the targeted legislation, the financial resourcing, or the officials contacted, will not signal a meaningful constraint of the political elite whose behaviour it claims to regulate. Firms and investors that understand this distinction will rationally discount these reform efforts as cheap signals.

This explanation is supported by OECD analysis reporting a significant implementation gap of 19 percentage points between the de jure adoption of lobbying registers and their de facto operationalisation (2026). Across the OECD, almost all registers fail to meet minimum disclosure standards for financial transparency, legislative targeting, or beneficial ownership, which strongly favours the cheap signalling explanation. However, a limitation of this explanation is that the quality of reforms relies on OECD definitions and is not directly captured by the binary treatment variable used in the analysis, meaning that variations in adoption cannot be disentangled from variations in implementation. This limitation and potential research agenda is addressed further in Section 6.3.

5.5.3 Measurement Lag and Temporal Aggregation

The third explanation is methodological, indicating that the annual panel data is too temporally coarse to deduce a signalling effect that operates at sub-annual frequency. In the event that business confidence increases within weeks or months of the announcement of reforms, and subsequently reverts to baseline as the limitations of implementation become apparent, the potential positive effect will be averaged out at the annual observation and made undetectable in the analysis. With the available data, this explanation cannot be ruled out.

This explanation remains ambitious, however, particularly given the measurement lag. The Rambachan-Roth sensitivity analysis confirms that, across the full range of plausible trend violations, the null findings survive (Figure C1.1; Table C1.1). Also, the five-year lag specification captures the medium-term window most directly applicable to Hypothesis 2 (Table 3; Table F1.2). This implies that, should a sub-annual spike dissipate within a single year, the structural shift would not reasonably influence the investment climate, thereby leaving Hypothesis 2 unsupported regardless.

Considering all three explanations, institutional saturation and the implementation gap have greater theoretical coherence and empirical consistency. While the measurement lag forms a genuine technical limitation, it is improbable that, by itself, it will account for the null findings across the full range of outcomes, estimators, and time horizons included in the analysis.

5.6 Pre-Trends and Inference

Central to the validity of the analysis is the fact that the null hypothesis is rejected by the joint F-test of pre-reform trends (Table 4), as the coefficients are not jointly flat. This indicates that there were divergences between treated and control countries before the reforms were adopted, implying some post-reform differences are attributable to a continuation of pre-reform differences, separate from the effects of reforms. Crucially, this means the parallel trends assumption is violated, which is an identifying condition on which the causal interpretation of the difference-in-differences estimates rests. However, there are two observations that are analytically important to contextualise the violation of the parallel trends assumption.

First, the direction of the pre-reform trends is important for how the violation is interpreted. The covariates that are most positively associated with BCI in the primary specifications are GDP growth and trade openness, which, as shown in the balance table (Table 1), are also covariates that are structurally higher for Reformers prior to the treatment relative to the control groups. It follows that any upwards pre-trend bias caused by this structural divergence would drive the treatment effects in a positive direction, although this is not the case. With the treatment

coefficients for BCI being negative and null under these conditions, the adverse findings become all the more striking.

Second, the null findings are shown to be robust to a wide range of assumed magnitudes of trend violations in the Rambachan-Roth sensitivity analysis (Figure C1.1; Table C1.1). Specifically, it demonstrates that although there are moderate departures from parallel trends, there is no plausible correction that reverses the null finding. The identification robustness of this finding is further enhanced by the Callaway-Sant’Anna estimator (Figure 5), which overcomes the concern that the Two-Way Fixed Effects estimates weights the heterogeneity of treatment effects across staggered cohorts by using only units that have not yet been treated as controls. Instead, it shows that the null findings are consistent across all reform cohorts.

Therefore, the violation of the parallel trends assumption is a limitation to be acknowledged but also contextualised, in the sense that it does not diminish the direction of the core null finding, and improbably conceals substantive, meaningful positive effects.

5.7 Synthesis: Boundary of the Theoretical Framework

All the cumulative evidence enables a unified answer to the two hypotheses. For Hypothesis 1, the extent to which mandatory lobbying transparency reforms affect business confidence and FDI inflows in OECD democracies is to no positive or statistically detectable extent. All specifications, control group configurations, and both primary estimators generate treatment coefficients for BCI that are negative (Table 2; Table 3; Table 6; Table 7; Figure 1; Figure F2.1). Seeing that Hypothesis 1 is not supported, Hypothesis 2 is by extension also not supported, in that reforms do not generate any structural and medium-term improvements on investment climates. This holds true across all treatment coefficients for FDI as a share of GDP for lagged specifications, and all applied robustness checks (Table 2; Table 3; Table 6; Table 7; Figure 2; Figure 4).

The mechanism decomposition identifies Link 1 as the failure in the theorised signalling framework, suggesting reforms do not raise business confidence in institutional quality. This is

confirmed directly in the Link 1 test (Table 8) and quantitatively through the weak instrument diagnostic (Table 9). Since Link 1 fails, the absence of a capital reallocation follows as a logical sequence rather than an independent question.

Taken together, these empirical findings create a boundary condition on signalling theory as applied to institutional reform. The adoption of lobbying registers as a solution to an information gap between government and business actors requires a certain degree of institutional uncertainty for them to function as costly signals. Consolidated OECD democracies, embedded in dense national, supra-national, and multilateral frameworks, already operate near or at the ceiling of business confidence, leaving limited scope for lobbying transparency reforms to have an effect. This extends operative variables beyond the fact of reform, to include design quality, enforcement credibility, and institutional variance.

6. CONCLUSIONS AND LIMITATIONS

6.1 Revisiting the Research Questions

The research questions that this study set out to answer had not previously been subjected to cross-country causal scrutiny, in spite of their institutional urgency:

1. *To what extent do mandatory lobbying transparency reforms affect business confidence and foreign direct investment (FDI) inflows in OECD democracies?*
2. *Does the economic impact of these reforms persist over the medium-term, suggesting structural improvements in investment climates, or is it limited to short-term confidence shocks?*

The motivation of these questions are founded in the divergence between the institutional endorsement of lobbying registers as economic policy instruments and the absence of empirical evidence evidencing the claim, that is significant and growing. International organisations, including the OECD, World Bank, and European Union, consistently frame comprehensive integrity reforms as governance instruments that reduce public spending, stabilise markets, and attract investment (OECD, 2026; World Bank, 2024; Council of the European Union, 2026). Supported by the literature on signalling theory and credible commitment, these lobbying registers constrain elite discretion in a verifiable and legally binding manner, which function as costly signals that reduce information asymmetry between governments and firms and investors. It follows that an increase in the confidence of firms and investors should drive greater investment towards the reforming countries (Spence, 1973; North and Weingast, 1989). This study conducts the first cross-country causal test of those claims, using a staggered difference-in-differences design across 24 OECD countries between 2000 and 2024 to demonstrate that the evidence does not support those claims.

6.2 Main Conclusions

6.2.1 Empirical Conclusions

H1 and H2 are both not supported by the evidence, suggesting that the adoption of lobbying transparency reforms does not cause an immediate or lagged increase in either business confidence or FDI inflows. Across the full range of specifications for H1, treatment coefficients for BCI are negative and statistically indistinguishable from zero. This holds true for the static Two-Way Fixed Effects model, the lagged specification from $t + 1$ to $t + 5$, the event-studies, and the Callaway and Sant’Anna (2021) group-time average treatment effect estimator, and is consistent in direction across all control group configurations (Tables 2, 3, 6, 7; Figures 1, 5). By default of these results, H2 is also not supported. The treatment coefficients from the lagged and static specifications for FDI as a share of GDP are neither statistically significant nor correctly-signed across any post-reform horizon (Tables 2, 3, F1.1, F1.2; Figure F1.4). Further analysis shows this also holds for Greenfield FDI, which excludes financial pass-through flows.

The null result across both hypotheses is not outcome specific. Expanding the outcome set from BCI and FDI as a share of GDP, to include Greenfield FDI, gross fixed capital formation, sovereign yield spreads, and the ICRG Political Risk Index, produces treatment coefficients that are null and negative in direction across the board (Table F1.1). Crucially, these results do not constitute an absence of evidence; rather, they are the evidence of absence. This is substantiated by placebo permutation tests with estimated coefficients that cluster around zero (Table 5; Figure C2.1), leave-one-out sensitivity analysis that confirm no individual country drives the finding (Figures 3, 4), and the Rambachan-Roth sensitivity analysis that demonstrates no plausible correction to the parallel trends violation reverses the null result (Figure C1.1; Table C1.1).

The decomposition of the theoretical mechanism identifies Link 1 as the locus of failure. As the Link 1 test re-estimates the Two-Way Fixed Effects model with all the expanded outcome set, no statistically significant and correctly-signed reform effect is found at any point from $t + 0$ to $t + 3$ (Table 8) to suggest the passage from Link 1 to Link 2. While the WGI Rule of Law coefficient is statistically significant at the five percent level, it is directionally adverse to the theorised

signalling hypothesis. The localisation of this mechanism failure is corroborated by the Link 2 instrumental variables test (Table 9). Using a reform dummy that proves a weak instrument for BCI, with a first-stage F-statistic below the Stock-Yogo threshold and below the conventional minimum of 10, the lobbying transparency reforms are shown to not shift the perception variable with sufficient variation to transmit an effect onto investment outcomes. A Link 1 failure entails the absence of capital reallocation at Link 2, as a logical consequence rather than an independent empirical question.

6.2.2 Theoretical Conclusions

Consistent with the theoretical reservations anticipated in the literature review, the theoretical conclusions have a boundary condition imposed on them by the empirical results. Two complementary interpretations, which are not mutually exclusive, offer explanations for this pattern.

