Climate policy reforms and the acceleration of solar and wind diffusion

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Abstract

We study how climate-policy reforms changed solar and wind deployment in 49 OECD+ countries from 1990 to 2023. Because adoption follows an S-curve, we measure diffusion speed as annual log growth of installed capacity and estimate Sun–Abraham event studies around policy strengthenings. We address policy bundling with first-reform and non-overlap restrictions plus controls for simultaneous policies, and control for international spillovers. The clearest effect is an immediate rise in solar growth after feed-in-tariff price reforms. Wind responses are weaker and delayed, mainly for feed-in-tariff price and duration increases. Planning, auctions, coal-exit measures, carbon pricing, portfolio standards, and RD&D do not yield stable average accelerations once timing and pre-trends are considered. In an illustrative no-policy diffusion baseline, observed policy histories are associated with roughly double solar capacity and 30% more wind capacity since the early 2000s, with larger proportional gains when support arrives early in the diffusion process.

JEL codes: Q42, Q48, Q55, O33, H23

Keywords: technology diffusion; solar/wind adoption; policy impacts; feed-in-tariffs; coal phase-out; auctions

1 Introduction

Solar and wind capacity have expanded dramatically over the past two decades, yet deployment rates vary enormously across countries at similar income levels and with similar resource endowments. This variation cannot be fully explained by cost differences or by demand growth alone (IEA 2021; IRENA 2020). Indeed, solar is now among the lowest-cost sources of electricity in many settings (IRENA 2024; Baumgartner and Farmer 2025) and has fallen globally thanks to a globalized solar photovoltaic (PV) module supply chain

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(Helveston et al. 2022). A central candidate explanation is policy. Adoption of new technologies, including solar and wind, is often well described by an S-curve: slow initial uptake, faster expansion, and eventual saturation (Grübler et al. 1999; Rogers 2003; Mansfield 1961; Geroski 2000; Comin and Hobijn 2010; Tankwa, Bassat, et al. 2025). This pattern implies that deployment dynamics vary across stages of diffusion and may reflect both policy interventions and endogenous forces such as learning, infrastructure development, and market formation. The policy-relevant question is therefore how specific instruments alter the timing and speed of deployment relative to this underlying trajectory, and whether acceleration is large enough to matter on climate-relevant timescales (IEA 2021; IRENA 2020).

This paper asks how specific policy instruments alter the speed at which solar and wind diffuse, when those effects materialise, and how large they are relative to the underlying trajectory. The key evaluation problem is that technology diffusion can accelerate even without new policy as a technology moves from early adoption toward widespread use, typically along an S-curve. We therefore account explicitly for non-linear adoption paths, distinguishing policy-induced acceleration from deployment that would have occurred through the natural diffusion process.

We combine harmonised policy data with annual solar and wind capacity for 49 OECD+ countries from 1990 to 2023. The design exploits variation in the timing and intensity of national reforms across feed-in tariffs, auctions, tax incentives, renewable portfolio standards, carbon pricing, and coal-exit policies. We compare deployment around these reforms with contemporaneous never- and not-yet-treated countries, while conditioning on global trends, country characteristics, and other policy changes. We test for the main threats to cross-country policy evaluation, including bundled reforms, subnational implementation, pre-trends, and international spillovers.

The S-curve diffusion perspective motivates our modelling choice. In the early, exponential phase of diffusion that solar and wind still occupy, a one-off addition to capacity is soon dwarfed by the trend: any policy effect that does not alter the growth rate is quickly forgotten. A change in the growth rate, by contrast, compounds – even a temporary acceleration shifts the entire subsequent capacity path and brings every later milestone forward, although their magnitude and duration remain empirical questions rather than implications imposed by the S-curve framework. Figure 1 illustrates this distinction using Dutch solar PV. Installed capacity follows an approximately exponential path over 2013–2023 (Panel A). Although proportional growth was virtually identical in 2015 and 2020 (Panel C), annual additions were nearly seven times larger in 2020 (Panel B), reflecting the larger installed base rather than faster diffusion. A specification in absolute additions would therefore assign very different outcomes to two years with essentially the same diffusion rate. We therefore measure deployment responses as the changes in diffusion speed.

Three patterns emerge from our findings. First, policy changes tend to bring forward deployment: the growth rate rises around treatment and then returns toward its baseline path. As the S-curve logic implies, these temporary accelerations still cumulate into sizeable level differences. Second, policy design and technology matter. The evidence is strongest for solar FIT-price reforms and delayed wind FIT-duration reforms. Positive profiles for solar FIT duration and renewable-expansion planning are more sensitive to pre-trends or horizon choice. By contrast, auctions, coal-exit measures, portfolio standards, and carbon pricing (economy-wide pricing instruments) do not yield stable average accelerations across specifications. Comparing instrument families, subsidy-type tools systematically outperform trading schemes: ETS and

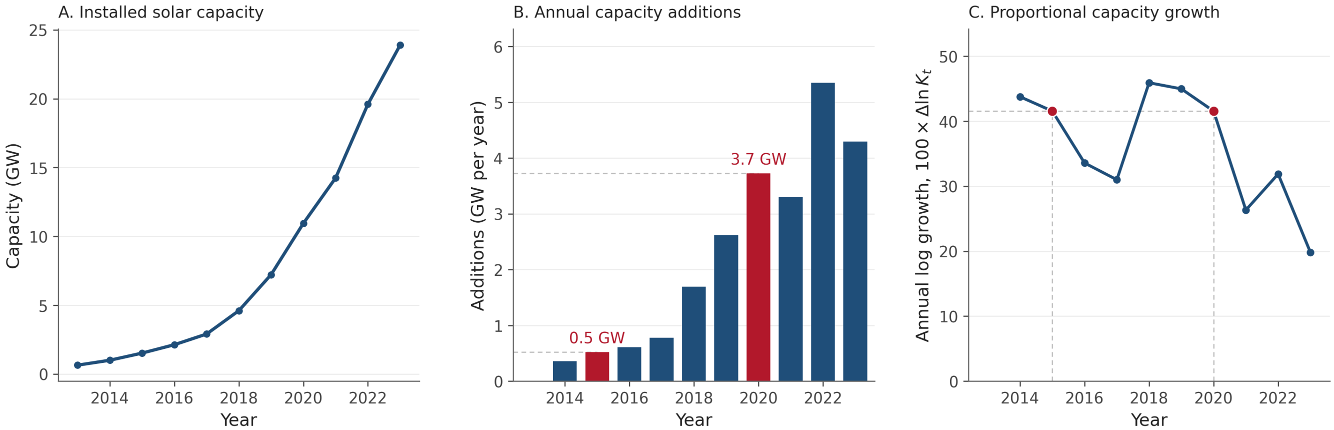


Figure 1: Absolute additions and proportional growth in Dutch solar PV. Panel A shows cumulative installed capacity, K_t; Panel B shows annual additions, $\Delta K_t \equiv K_t - K_{t-1}$; and Panel C shows annual log growth, $100\Delta \ln K_t \equiv 100[\ln K_t - \ln K_{t-1}]$. Red markers identify 2015 and 2020. Capacity grew by 41.6 log points in both years, but annual additions were approximately seven times larger in 2020 (3.7 GW versus 0.5 GW). Thus, similar proportional diffusion can produce very different absolute additions as the installed base expands. The sample begins in 2013, when solar first exceeded 1% of national electricity capacity, consistent with the Data strategy (See Appendix A). Over 2013–2023, a fitted exponential (on panel A) has a slope of 37.1 log points per year ($R^2 = 0.995$).

carbon taxes do not show consistent increases in adoption speed. Third, timing is more important than mere policy-existence. Among screened positive episodes, earlier reform timing is associated with larger proportional capacity gains, especially for solar. We interpret this final pattern as descriptive heterogeneity rather than as a causal law of policy timing.

The results are robust to restricting the sample to first or isolated reforms; accounting for neighbouring deployment, policies, and electricity-trade exposure; and allowing separate event-time dynamics for federal countries. Common global cost trends are absorbed by year fixed effects, while remaining cross-border spillovers would tend to attenuate the estimates. We reserve causal interpretation for policy families that pass joint pre-trend tests. As further validation, policy *weakenings* produce corresponding slowdowns. Together, these results indicate that observed policy bundling, cross-border linkages, and federal policy measurement do not drive our main findings.

To assess quantitative importance, we embed screened event-study estimates in country-specific S-curve baselines and simulate a no-policy counterfactual. We add overlapping policy contributions, limited to 5 years, in log-growth and compare this path with a constant-growth baseline calibrated from the four pre-reform years. This illustrative accounting assumes additive effects and includes only responses that pass our statistical screens. The median mapped policy-on/no-policy capacity ratio is about 2.0 for solar and 1.3 for wind, implying model-based capacity differences of roughly 100% and 30%, respectively. The policy-on path reaches its 2023 capacity two to three years earlier in the median country, and up to 15 years earlier in the most affected cases. These counterfactual shifts are larger in some countries and modest in others, indicating that policy materially affects the timing of diffusion even when it does not fully determine eventual renewable shares.

We make three contributions to several strands of literature. First, we anchor the measurement of policy impact in a theory-informed diffusion baseline. We study technology adoption in capacity terms and interpret it within an S-curve framework, so that the natural tendency toward accelerating diffusion is not mistakenly attributed to policy. This aligns the outcome with actual technology take-up and avoids additional noise from denominators, weather, fuel cycles, or inventory methods that affect generation shares and economy-wide emissions (Mansfield 1961; Rogers 2003; Bass 1969; Fisher and Pry 1971; Marchetti and Nakićenović 1979; Meade and Islam 1998; Comin and Hobijn 2010; Way et al. 2022). In contrast, Stechemesser et al. (2024) examine a large multi-country panel in which composite climate-policy indices are regressed directly on aggregate CO_2 emissions, providing valuable evidence on mitigation but not on technology-specific adoption dynamics, while Zhao et al. (2013) relate policy support to broad renewable indicators using policy presence and counts without an explicit diffusion benchmark. Our approach complements these studies by combining technology-specific capacity paths, a diffusion baseline, and design-based measures of policy stringency.