The first interpretation holds that institutional saturation places a ceiling on the credibility gains that any lobbying transparency reform can generate. Seeing that the signalling mechanism is most potent under conditions of institutional uncertainty, where the credible commitment of political elites is not already priced into investor expectations, OECD democracies already operate near the ceiling of institutional credibility, characterised by relatively high institutional quality. Embedded in dense national, supranational, and multilateral accountability frameworks, there is little room for any single-category transparency reform to shift investor expectations at the margin. In this context, lobbying registers may increase transparency, but not to the extent required to reduce the informational asymmetry that is the binding constraint on investment decisions.

Second, signal quality varies sufficiently across registers to undermine the credibility of the signal itself. For a signal to carry informational value, its cost must be negatively correlated with the quality being communicated, meaning it must be prohibitively costly for actors without genuine commitment to mimic. Across the OECD, most existing registers fall short of this threshold, as only 32 percent require disclosure of the specific legislation being targeted, and

only 18 percent require the disclosure of lobbying budgets. Due to the 19 percent implementation gap between de jure adoption and de facto enforcement, the binary treatment used in this study conflates cheap signals with costly ones for most reforming countries, and in response rational firms and investors have no basis for revising their institutional priors (OECD, 2026; Katsaitis, 2024).

The experimental evidence provided by Crepaz and Arikan (2024) supports these interpretations. Unlike political financing and asset declarations, the perceptions of the targeted audience are not shifted by the disclosure requirements of lobbying registers, denoting a null result attributable to the deeply embedded skepticism with which private sector actors assess the relationship between interest groups and governments. This macro-level null result of this study is consistent with the micro-level null result of Crepaz and Arikan (2024), which are also broadly consistent with Andrews' (2013) account of isomorphic mimicry, which holds that governance reforms are outwardly compliant with international standards, but do not alter underlying incentive structures domestically. This account aligns with Grimmelikhuijsen et al.'s (2013) argument that disclosure requirements may entrench, rather than dispel, prior evaluations of elite capture when audiences receive the information with deep-seated skepticism towards institutions. Considering this wealth of evidence, it stands to reason that the credibility of institutional signals count as much as their existence, as low-cost signals by actors without genuine commitment carries no informational value for rational actors, regardless of how widely it has been adopted.

6.3 Main Limitations

This section outlines four principal design and data limitations, and two scope and enforcement limitations, that may constrain the interpretation and generalisability of the findings.

6.3.1 Design and Data Limitations

The violation of the parallel trends assumption is the most consequential limitation on the causal interpretation of the primary estimates. In a staggered difference-in-differences panel design, the validity of the estimation rests on the identifying assumption that treated and control units should

follow parallel outcome trajectories in the absence of treatment. From the joint F-test of pre-reform BCI trends, the null of jointly flat coefficients is rejected, meaning that Reformers and Non-reformers structurally diverged prior to treatment adoption (Table 4), and the parallel trends assumption is violated.

However, two observations contextualise this limitation. First, the direction of the pre-treatment bias is significant, showing higher GDP growth and trade openness for Reformers, while both covariates are positively associated with BCI in the primary specifications (Table 1). Conceivably, the upward contamination attributable to this structural divergence would bias treatment effects positively, yet the estimated coefficients are null and negative throughout, which make the results all the more striking. Second, the Rambachan and Roth sensitivity analysis provides evidence across a wide range of plausible magnitudes of trend violation that the null finding is robust (Figure C1.1; Table C1.1). No defensible correction reverses the direction or statistical insignificance of the estimates. Therefore, acknowledging the parallel trends violation as a boundary condition on the strength of the causal claim is more accurate, than a total invalidation of the core finding.

Another design limitation is the size of the treatment group and the statistical power. Seeing that the identifying variation emerges from only eight Reformers, the precision of the estimates is limited, which raises the possibility that the study lacks the statistical power to detect treatment effects that are small but substantively significant. To be precise, the risk of a Type II error is raised, because with 24 country clusters, the finite-sample properties of cluster-robust standard errors may be imprecise (Cameron and Miller, 2015). To address this limitation, the Driscoll-Kraay standard error specification, which accounts for both serial correlation within countries and spatial cross-sectional dependence between them, shows that the BCI coefficient becomes more negative, and not less. This implies a lack of precision in the baseline standard errors that does not explain the null result on that outcome. Yet, the size of the treatment group precludes the detection of heterogeneous positive effects at country level, and the results should be interpreted accordingly.

The binary coding of the treatment variable is another limitation, assuming all reforms are homogeneous interventions. As the binary indicator is equal to one in all years from the reform year onwards, equal weight is assigned to lobbying registers across the sample, irrespective of their substantive quality, disclosure depth, or enforcement capacity. In short, a classical measurement error is introduced, which attenuates estimated effects towards zero (Hausman, 2001). Only reforms which impose genuinely high costs on the signalling actor, which cannot be mimicked by actors without genuine commitment to elite constraint, should generate perceptual and investment responses (Spence, 1973). When thin and unenforced registers are conflated with substantively robust ones, the binary specification may underestimate the effects that reform quality produces. To mitigate this concern, the cohort-level heterogeneity analysis tests whether effects are different between early, mid-, or late-adopting cohorts, and across countries stratified by baseline institutional quality (Table D1.1; Table D2.1; Figure D1.1; Figure D1.2; Figure D1.3), to find statistically significant positive effects do not emerge among any subgroups. This finding is supported across all cohorts by the Callaway and Sant’Anna (2021) estimator. That being said, an immediate priority for future research would be to develop a continuous and quality-weighted annual treatment index.

The final design limitation relates to the measurement of outcome variables: each of the primary outcomes have measurement constraints that may attenuate the detectability of any reform signal. The OECD’s standardised BCI aggregates manufacturing-sector survey responses, which are calibrated to track co-movements with the Composite Leading Indicator and de-trended GDP, which for the purposes of this study, means the indicator conflates cyclical macroeconomic conditions with institutional perceptions related to governance (OECD, 2021). Reform signals that shift investor perceptions of institutional quality are thus diluted within the broader cyclical variations factored into the BCI. Also raising a limitation, the outcome variable of FDI as a share of GDP has structural outliers, such as Ireland and Luxembourg, which record extreme values driven by the activities of special-purpose entities and multinational profit-shifting that are unrelated to governance improvements (UNCTAD, 2024). However, both of these limitations are addressed in the robustness analyses (Figures 3, 4; Table F1.2). Much the same for the primary FDI measure, null results are also produced for Greenfield FDI, while the leave-one-out analysis confirms that no individual country, including Ireland and Luxembourg, drive these results.

6.3.5 Scope and Enforcement Limitations

The restriction of the sample to 24 OECD member countries, while theoretically motivated, presents a limitation to the external validity of the findings. Populated by democracies that exceed a quality threshold of governance institutions, the sample is a deliberate theoretical boundary condition, rather than a data-driven constraint. Since the signalling mechanism requires that elite discretion is politically costly to reverse, and that accountability structures make these interventions credible to investors, the inclusion of non-democratic states in the sample would conflate the theorised mechanism with broader regime-type effects (North and Weingast, 1989). As such, endogeneity would be introduced that county fixed effects could not resolve. Broad generalisability of the study is the logical tradeoff, as middle-income or transitional democracies with lower institutional baselines and higher governance uncertainty, may see lobbying transparency reforms carry greater signalling weight. Where rule-of-law is limited and particularism is widespread, even a marginal improvement in a mandatory register may be more visible to investors, as it departs sharply from the prevailing norm (Mungiu-Pippidi, 2015).

Another limitation is the absence of consistent, cross-country data on de facto register implementation, enforcement activity, and disclosure quality across the observation window. The OECD's Public Integrity Indicators could not be incorporated into the primary estimation strategy, as the indicators are developed at survey intervals with significant gaps in earlier years. With a 19 percent difference between de jure adoption and de facto implementation of lobbying registers (OECD, 2026), the binary treatment may overstate the actual signal credibility in most Reformers. Without data evaluating the quality of registers, two theoretically distinct conclusions for the null result cannot be distinguished: whether lobbying transparency reforms produce no investment response even when properly implemented, or that the current registers do not constitute sufficiently credible signals in the first instance. From Katsaitis' (2024) comparative analysis, it appears most OECD registers fail to satisfy the costly-signal conditions of Spence (1973) because elite behavior is not meaningfully constrained. This implies lobbying registers have partial coverage, weak sanctions, and a substantial gap between the legal requirements and in practice realities of disclosure. To distinguish these interpretations empirically, a longitudinal, quality-weighted index of register implementation must be developed.

6.4 Future Research

In order from the most tractable to most methodologically ambitious, three priority research directions emerge from the findings and limitations of this study.

First, the development of an annual and country-level composite index of lobbying registers is the most immediate and critical extension of this study. Capturing disclosure depth, enforcement capacity, and implementation fidelity, such an index would enable the re-estimation of the primary specifications with a continuous treatment variable. This would clarify whether the signalling mechanism operates on the condition of reform quality, rather than the adoption of reforms alone. Assuming that only the registers that impose genuinely costly constraints on elite discretion produce perceptual and investment responses, the development of this index would ensure that the binary coding used in this study, which attenuate those effects towards zero, does not bias the results of future studies (Spence, 1973).

Among currently available sources, the OECD's Public Integrity Indicators provide the most plausible near-term foundation for such an index, with standardised and comparable measures of the implementation of anti-corruption and integrity policies across member and non-member countries (OECD, 2026). Complementary to these indicators is the *Anti-Corruption and Integrity Outlook* series, which catalogues the design, scope, and enforcement provisions of lobbying registers on a biennial basis (OECD, 2026). To extend their coverage to the earlier years of the observation window where data remains sparse, supplementary coding drawn from the national assessments and legislative archives of Transparency International could enable a more continuous and granular treatment variable to be constructed. Although, relying on expert-codes ordinal scoring rather than directly observed administrative data, as is the case for the OECD Public Integrity Indicators, may be subject to inter-coder reliability constraints. Looking further ahead, the Global Corruption Observatory combines micro-level datasets on government decision-making with computational methods to map the structural conditions under which capture and undue influence persists, representing a promising methodological complement insofar as it integrates administrative, legislative, and network data to yield enforcement-quality indicators that survey-based sources cannot supply at annual frequency.

Second, the use of sub-annual or high-frequency data on perception outcomes would overcome the limitation of temporal annual aggregation in this study, as short-term perceptual responses to reform announcements or implementation events may diminish within a single calendar year. To test whether the signalling mechanism operates at a finer temporal resolution than the annual panel, monthly or quarterly BCI data, or firm-level survey responses from the OECD Business Tendency Surveys could detect these perceptual responses. In addition, rather than centering the event-study design on the data of legislative effect, using reform announcement dates would address whether investor expectations adjust in anticipation of reform instead of upon enactment, as expected under the efficient markets hypothesis (Fama, 1970).

Third, extending this study to include heterogeneous institutional contexts and comparative designs would establish the generalisability of this study's findings across a broader range of governance environments. As raised in Section 6.2.2, the institutional saturation interpretation of the null results produces a testable prediction: that in contexts where the institutional baseline is lower and governance uncertainty is higher, lobbying reforms should produce larger and more detectable effects. An extension of this study to include middle-income countries in the sample, leveraging the World Bank's Business Ready Indicators and IMF governance data, would test this prediction directly. Case studies, such as synthetic control designs for individual high-quality reformers and qualitative process-tracing studies of investor decision-making in reforming countries, could provide the micro-level evidence on perception shifts that aggregate panel estimations cannot (Abadie, Diamond and Hainmueller, 2010).

6.5 Synthesis: Cheap Signals, Null Returns

This study represents the first cross-country empirical causal analysis to evaluate the relationship between lobbying transparency reforms and investment outcomes, and to find it empirically absent across the full range of outcome variables and estimation strategies employed. As adopted across OECD countries between 2000 and 2024, lobbying registers cause no statistically detectable increase in business confidence, no structural increase in FDI, and no measurable shift in expert-assessed political risk or sovereign borrowing costs. That said, the normative case for lobbying transparency is by no means diminished. Accountability, fairness, and the constraint of

undue influence over public decision-making are democratic imperatives that hold regardless of any economic advantages (OECD, 2024). What the evidence sheds light on is the empirical foundations for the overstatement made by international organisations that lobbying registers are governance investments that yield foreseeable economic returns (OECD, 2026; Council of the European Union, 2026). The costly signals that rational firms and investors require before revising their institutional priors is not met by lobbying registers that are thinly enforced, narrow in scope, and subject to a persistent implementation gap. If a signal can be produced at a low cost by actors without genuine commitment, it transmits no informational value for rational observers, regardless of how widely it is adopted (Spence, 1973).

7. POLICY RECOMMENDATIONS

Three recommendations follow from this study's empirical findings, addressed to international organisations and national governments, and operationalised through Chile, Sweden, and Canada as representatives of the Reformer, Non-reformer, and Established governance configurations.

Recommendation 1: Refrain from overstating economic and investment outcomes

When reform advocates overstate policy outcomes, two compounding risks are produced. First, a credibility risk is raised for international organisations. If promised economic and investment returns fail to materialise, the credibility of reform advocacy diminishes which may weaken future coalitions around governance reforms. This aligns with the transparency paradox documented by Grimmelikhuijsen et al. (2013; 2014), who suggest that over-promising reform outcomes can depress rather than increase institutional trust. Second, it incentivises the adoption of weak registers in the attempt to cheaply signal investment attractiveness, producing a material substitution risk for governments.

7.1.1 Recommendations for International Organisations

International organisations should, as an immediate priority, audit the evidence base on which their public integrity and lobbying transparency advocacy has been developed. The OECD Working Party on Public Integrity and Anti-Corruption (PIAC), in their forthcoming implementation guidance on the *Recommendation on Transparency and Integrity in Lobbying*, has the opportunity to distinguish democratically grounded effects of reforms that are evidentially supported, from the investment claims that are contingent on quality, context, and enforcement credibility. The World Bank's *Country Policy and Institutional Assessments* has the opportunity to request proven implementation quality as a precondition for credit awarded for lobbying transparency reforms, including registration coverage, verification mechanisms, and sanctions applied. The EU's Anti-Corruption Directive transposition monitoring frameworks should disentangle the integrity-economic rationale, which is already normatively supported under Article 10 TEU and the Charter of Fundamental Rights.

7.1.2 Recommendations for National Governments

The same course of action is recommended to national governments, where reform framing has direct consequences for their political durability. Under Chile's *Law 20.730*, implemented under the *National Public Integrity Strategy*, the Consejo para la Transparencia (Council for Transparency) should institutionalise evaluation through democratic-access indicators. These should capture disclosure volume, actor diversity, and the degree to which influence activities were previously opaque, rather than macroeconomic metrics. Turning to Sweden, their proposed new register, submitted to the Riksdag in April 2026, should be established in the *Principle of public access to official records* and GRECO Fifth Round recommendations, rather than economic appeals. For Canada, the Lobbying Commissioner has framed 21 recommendations on transparency, fairness, and the protection of decision-making independence to The Standing Committee on Access to Information, Privacy and Ethics, which is a rationale supported by evidence that should be reinforced throughout the statutory review.

Recommendation 2: Enhance the design and targeting of lobbying registers

This study's mechanism diagnosis identifies the lack of a causal relationship between register adoption and business confidence, which is consistent with OECD implementation data (2026). Only 38 percent of member countries satisfy the baseline criteria on lobbying implementation; only 32 percent of registers require the targeted legislation to be disclosed; and only 18 percent require disclosure of lobbying budgets. All the while, beneficial ownership transparency has declined from 42 to 17 percent since 2022, and 19 percentage point gap persists between de jure adoption and de facto implementation across OECD integrity frameworks (2026). Thus, the costly-signalling mechanism fails most OECD registers are low-cost, low-content, and incapable of producing the governance reassessment required for an investment response (Spence, 1973). The policy implication is that current registers lack the informational architecture necessary to function as credible signals. The threshold at which that architecture begins is a four-field minimum standard, including: 1. Mandatory disclosure of the targeted legislation; 2. The beneficial ownership of the lobbying entity; 3. The financial resources deployed; and 4. The public officials contacted (Chari, Hogan and Murphy, 2010).

7.2.1 Recommendations for International Organisations

The normative framework established in the 2024 revision of *Recommendation on Transparency and Integrity in Lobbying* is necessary but insufficient. This provides the Working Party on Public Integrity and Anti-Corruption with the chance to develop a tiered, quality-differentiated benchmarking standard as specified. Tier attainment rather than binary register presence should be added to the methodological approach of the Public Integrity Indicators. GRECO should incorporate such a minimum standard as the structured evaluation benchmark in future evaluations, enabling cross-country comparison of register quality rather than mere presence.

7.2.2 Recommendations for National Governments

To achieve these four standards, Chile should implement the three structural changes identified in an OECD 2024 Public Governance Review: introducing a unified definition of indirect influence actors that extends coverage to think tanks and third-party intermediaries; reinforcing the passive-subject Agenda Pública with a mandatory active-subject register linked to the legislative tracking system; and equipping the Council for Transparency with powers to conduct proactive compliance investigations, impose penalties, and receive anonymous complaints. Noting Sweden is legislating de novo, it should adopt a design to enter force on July 2027 that achieves: definitions of scope in primary legislation; enforcement assigned to the The Legal, Financial and Administrative Services Agency; lobbying actors auto-linked to the *Rixlex* via the diarium dossier system; and a quarterly disclosure cycle that matches Finland's register. Canada should replace the 'significant part of duties' threshold with a registration requirement subject for small organisations, require disclosure of grassroots lobbying activities and beneficial ownership of interest group entities, and introduce proportionate financial penalties; all measures already endorsed by the The Lobbyists Registrars and Commissioners Network in March 2026 as reflecting proven practice across Canadian jurisdictions.

7.2.3 Sequencing by Baseline Institutional Quality

The heterogeneity analysis (Table D2.1) demonstrates that treatment effects are most attenuated in higher-quality institutional settings, and the small positive effects detectable in lower-quality settings are consistent with the institutional saturation hypothesis. The implication of this finding for reform sequencing is that countries with lower institutional quality tend to be constrained by enforcement credibility above all else. Disclosure legislation cannot constitute a costly signal without being enforceable, because rational actors discount obligations that carry no sanction (Mungiu-Pippidi, 2015). For those with mid-level institutions, the priority should be to deepen disclosure requirements, including legislative targets, financial data, and beneficial ownership, as features most directly associated with credible signalling (Spence, 1973). For countries with high-level baselines, further depth improvements display diminishing returns, such that the most productive investment is interoperability with adjacent integrity systems

Recommendation 3: Extend lobbying transparency into governance frameworks

Consistent with the institutional saturation hypothesis, the heterogeneity analysis (Table D2.1) displays treatment effects that are most attenuated in institutional settings of high standards. This corroborates with evidence advanced by Laboutková, Šimral and Vymětal (2025), suggesting for transparency measures to be meaningful, FOI provisions, legislative footprint requirements, conflict-of-interest disclosures, and open government data standards must operate in mutually reinforcing relation. Isolated disclosure requirements are not sufficient to shift the perceptions of intended audiences. Even then, disclosing registered lobbyists in opaque institutional environments may confirm rather than dispel assessments of elite capture (Crepaz and Arikan, 2024; Grimmelikhuijsen and Meijer, 2014). The fact that lobbying transparency as a standalone instrument is insufficient to generate credibility, may be surpassed when it is intentionally and systematically embedded within a coherent public integrity architecture.