Second, we contribute to the empirical literature evaluating the impact of policies on renewable deployment. A substantial body of work links policies to clean-energy invention, learning, and investment (Newell et al. 1999; Popp 2002; Johnstone et al. 2010; Tankwa and Barbrook-Johnson 2025), and cross-country panels have connected particular deployment instruments to build-out. For Feed-in-Tariffs/premiums, Jenner et al. (2013) and Smith and Urpelainen (2014) show that tariff level, duration, and degression are strongly associated with increases in renewable capacity and generation, while Dijkgraaf et al. (2018) confirm for PV that design details shape the size and persistence of additions. However, these studies focus mainly on pre-2010 EU samples using early and sometimes unstable data, which later work suggests can bias elasticities (Baumgärtner et al. 2024). Other cross-national analyses examine policy mixes or counts but rely on coarse proxies for deployment (aggregate "renewables" shares, investment flows), short time windows, or small country samples that limit generalisability (Carley 2009; García-Álvarez et al. 2017; Matthäus 2020; Xin-gang et al. 2020). Parallel quasi-experimental studies deliver clean identification for single instruments – such as the UK Carbon Price Support (Leroutier 2022) or specific U.S. renewable portfolio standard designs (Deschenes et al. 2023; Joshi 2021; Feldman and Levinson 2023) – but are necessarily narrow in scope. We extend this literature by providing comparable, long-horizon adoption dynamics for solar and wind across many OECD+ countries and a broad set of instrument families, using harmonised stringency measures and a heterogeneity-robust event-study framework.

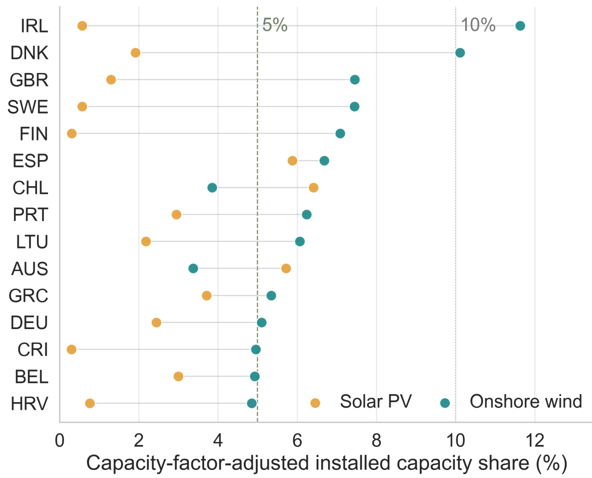
Third, we connect to the emerging literature that quantifies the magnitude of policy impacts and attributes emissions reductions to specific instruments. Recent work estimates sizeable mitigation effects for carbon pricing and other climate policies, often with careful quasi-experimental designs (Stechemesser et al. 2024; Leroutier 2022; Bayer and Aklin 2020; Greenstone and Nath 2020), and synthesises trade-offs across instrument types (Peñasco et al. 2021). Most of these contributions, however, target emissions rather than technology adoption or focus on one policy family in one region. We instead map shocks to the speed of diffusion into counterfactual capacity paths over a diffusion baseline, quantifying short- to medium-run contributions of different instruments to deployment. The closest studies to ours are Deschenes et al. (2023), Joshi (2021), and Feldman and Levinson (2023), who exploit heterogeneous U.S. state adoption of renewable portfolio standards to estimate how much wind and solar generation (or generation shares) can be attributed to renewable portfolio standards (RPS) obligations. Our analysis differs by covering multiple instrument families rather than a single standard, tracking technology-specific capacity instead of generation shares alone, and embedding dynamic effects in an explicit diffusion baseline. This allows us to compare instruments on common terms – harmonised stringency, event-time responses, and induced deviations from the underlying S-curve trend.

From a policy perspective, the contribution is to identify which instruments, in practice, have delivered sizeable and durable increases in solar and wind deployment, when those effects arise in the diffusion process, and how large they are relative to the underlying trend. That information is directly relevant for designing policy portfolios that can scale low-carbon technologies fast enough to meet climate goals.