7.3.1 Recommendations for International Organisations

The OECD, in close coordination with GRECO and UNODC, is well-placed to develop a Public Integrity Design Architecture that cross-maps lobbying registers, using five instrument classes that are operationally linked, including: 1. Asset and interest declarations for public officials; 2. Post-public-service cooling-off rules; 3. Political finance oversight and campaign contribution disclosure; 4. FOI legislation with enforceable response timelines; and 5. Beneficial ownership transparency for lobbying entities. The forthcoming implementation guidance provided by the OECD PIAC on the 2024 *Recommendation of the Council on Transparency and Integrity in Lobbying and Influence* is uniquely positioned to operationalise the public decision-making footprint into a measurable interoperability standard. This compliance reporting should be formalised by GRECO, requiring countries to cross-reference lobbying registration data against interest declarations, and ensure post-public-service restrictions are monitored by the same body administering lobbying oversight. The EU's Directive transposition monitoring should determine whether Member Countries can sustain an operational link between prevention obligations and lobbying transparency provisions, and whether national coordination mechanisms can generate cross-instrument implementation data.

7.3.2 Recommendations for National Government

Although Chile's constituent instruments (Laws 20.730, 20.285, 20.880, and 20.900) provide the features of a leading integrity architecture, there is a design flaw of weak interoperability. Three linkages should be prioritised, including: automated InfoLobby-InfoProbidad (official lobbying register and asset and interest declarations platforms) cross-referencing using the RUT (unique tax number) as a common identifier; InfoLobby-SERVEL (national electoral authority) linkage to identify the overlap between political donations and lobbying activity; and InfoLobby-ChileCompra (public procurement platform) cross-referencing to detect correlations between influence activity and government contracts. The cross-system reporting mandate should be assigned to the Council for Transparency to review the coherence of these linkages. For Sweden, three courses of action are available that would improve interoperability. These include: legislating cooling-off periods for ministers and secretaries; linking legislative footprint

requirements to case numbers in the Riksdag registry; and appointing a central body to oversee integrity risks; a gap identified in Sweden's 2024 OECD Integrity Review (OECD, 2024). Canada should formally align the The House of Commons Standing Committee on Access to Information, Privacy and Ethics review of the *Lobbying Act* with the concurrent review of the *Conflict of Interest Act*; develop an integrated public integrity portal consolidating the lobbying registry, ethics commissioner disclosures, LEGISinfo bill data, and Elections Canada financial returns; and task The Lobbyists Registrars and Commissioners Network with developing pan-Canadian technical standards for interoperability of lobbying and integrity disclosure data.

7.3.3 Time Horizon and Expectation Management

The plausible credibility gains that an integrated integrity architecture can generate are structural and long-run, not immediate post-adoption effects. The staggered difference-in-differences design finds no positive trajectory emerging at any post-reform horizon in the aggregate OECD sample, which estimates treatment effects from $t + 0$ through $t + 10$ (Tables 2, 3; Figures 1, 2). The signalling mechanism through which lobbying transparency is theorised to affect investor perceptions operates by the gradual accumulation of credibility. Through consistent enforcement, sanctions, and stable regulation across electoral cycles, transparency reforms are progressively integrated into the broader institutional environment (North and Weingast, 1989; Spence, 1973). The expectations of domestic and international policymakers should consider an integrated integrity architecture as a long-term investment in institutional credibility whose returns, if they are to materialise, will do so over a reform horizon that exceeds the short- to medium-term window that any single evaluation study, including this one, is positioned to capture.

Synthesis: A Research and Evaluation Agenda

The three recommendations present the justificatory, design, and architectural features of a coherent and evidence-based reform programme. First, a recalibration of the integrity dividend narrative is a precondition for sustained national political support. By justifying reforms on democratic grounds, their normative potential can be realised without weakening institutional credibility by speaking to macroeconomic returns. Second, a redesign of lobbying registers is a precondition to meet the costly-signal threshold through which governance perceptions may be altered. Third, comprehensive public integrity architecture that embeds lobbying transparency is a structural condition for credibility gains to potentially be detected.

In addition to these recommendations, a shift in evaluation is required. As established in this study, if lobbying registers do not generate investment returns in the short- to medium-term, evaluation frameworks will systematically misdiagnose reform progress. Their methodologies should shift to measurable outcomes that are specific to integrity, including shifts in: identified conflicts of interest, instances of government contracts awarded to politically connected firms, and citizens' perceptions of undue influence as a trust sub-indicator (OECD, 2024). These metrics enable lobbying transparency measures to be assessed on the normative and procedural ground where its effects are most plausibly located.

Ultimately, the absence of a measurable economic return does not diminish the case for these reforms, rather it reframes it. To increase the prospect of these returns, the policy challenge for international organisations and national governments is thus not merely to make lobbying more transparent, but to ensure the governance of these mechanisms are more credible.

APPENDICES

Appendix A: Data Sample and Construction

A1: Descriptive statistics

Table A1.1. Summary statistics (all variables, 24 OECD countries, 2000 to 2024)

Variable	Obs	Mean	Standard Deviation	Min	Max
bci	597	96.85	17.07	9.68	105.42
fdi_gdp	600	4.31	10.61	-40.11	105.64
gdp_growth	600	2.26	3.03	-14.84	24.62
trade_open	600	86.54	43.03	19.56	258.78
wgi_corruption	576	51.63	38.82	2.86	96.37
wgi_ruleoflaw	576	64.74	33.95	3.71	93.26

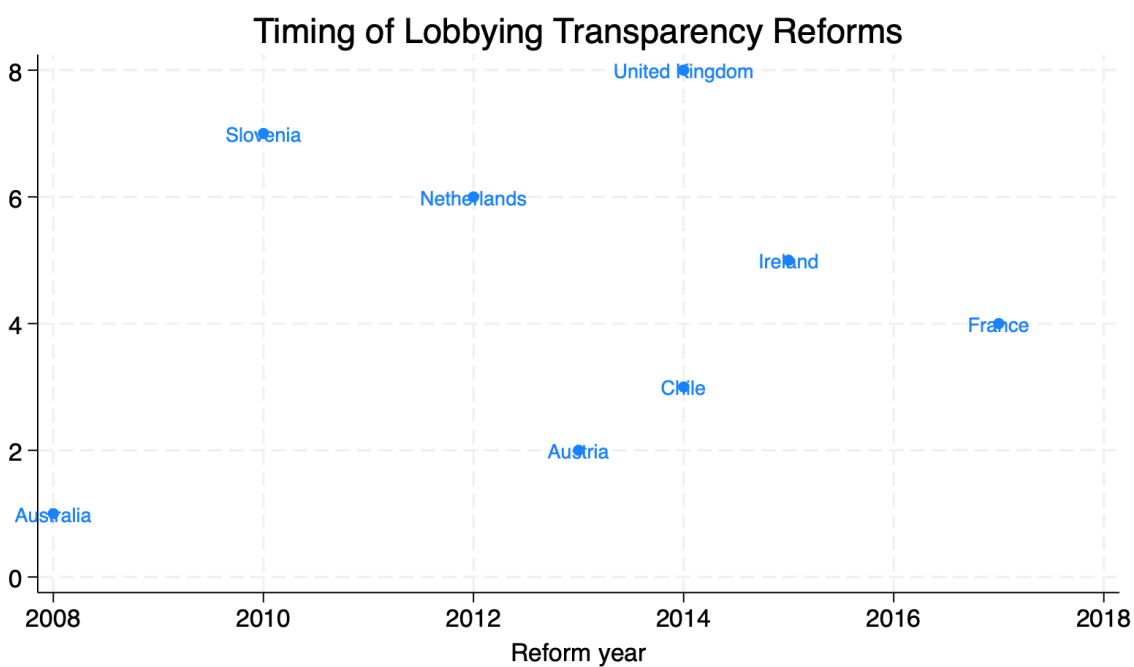
A2: Treatment Coding and Timing

Table A2.1. Treatment timing by country

Country	ISO3	Group	Reform Year	Sample Start	Sample End
Australia	AUS	Reformer	2008	2000	2024
Austria	AUT	Reformer	2013	2000	2024
Canada	CAN	Established	–	2000	2024
Chile	CHL	Reformer	2014	2000	2024
Denmark	DNK	Non-reformer	–	2000	2024
Finland	FIN	Non-reformer	–	2000	2024
France	FRA	Reformer	2017	2000	2024
Germany	GER	Established	–	2000	2024
Hungary	HUN	Established	–	2000	2024
Ireland	IRL	Reformer	2015	2000	2024
Israel	ISR	Established	–	2000	2024
Japan	JPN	Non-reformer	–	2000	2024
Lithuania	LTU	Established	–	2000	2024

Luxembourg	LUX	Established	–	2000	2024
Netherlands	NLD	Reformer	2012	2000	2024
New Zealand	NZL	Non-reformer	–	2000	2024
Norway	NOR	Non-reformer	–	2000	2024
Poland	POL	Established	–	2000	2024
Slovenia	SVN	Reformer	2010	2000	2024
Spain	ESP	Non-reformer	–	2000	2024
Sweden	SWE	Non-reformer	–	2000	2024
Switzerland	CHE	Non-reformer	–	2000	2024
United Kingdom	GBR	Reformer	2014	2000	2024
United States	USA	Established	–	2000	2024

Figure A2.1. Treatment timing timeline (reform years across eight reformer countries)