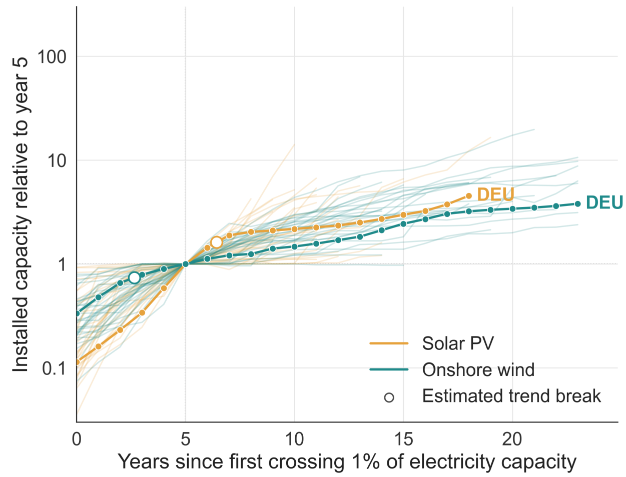
2 Background and stylized facts

This section assembles four stylized facts that anchor our empirical design and give preliminary findings.

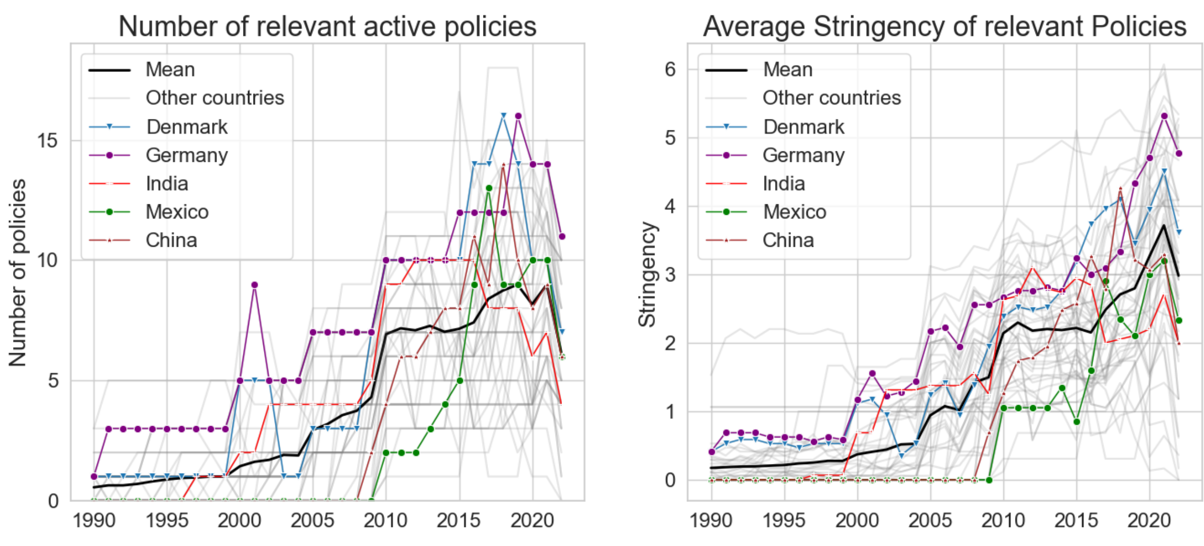
- **Fact 1:** In all OECD+ countries, solar and wind represent a small share of energy capacity and production, making early-phase approximations empirically relevant.
- **Fact 2:** Long-run global as well as national diffusion is well described by logistic S-curves.
- **Fact 3:** Country series exhibit discrete accelerations that depart from a smooth S-shaped path.
- **Fact 4:** Renewable-support policies have multiplied and increased their stringency over time with various mixes and calendars for each country. The resulting timing variation motivates a stacked event-study with country and time fixed effects.



(a) Solar and wind penetration remains low



(b) Adoption paths exhibit trend breaks



(c) Policy portfolios became denser and more stringent

Figure 2: Stylized facts motivating the empirical design. Panel (a) shows the penetration (share of capacity) of solar and wind for the top 15 (out of 49) OECD+ countries with adoption data and policy data as of 2023, showing solar and wind remain in early diffusion across most OECD+ countries. Panel (b) shows the log installed capacity of wind and solar, normalised to 1 and relative to the year where it crosses 1% of electricity capacity, with discrete trend breaks. Panel (c) shows the evolution of renewable support policy intensity across 22 instruments in 49 OECD+ countries. Left: count of active policies. Right: average stringency of active policies. The black line denotes the cross-country average and gray lines represent other countries. Highlighted cases – Canada, Germany, the UK, India, and China – illustrate the diversity of national contexts yet a shared pattern of rising policy engagement.

2.1 Fact 1: Solar and wind are still in early diffusion

The global penetration of solar and wind remains low. In most countries, solar still supplies less than 5% of total energy use and wind less than 10%. Figure 2a shows that even frontrunners such as Hungary and the Netherlands (solar) or Denmark and Ireland (wind) typically obtain under 15% of their total final energy from these sources, and their shares in total useful energy consumption are smaller still. Globally, solar and wind (including offshore wind) together provided only around 15% of electricity in 2024 (Wiatros-Motyka et al. 2024).

Current penetration levels remain far below plausible long-run ceilings. Net-zero scenarios project solar and wind supplying roughly two-thirds of global electricity by mid-century, with renewables approaching 90% of total generation.[1]

Early diffusion is also the period in which deployment and cost dynamics are most tightly coupled. Experience curves describe how cost decline follows cumulative deployment of the technology (Wright 1936; Nemet 2006; Lafond et al. 2018; Farmer and Lafond 2016; Way et al. 2022). This creates a dynamic complementarity between deployment and technology progress: policies that push adoption today can accelerate future cost reductions, which in turn make further deployment more attractive. We do not decompose innovation or learning effects in this paper. Instead, the early-diffusion fact motivates our focus on realised deployment: if solar and wind remain far from saturation, temporary policy-induced changes in growth rates can cumulate into large differences in capacity and in the timing of scale-up.

2.2 Fact 2: Long-run national-level technology diffusion is well approximated by S-curves

Innovations typically diffuse along *S-shaped* paths, because adoption is reinforced by social learning and imitation: the propensity to adopt rises with the stock of prior adopters, generating the familiar slow–fast–slow pattern of takeoff, rapid ascent, and saturation (Mansfield 1961; Rogers 2003; Bass 1969). While the eventual ceiling and the pace of ascent vary widely across countries and technologies (Geroski 2000; Comin and Hobijn 2010), the logistic form provides a robust long-run summary: Tankwa, Bassat, et al. (2025) fit more than 3,000 country–technology series across 27 technologies and find that national adoption is well described by logistic S-curves over the full diffusion arc, with only modest, domain-specific noise.

Figure 2b shows that, for the countries with the highest solar and wind capacity shares, a log-linear approximation tracks the observed paths well. Because most countries remain in the formative or early-growth stage of the curve (Section 2.1), where the logistic is approximately exponential, we can characterise diffusion *speed* without estimating each country's eventual saturation level L_i – the parameter that is hardest to identify before an inflection point is observed. This is the property our design exploits: we measure policy effects as deviations in the growth rate k around a smooth diffusion baseline, rather than against an assumed long-run ceiling.

2.3 Fact 3: Adoption profiles exhibit trend breaks

A common working assumption in national adoption studies is that phases of diffusion (formative, growth, saturation) connect smoothly so that long-run dynamics can be summarized by a single trajectory[2]. In the early stage, this implies that the log of cumulative adoption is close to linear in time. Conditional on the logistic providing a smooth benchmark for national diffusion, we then characterise departures from

[1] For example, the IEA's *Net Zero by 2050* scenario and related analyses project that solar and wind together approach 70% of global electricity by 2050 (IEA 2021; IRENA 2020).

[2] However, Fisher–Pry's logit transform yields piece-wise-linear substitution paths representing growth, saturation, and collapse stages of adoption (Fisher and Pry 1971; Grübler et al. 1999)

it by fitting continuous piecewise log-linear segments to log cumulative adoption using an automated break-detection procedure (details in Appendix D).

Using this procedure, we detected **122** breaks across the panel under the strong setting (p-value significance and break separation threshold see Appendix D): **55** in solar and **67** in wind. Wind shows more breaks, potentially explained by the technology having a longer diffusion history in many countries.

These results indicate that, even in the early regime where log-linear growth is a good first approximation, national adoption frequently exhibits statistically discernible departures.

The break patterns are therefore not themselves evidence of policy effects; they are evidence that a smooth diffusion baseline is incomplete for annual deployment dynamics. They motivate an event-time design that can distinguish anticipation, delayed construction, short-run acceleration, and reversion toward baseline growth.

2.4 Fact 4: Policy use expanded in number and stringency

Figure 2c shows that policy support for renewables broadened and deepened over three decades. The left panel shows a steady rise in the number of active instruments, with a clear step-up after 2005 and another around 2012–2016. By the early 2020s, the average OECD+ country runs close to eight relevant instruments simultaneously, compared with roughly one to three in the 1990s. The right panel tracks a parallel increase in *stringency*: there are more instruments in force, but the typical instrument is stronger than its earlier counterpart. The co-movement of counts and stringency suggests policy *layering* (new tools added on top of existing ones) as well as periodic *upgrades* (stronger targets, tighter eligibility, higher prices, or larger volumes).

The composition of support also shifts over time, from early Feed-in-tariffs towards competitive auctions and market-integrating measures; with carbon pricing growing in salience where present and RD&D pivoting toward systems integration after 2015[3]. Early leaders (the UK, Germany), rapid scalers (India), and later adopters (China, Mexico) follow different calendars but converge toward higher policy density and stringency by 2020–2022. These instruments are not interchangeable: they turn public intent into project revenue through different channels – guaranteed offtake, competitive contracts, tradable obligations, or operating-margin effects (Đukan and Kitzing 2023; Alcorta et al. 2024; Polzin et al. 2019). This is why we keep policy families distinct rather than collapsing them into a single climate-policy index, and why instrument *design*, rather than mere presence, is likely to govern deployment – a mechanism we develop in Section 6. Climate legislation arrived in pulses rather than a single global trigger (Eskander et al. 2021). Adoption itself typically responds to falling technology costs, prior deployment, previous policies[4] and political pressure. The empirical analysis therefore does not treat policy events as isolated shocks in the full sample; it compares dynamic adoption paths around reforms while conditioning on country fixed effects, common year shocks, and contemporaneous policy portfolios, before turning to stricter subsamples with cleaner timing.