A3: Raw Outcome Trends

Figure A3.1. Raw BCI trends by group

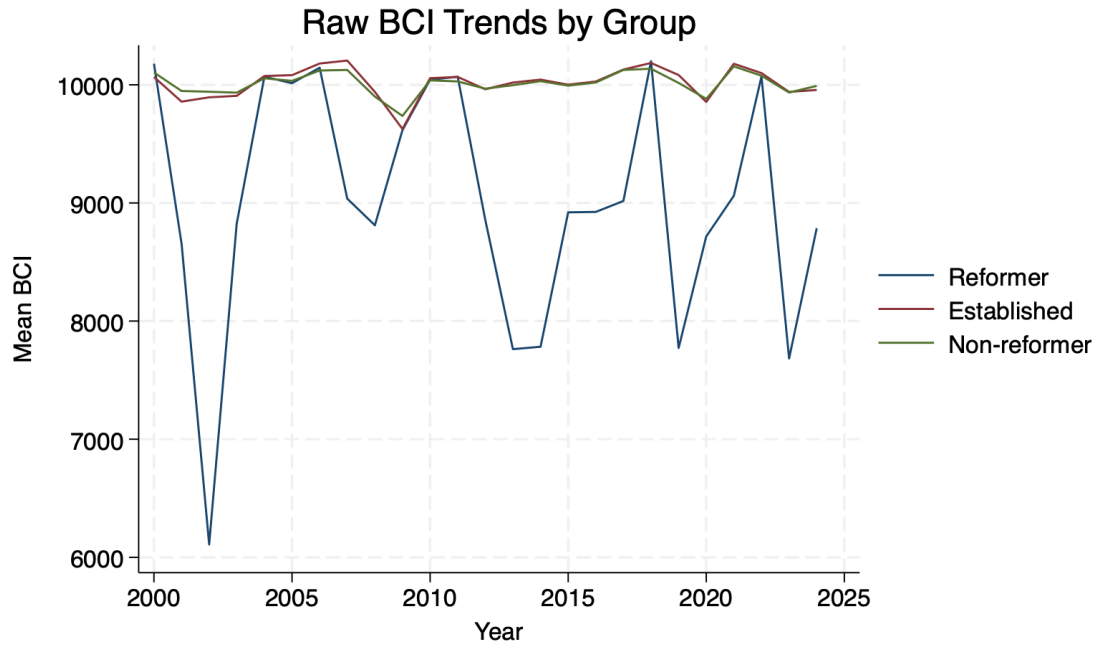
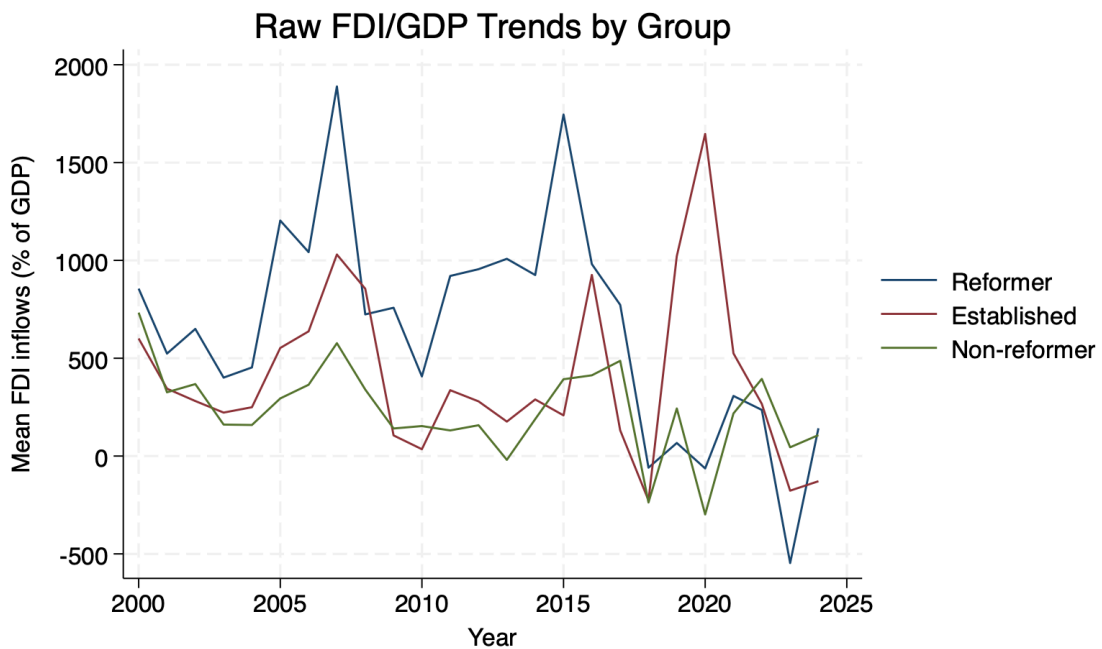


Figure A3.2. Raw FDI/GDP trends by group



Appendix B: Extended Primary Estimates

B1. Event-Study Coefficients

Table B1.1. Event-study coefficients of reform (BCI and FDI/GDP, $t = -2$ to $t = +10$)

Years Since Reform	BCI Coefficient (SE)	FDI/GDP Coefficient (SE)
-2	-1514.64 (2277.58)	152.93 (241.92)
-1	-3193.08 (3200.67)	103.11 (282.92)
0 (reform year)	-782.21 (1670.14)	69.95 (316.00)
1	2572.62 (2631.01)	-342.97 (451.65)
2	2396.79 (1647.63)	-893.06 (718.99)
3	2204.23 (1829.31)	-604.40 (636.37)
4	1953.94 (1739.60)	-841.33 (766.85)
5	-737.94 (1721.14)	-115.23 (601.07)
6	845.37 (1463.14)	-501.37 (559.55)
7	1755.50 (1613.12)	315.30 (275.48)
8	1338.57 (1363.28)	391.92 (706.75)
9	972.75 (1605.80)	-385.61 (482.84)
10	1282.74 (1595.25)	-614.75 (555.66)

B2. Advanced Difference-in-Difference Decomposition and Estimator Comparison

Figure B2.1. Goodman-Bacon decomposition plot, BCI

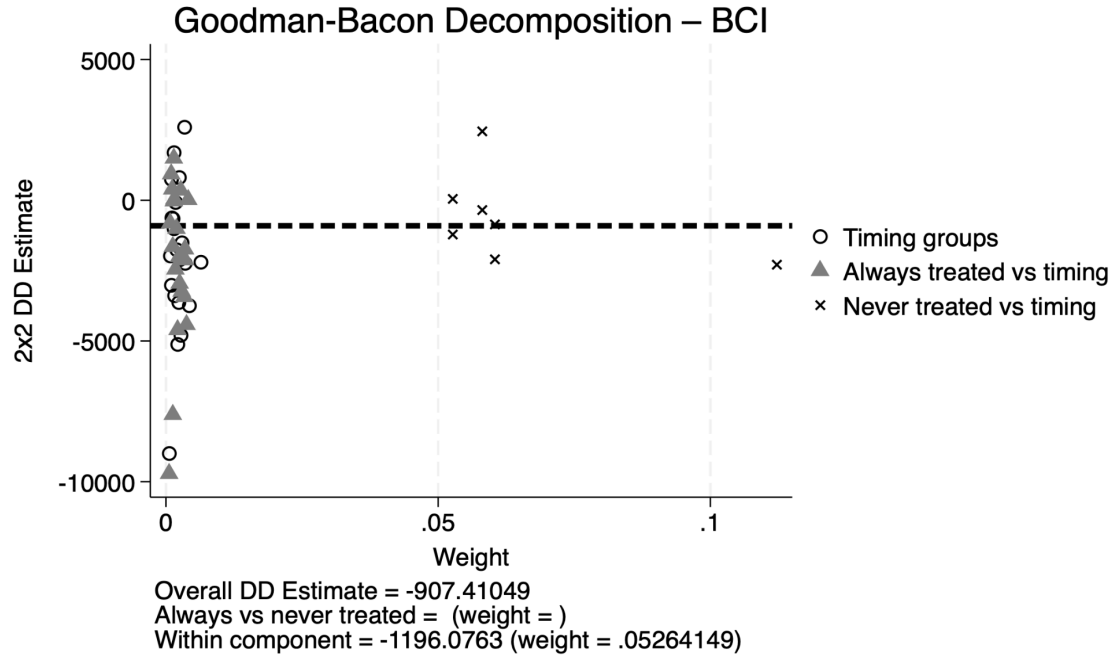
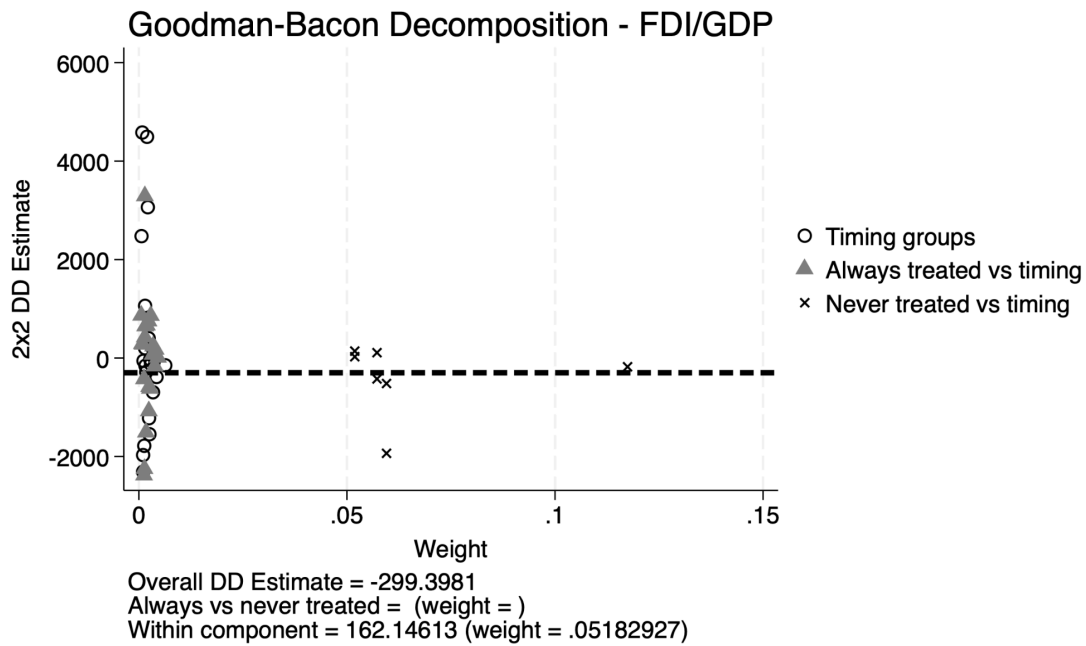