These patterns motivate three design choices. First, we use harmonised, design-based stringency measures rather than policy-presence indicators. Second, we condition on co-moving instruments, because countries layer and upgrade portfolios rather than adopting policies in isolation. Third, we use dynamic event-study

[3] Consistent with earlier work such as Eskander et al. 2021; Mealy et al. 2025

[4] Studies of policy mixes emphasize sequencing along a "feasibility frontier," where relatedness predicts the next instrument and balanced portfolios support innovation and deployment (Kern et al. 2019; Mealy et al. 2025).

estimators that compare instruments on common, technology-specific adoption outcomes while allowing responses to differ by timing and design.

3 Data

3.1 Technology adoption data and sample construction

Our adoption measure is S_{it}, the cumulative installed capacity (MW) of solar or wind in country i and year t. We track capacity rather than generation so that the outcome reflects deployment decisions directly, without the weather, fuel-cycle, and accounting variation that affects generation-based shares; we confirm that the main results carry over to a generation-based outcome in Appendix B.4.

Coverage and sources. We assemble an annual country–technology panel for utility-scale solar photovoltaic ("solar") and onshore wind ("wind") spanning 1990-2023. Capacity (MW), generation (MWh), and cost (USD/kW) are compiled from Baumgartner and Farmer (2025) with original series from BP (2022), IEA (2023), and IRENA (2023), complemented by adoption, cost, and electricity generation detail from IRENA (2025) and IRENA (2024). Macro-economic covariates are primarily taken from the World Development Indicators (World Bank 2025).

The policy data come from the OECD *Climate Actions and Policies Measurement Framework* (CAPMF), developed under the International Programme for Action on Climate (Nachtigall et al. 2024; OECD 2025). CAPMF tracks about 130 policy variables that are aggregated into 56 policy instruments (and other climate actions) for OECD countries plus partner economies (e.g. Brazil, China, Indonesia, South Africa, Argentina) and EU aggregates over 1990–2023.[5] CAPMF track active policies, thus policies appear in the database at their first implementation date.

Sample restrictions. The analysis targets utility-scale diffusion dynamics. We therefore apply three restrictions designed to exclude settings where annual capacity changes reflect discrete project timing rather than incremental market adoption. First, we drop countries with fewer than ten years of non-missing observations for the technology in question. Second, to focus on utility-scale deployment, we exclude additions below 1 MW. Third, we remove cases with minimal deployment where activity is dominated by one or a handful of large projects, leaving no meaningful market dynamics to observe.

3.2 Policy data and instrument granularity.

At the most detailed, variable level, each policy is described by quantitative fields (e.g., tax rates, coverage, benchmarks, permit prices, auction volumes) and qualitative design features. CAPMF groups these variables into broader *instrument families* (56 types in total; e.g., carbon taxes, ETS, renewable auctions, Feed-in-Tariffs/premiums, RECs, fossil-fuel subsidy reform, standards, public finance, and R&D support).

For each country–instrument–year, CAPMF provides a normalised *stringency* score on $[0, 10]$, constructed from the in-sample distribution of the underlying variable levels (e.g., tax rate and coverage for a carbon tax; permit prices and coverage for ETS; auction or FiT parameters for renewables), so that 0 indicates no or

[5] See OECD CAPMF overview and documentation for the variable-to-instrument mapping and annual updates.

minimal stringency and 10 very stringent design.[6] Because the stringency score is defined within instrument families, coefficients should be interpreted as responses to marginal increases in *relative* policy stringency within a given instrument type, rather than directly comparable intensity units across different instruments.

In our analysis, we work at the instrument-family level, focusing on series closest to the power sector and renewables (e.g., carbon tax – electricity, ETS coverage/price, renewable auctions, FiTs/premiums, REC obligations, fossil subsidy reform), and retaining broader cross-sector items only for robustness. Because CAPMF stringency is already on $[0, 10]$, we do not further rescale it, beyond harmonising labels and ensuring a consistent instrument dictionary across countries and years.