Figure B2.2. Goodman-Bacon decomposition plot, FDI/GDP



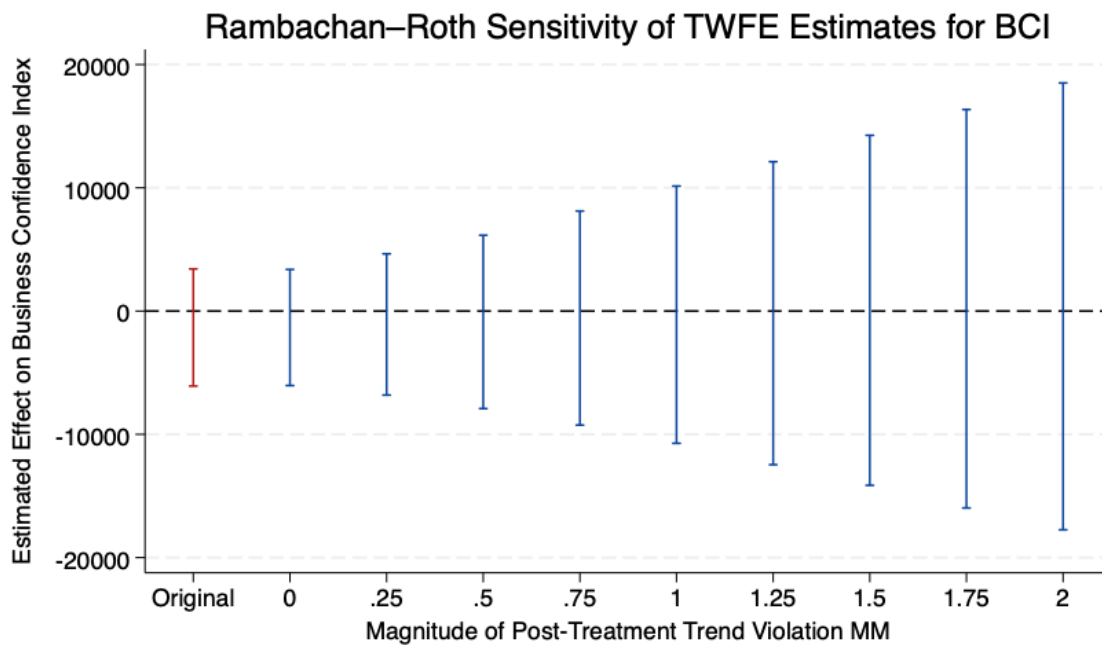
Appendix C: Identification and Robustness Diagnostics

C1. Sensitivity to Parallel Trends Violations

Table C1.1. Rambachan-Roth sensitivity analysis (BCI and FDI/GDP across event-time shifts)

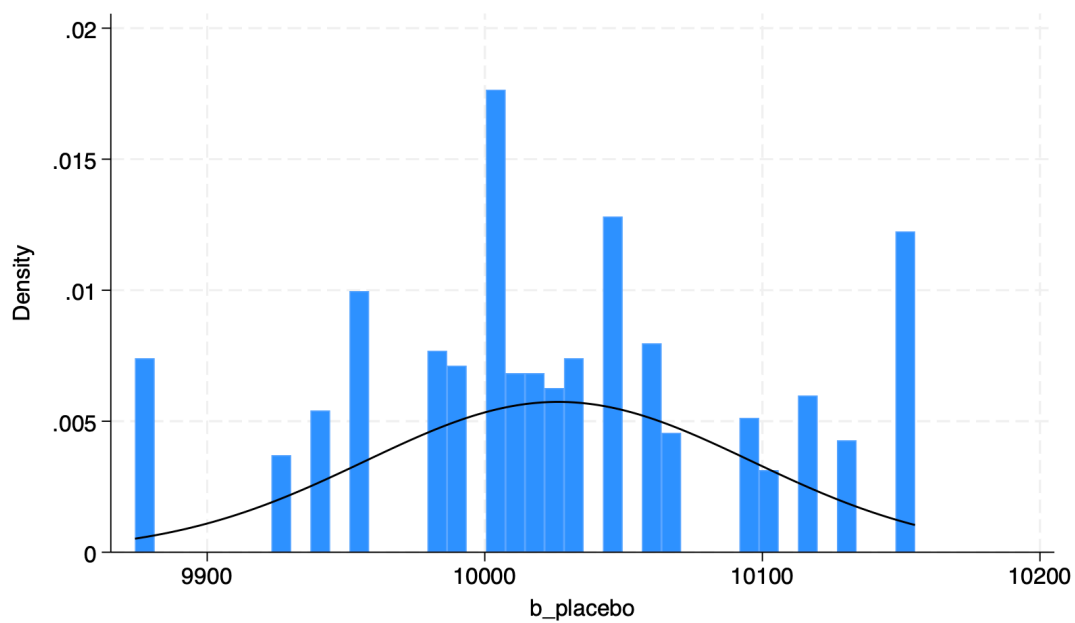
Event Time Shift	BCI Coefficient	Std. Error	FDI/GDP Coefficient	Standard Error
0	0	–	0	–
2	-1.515	2.278	1.529	2.419
3	-3.193	3.201	1.031	2.829
4	-0.782	1.67	0.7	3.16
5	2.573	2.631	-3.43	4.516
6	2.397	1.648	-8.931	7.19
7	2.204	1.829	-6.044	6.364
8	1.954	1.74	-8.413	7.669
9	-0.738	1.721	-1.152	6.011
10	0.845	1.463	-5.014	5.596
11	1.755	1.613	3.153	2.755
12	1.339	1.363	3.919	7.067
13	0.973	1.606	-3.856	4.828
14	1.283	1.595	-6.148	5.557
15	-0.032	1.647	-5.751	6.675
16	1.518	1.706	-1.991	5.76
17	-1.333	2.425	0.464	5.617
18	-0.316	1.838	-1.576	8.144
19	-1.32	1.013	-5.577	8.205
20	-0.523	1.598	-1.557	7.812
21	0.681	1.566	-2.625	8.687
22	-3.165	2.575	-5.122	9.086
23	-1.362	1.465	-7.429	9.657
24	-0.686	1.211	-12.196	10.378
25	0.153	1.799	-9.857	10.681
26	0.8	1.909	-4.72	8.765
27	-1.685	1.965	-2.31	7.508
28	0.86	1.529	-8.125	11.564
29	-4.362	3.548	-4.843	9.164
30	1.226	2.117	8.136	5.55
31	-1.363	1.312	1.751	6.119
32	1.252	3.006	2.317	6.233
33	-0.6	1.28	-0.803	8.444

Figure C1.1. Rambachan-Roth sensitivity plot for BCI



C2. Placebo and Randomisation Inference

Figure C2.1. Placebo permutation distribution (placebo treatment coefficients for BCI and FDI/GDP)



C.3 Alternative Control Groups and Clustering

Table C3.1. Alternative control group results (reformers vs non-reformers; vs established; vs all controls)

Outcome	Control Group Comparison	Treated Coefficient	Standard Error	t	P> t	95% CI Lower	95% CI Upper
BCI	Reformers vs Non-reformer	-1216.513	620.247	-1.96	0.069	-2538.538	105.511
BCI	Reformers vs Established	-1105.942	615	-1.8	0.092	-2416.784	204.9
BCI	Reformers vs All Controls	-1063.871	581.989	-1.83	0.081	-2267.806	140.064
FDI/GDP	Reformers vs Non-reformer	-190.321	220.419	-0.86	0.401	-660.132	279.49
FDI/GDP	Reformers vs Established	-328.198	210.804	-1.56	0.14	-777.514	121.117
FDI/GDP	Reformers vs All Controls	-313.842	221.381	-1.42	0.17	-771.803	144.119

Table C3.2. Alternative clustering results (country-clustered, robust, Driscoll-Kraay SEs for BCI and FDI/GDP)

Dependent Variable	Cluster / Robust SE	Treated	Standard Error	t	P> t	95% CI Lower	95% CI Upper	Obs.	Groups
BCI	Country cluster	-1063.871	581.9886	-1.83	0.081	-2267.806	140.0641	574	24
BCI	Robust	-1063.871	581.9886	-1.83	0.081	-2267.806	140.0641	574	24
BCI	Driscoll-K	-1063.871	390.0338	-2.73	0.012	-1870.717	-257.0245	574	24
FDI/GDP	Country cluster	-313.842	221.3806	-1.42	0.17	-771.8027	144.1188	576	24
FDI/GDP	Robust	-313.842	221.3806	-1.42	0.17	-771.8027	144.1188	576	24
FDI/GDP	Driscoll-K	-313.842	225.2837	-1.39	0.177	-779.8767	152.1928	576	24

Appendix D: Heterogeneity Analyses

D1. Heterogeneity by Reform Cohort Timing

Table D1.1. Heterogeneity by cohort timing (late vs early reformers, BCI and FDI/GDP; implied ATT)

Outcome	Cohort	Coefficient (treated)	Std. error	p-value	Implied ATT
BCI	Late reformers	-2,228.5	1,115.40	0.086	-2,228.5
BCI	Early reformers	273.1 (diff)	1,208.70	0.828	-1,955.4
FDI/GDP	Late reformers	338.5	793.8	0.683	338.5
FDI/GDP	Early reformers	-620.3 (diff)	931.5	0.527	-281.8

Figure D1.1. Heterogeneous dynamic effects – mid reform cohort (BCI event-study)