3.3 Selecting policies relevant to wind and solar

The OECD database contains far more climate and energy policies than we can meaningfully analyse in a single technology-specific event study design. We therefore pre-specify a compact set of policy families that are both (i) directly linked to power-sector renewable investment incentives and (ii) repeatedly studied in the cross-country diffusion literature (Section 2). We first run a broad keyword pass over policy entries – *Renewables*, *Electricity*, *Coal*, and *Fossil fuels* – to define a wide candidate set. We then retain the policy types that are repeatedly studied alongside wind and solar deployment across the literature (Section 2): technology-specific price and allocation schemes (Feed-in-Tariffs/premiums, auctions/long-term contracts), renewable certificate or portfolio obligations, electricity-sector carbon price instruments and fossil price/subsidy reforms, coal-exit measures, planning targets, and renewables RD&D. We drop policies that mainly target other technologies (such as hydrogen or nuclear) or non-power sectors, and keep broad or cross-border measures only for robustness checks, not in the baseline.

After harmonisation and filtering, the regression samples contain 43 countries for solar and 40 for wind. The data cover 22 instruments grouped into 10 families and 1,098 country–instrument policy instances, of which 794 are qualifying strengthening episodes under our event rule. A reform qualifies when an instrument's CAPMF stringency score increases by at least two points on its 0–10 scale over two years. Because solar and wind are studied separately, these 794 reforms generate 1,286 treated country–technology events (1,197 after excluding countries without a usable capacity series).

4 Methods

In this section we link the diffusion model, the policy shocks, and the empirical design.

4.1 Modelling adoption

We assume national diffusion is well-approximated by an S-curve as discussed in Section 2.2 (Way et al. 2022; Nemet et al. 2023; Baumgärtner et al. 2024).

The logistic diffusion path of the cumulative deployment $S_i(t)$ at time t is

$$S_i(t) = \frac{L_i}{1 + \exp[-k_i(t - t_{0i})]}, \tag{1}$$

[6] CAPMF stringency is instrument-specific and distinct from the OECD EPS index (0–6, 13 instruments); we use CAPMF because it provides technology-relevant power-sector instruments with annual, design-based intensity.

with L_i the saturation level, k_i the diffusion speed, and t_{0i} the inflection time ($S = L/2$). It is widely used in technology diffusion studies due to its simplicity and robust empirical fit. (Meade and Islam 1998; Tankwa, Bassat, et al. 2025).[7]

For the early stages $S_{it} \ll L_i$, relevant to wind and solar (see Section 2.1), it approximates to

$$\log S_{it} \approx \log L_i + k_i(t - t_{0i}) \equiv k_i t + c_i, \tag{2}$$

with c_i constant and $k_{it} \equiv \frac{d \log S_{it}}{dt} \approx \Delta \log S_{it}$ the diffusion speed.

4.2 Identification strategy: Sun–Abraham event study

Our objective is to estimate how policy onsets or strengthenings shift the diffusion *speed* $k_{it} \equiv \Delta \log S_{it}$ (cf. Equation 1 and Equation 2). Let E_i denote the year of the qualifying event in the focal policy for country i, and define relative time $\ell_{it} = t - E_i$. Countries that share the same event year $E_i = e$ form a *cohort*.

We summarise the dynamic policy effects with a simple relative–time difference–in–differences specification:

$$k_{it} = \sum_{\ell=-K}^{M} \nu_\ell \, \mathbf{1}\{\ell_{it} = \ell\} + \boldsymbol{\delta}^\top \mathbf{X}_{i,t-1} + \mu_i + \gamma_t + \varepsilon_{it}, \tag{3}$$

where $\mathbf{1}\{\ell_{it} = \ell\}$ is an indicator that country i is ℓ years away from its event in year t ($\mathbf{1}$ is equal to 1 if the condition is satisfied and 0 otherwise), with $\ell = -1$ omitted as the reference period.[8] The vector $\mathbf{X}_{i,t-1}$ collects lagged country–year covariates (Section 4.5), and μ_i and γ_t are country and year fixed effects. In the figures we plot ν_ℓ and associated confidence intervals over the window $\ell \in [-K, M]$ (we set $K, M = 5$ for the main specification).

Intuitively, the coefficients ν_ℓ trace out the average change in diffusion speed ℓ years before and after the policy event, relative to the pre–event year $\ell = -1$, comparing countries that experienced an event to countries that are never- or not–yet treated in the same calendar year.

In estimation we use the interaction–weighted event–study estimator of Sun and Abraham (2021). The estimator first recovers cohort–by–relative–time coefficients for each cohort e and then aggregates them into the event–time averages ν_ℓ using explicit weights; formally, ν_ℓ is the weighted average in Appendix 19. Full details of the cohort structure, stacking across events, and aggregation are provided in Appendix C.1.

We prefer this approach to traditional two–way fixed–effects (TWFE) regressions because our setting combines staggered adoption with heterogeneous dynamic responses across cohorts. Under these conditions, TWFE estimators generally recover non–convex averages of cohort– and period–specific effects, sometimes with negative weights, which complicates interpretation and can yield biased estimates of the average effect on the treated (Goodman-Bacon 2021; Chaisemartin and D'Haultfoeuille 2020; Sun and Abraham 2021). By contrast, the Sun–Abraham estimator targets well–defined cohort–specific event–time effects under cohort–specific parallel trends and provides directly interpretable dynamic paths around policy events.