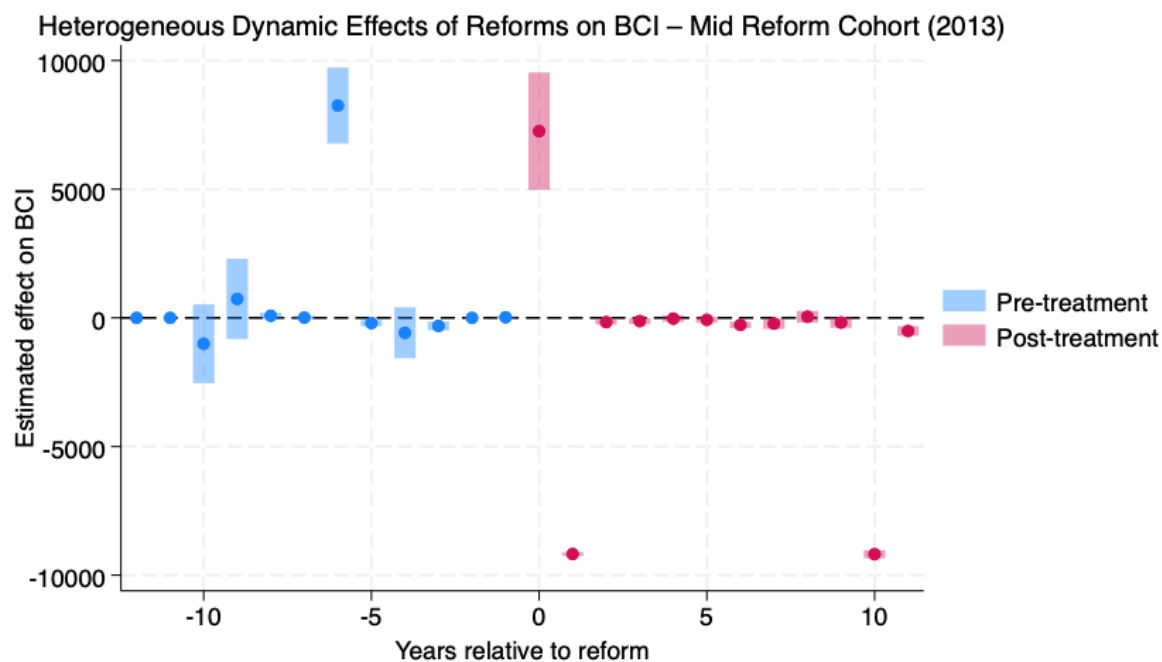


Figure D1.2. Heterogeneous dynamic effects – early reform cohort

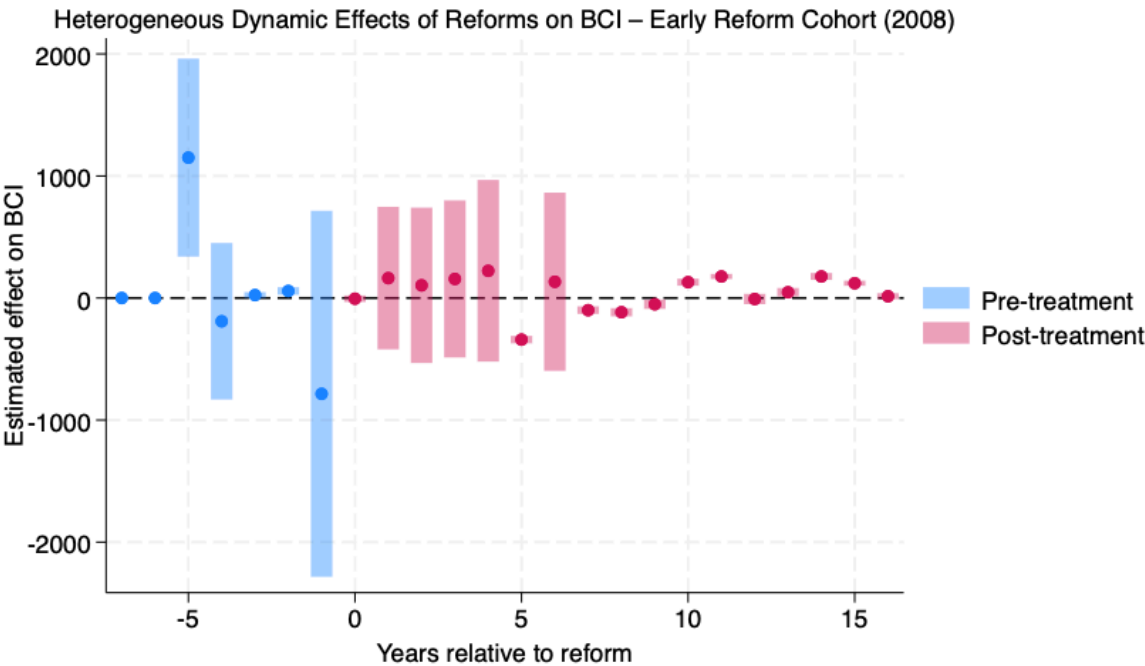
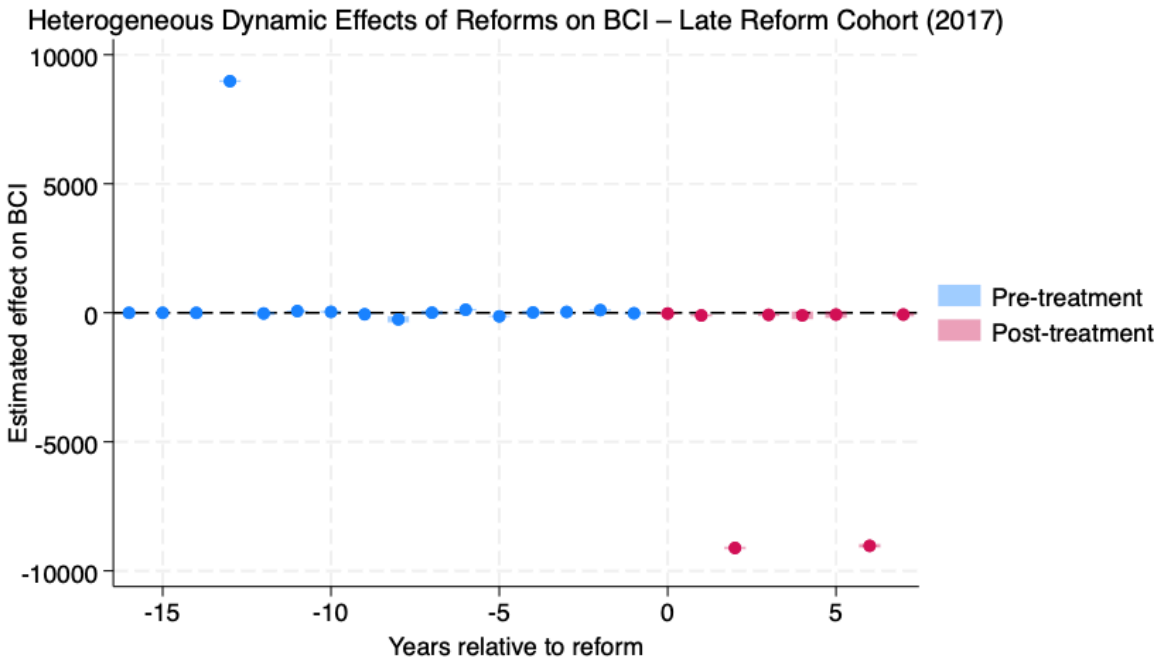


Figure D1.3. Heterogeneous dynamic effects – late reform cohort



D2. Heterogeneity by Baseline Institutional Quality

Table D2.1. Heterogeneity by institutional quality (low vs high WGI Control of Corruption; BCI and FDI/GDP)

Outcome	Cohort / Baseline Quality	Coefficient (treated)	Standard error	p-value
BCI	Low-quality countries	-1,216.5	620.20	0.069
BCI	High-quality countries	omitted (reference)	–	–
FDI/GDP	Low-quality countries	-190.3	220.4	0.401
FDI/GDP	High-quality countries	omitted (reference)	–	–

Appendix E: Governance Outcomes as Intermediate Signals

E1. WGI Indicators as Outcomes

Table E1.1. WGI as intermediate outcome (Control of Corruption and Rule of Law as dependent variables)

Outcome	Coefficient (treated)	Std. error	p-value	95% CI
WGI Corruption	-53,154	57,401	0.36	[–171,897, 65,590]
WGI Rule of Law	-114,028	48,747	0.03	[–214,869, –13,186]

Appendix F: Further Analysis: Expanded Outcomes and Mechanism Tests

F1. Expanded Outcome Set: Static and Dynamic Specifications

Table F1.1. Static Two-Way Fixed Effects model of lobbying transparency reforms and six economic indicators

Variable	BCI	FDI/GDP	GFCF/GDP	Yield Spread	ICRG Risk	Greenfield FDI
Treated	-0.286	-2.765	0.23	-0.025	0.425	-3544.542
	(0.371)	(2.001)	(0.98)	(0.27)	(1.073)	(3992.585)
Observations	597	600	600	588	528	528

Table F1.2. Lagged specification ($t + 1$ to $t + 5$) of six economic indicators

Variable	BCI	FDI/GDP	GFCF/GDP	Yield spread	ICRG risk	Greenfield FDI
L1	0.448	0.622	1.851	-0.161	0.508	-2209.437
	(0.605)	(1.976)	(1.526)	(0.35)	(1.302)	(1968.503)
L2	-0.899*	-4.445	-0.836	0.369	0.179	3926.974
	(0.454)	(3.584)	(0.681)	(0.217)	(0.501)	(3684.414)
L3	0.625**	3.225	-0.209	-0.177	0.076	-3092.161
	(0.289)	(4.171)	(0.763)	(0.131)	(0.59)	(4668.268)
L4	-0.191	-1.933	2.696	-0.154	0.396	-13.328
	(0.408)	(3.677)	(2.944)	(0.236)	(0.319)	(3232.312)
L5	-0.042	-6.814	-4.279	-0.153	-0.713	-5048.772
	(0.252)	(5.806)	(3.259)	(0.199)	(0.654)	(3196.826)
Observations	480	480	480	480	432	412

Figure F1.1. Event-study plot for GFCF/GDP

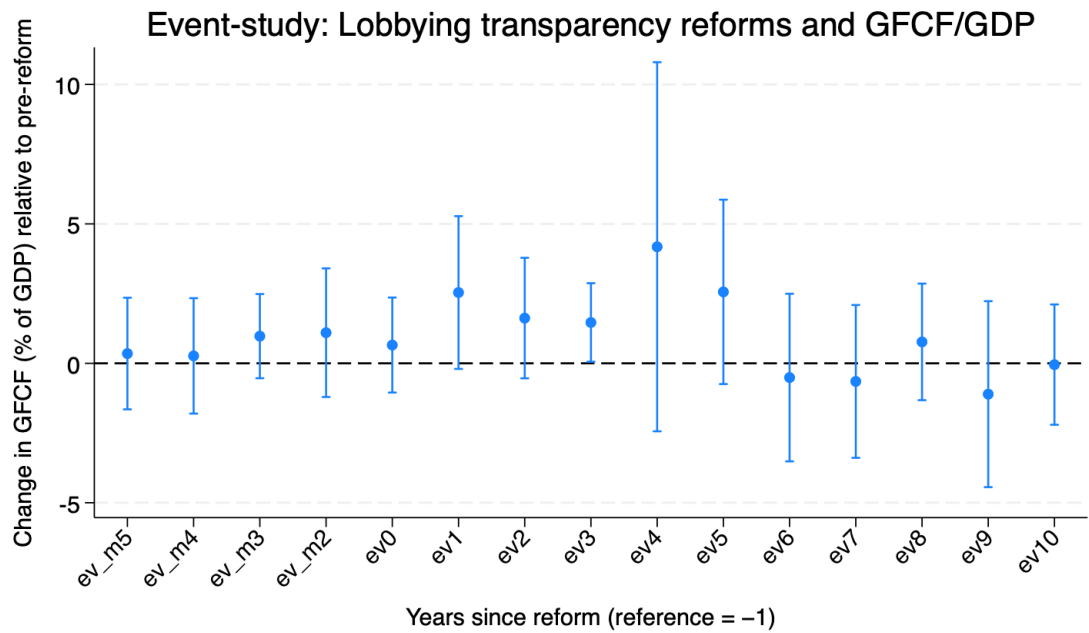


Figure F1.2. Event-study plot, yield spread

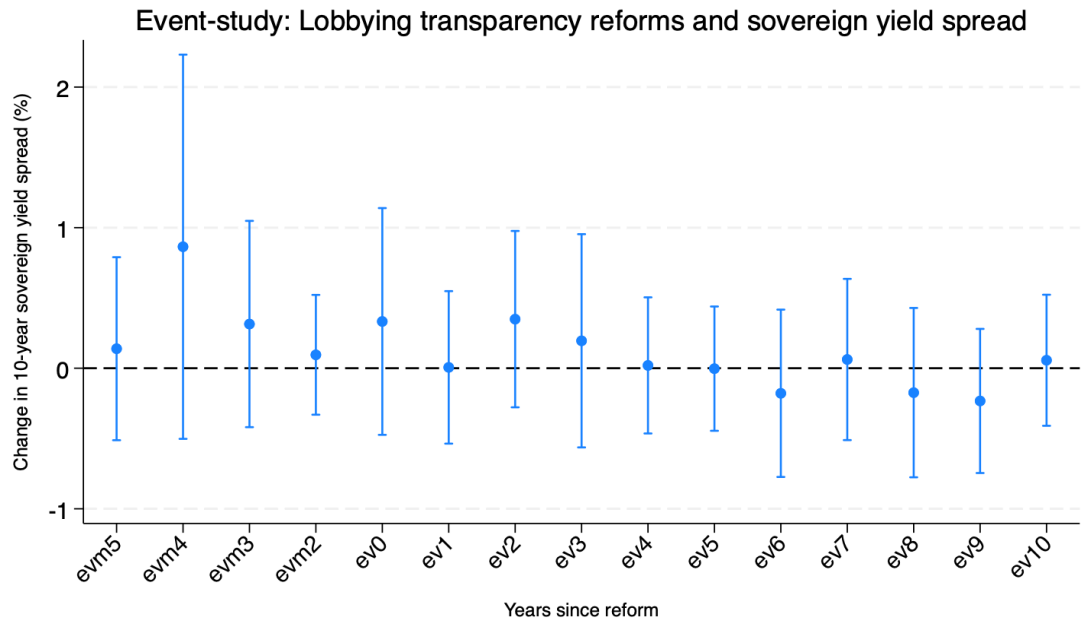


Figure F1.3. Event-study plot, ICRG political risk

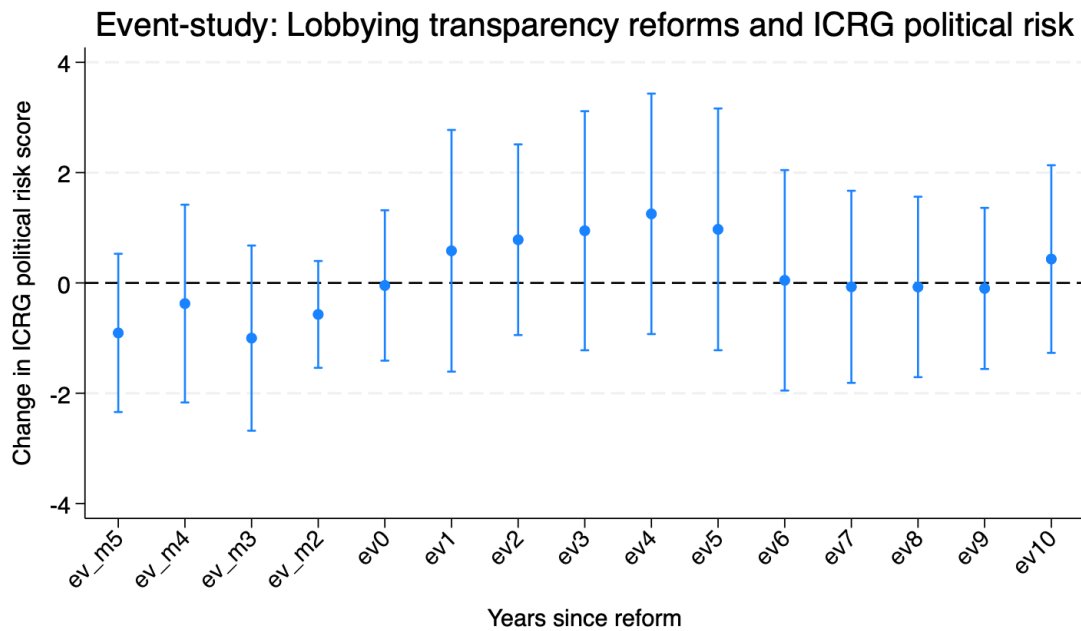
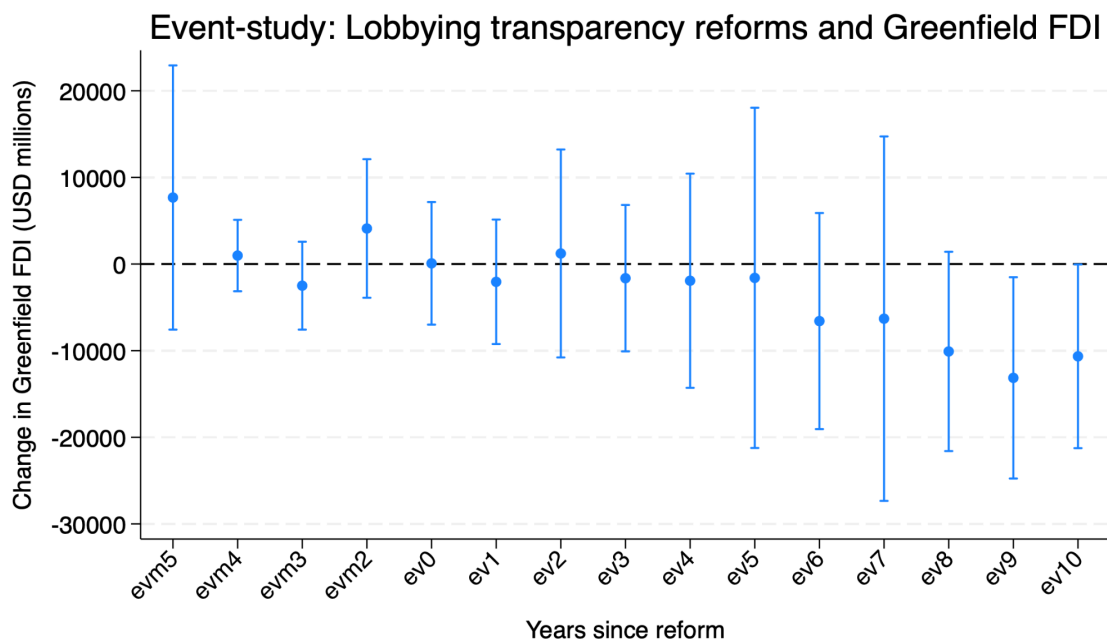


Figure F1.4. Event-study plot, Greenfield FDI



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Does Public Integrity Pay? The Effects of Lobbying Transparency Reforms on Investment Outcomes in OECD Countries, 2000 to 2024

Dumas, Oscar

Abstract

Lobbying transparency reforms are increasingly marketed by international organisations as instruments of economic governance that reduce uncertainty, stabilise regulatory environments, and attract foreign investment. The causal evidence for that proposition is absent from the literature. This study aims to fill that gap, drawing on signalling and credible commitment theory to test whether register adoption shifts investor perceptions of institutional quality and whether those shifts redirect productive capital toward reforming countries. The results of a staggered difference-in-differences panel analysis across 24 OECD democracies between 2000 and 2024 indicate that findings are null and the causal chain fails at its first link: registers as currently designed do not produce a detectable shift in perception- or risk-based indicators. These findings call for a reframing of the economic rationale international organisations have staked on lobbying transparency, a redesign of registers to meet the informational threshold a credible signal requires, and a reorientation of reform advocacy toward the democratic accountability rationale that is sustained by the evidence base.

Keywords: Lobbying transparency; Signalling theory; Foreign direct investment; OECD democracies; Staggered difference-in-differences panel design