Interpretation of the estimand. With repeated reforms, overlapping policies, and common shocks, estimates are event-conditional, history-dependent averages rather than single-lever effects. They compare

[7] Note that in the early phases of deployment, using alternative frameworks such as a Bertalanffy-Richards, a logistic or Gompertz curves, would have no impact as they all model the early phases as exponential.

[8] Therefore, each ν_ℓ is interpreted relative to the year just before the event.

diffusion speed around focal reforms to not-yet-treated countries, conditional on covariates and other policies. We assess parallel trends using first reforms, and heterogeneity using stricter, separated episodes.

Because cumulative capacity follows an S-curve, $\Delta \log$ capacity is mechanically largest at low penetration, so early adopters grow quickly irrespective of policy. The design isolates the policy component of this growth: country fixed effects absorb time-invariant differences in adoption stage, and identification rests on event-time deviations from that trajectory rather than on the level of growth. As a direct check, restricting the sample to later stages of adoption (where mechanical growth is small) attenuates the responses but preserves their sign and timing (Appendix B.5.4).

We report standard errors clustered at the country level. For each policy family and event definition, we test whether pre–treatment dynamics are consistent with the parallel–trends assumption using a joint Wald test (see Appendix B.3).

4.3 Defining policy events

Let $P(t) \in [0, 10]$ denote the policy stringency at time t. We define a policy event as any change in stringency over a window of $\tilde{\tau}$ years that exceeds a minimum threshold $\tilde{\eta}$. Formally,

$$E_t = \mathbf{1}\{|P(t) - P(t - \tilde{\tau})| \geq \tilde{\eta}\} \times \eta_t \tag{4}$$

where

- $\tilde{\tau}$ is the evaluation horizon, typically two years for "regular" (non-sharp) events (Stechemesser et al. 2024), or one year for sharp events,
- $\tilde{\eta}$ is the stringency threshold, set to 2 for "significant" events (again consistent with Stechemesser et al. 2024), or to 0 when testing for any policy change as a robustness check,
- $\eta_t = P(t) - P(t - \tilde{\tau})$ is the net change in stringency over the event window.

Therefore, $E_t > 0$ denotes a strengthening and $E_t < 0$ a weakening in policy. This approach aligns with recent practice in large-scale ex-post policy evaluations (e.g. Stechemesser et al. 2024). It allows us to filter out minor fluctuations and focus on substantial changes, improving the signal-to-noise ratio when identifying policy events. In our baseline specification we use a significant-gradual-2y events ($\tilde{\eta} = 2$,$\tilde{\tau} = 2$).

4.4 Identifying assumptions and threats to interpretation

Our estimates compare changes in capacity growth around policy reforms with changes in countries that are never treated or not yet treated. A causal interpretation requires that, absent the reform, these groups would have followed similar trends after conditioning on the fixed effects, covariates, and non-focal policy controls included in the baseline specification. The Sun–Abraham estimator addresses staggered treatment timing, but it does not make policy adoption exogenous. Section 5.5 addresses the main threats discussed below with multiple robustness tests and shows that our estimation remains credible.

Anticipation and endogenous policy timing Policy reforms may respond to prior deployment trends, while developers may anticipate announced reforms. Both mechanisms can shift capacity growth before the recorded event: firms may delay commissioning in expectation of more favourable rules, or governments may strengthen support after deployment slows. We therefore use pre-treatment coefficients to assess whether treated and comparison countries were already on different paths. When clear pre-trends appear, we interpret post-reform estimates as associations rather than causal effects. Policy timing is also imperfect: reforms may be announced before implementation, phased in gradually, delayed administratively, or recorded approximately in CAPMF. Estimated responses may therefore spread across neighbouring event years. Our baseline defines events as large two-year stringency changes, and we test robustness to sharper one-year and any-change thresholds.

Policy bundling and repeated reforms Energy policies are often adopted as packages rather than isolated instruments. Targets, feed-in tariffs, auctions, grid reforms, certificate obligations, and carbon pricing may change close together, so responses around a focal event may partly reflect concurrent reforms, while repeated reforms within a policy family complicate interpretation of later episodes. Identification therefore requires that, conditional on country and year fixed effects and observed contemporaneous policy changes, focal reform timing is unrelated to counterfactual diffusion-speed trends. We make this assumption more credible by controlling for stringency changes in all non-focal policy families and testing first-reform and temporally isolated subsamples. Still, unobserved or poorly measured complementary reforms may remain, so estimates reflect focal reforms in their usual policy context.

Cross-country spillovers A related concern is that reforms and deployment outcomes may diffuse across countries. Governments may imitate neighbours or peer economies, respond to regional institutions, or be affected by shared supply chains, developers, expertise, and demonstration effects. If these dynamics correlate with domestic reform timing, event-study coefficients may partly capture regional diffusion rather than the reform itself. Year fixed effects absorb common global shocks but not localised spillovers, so we control for nearest-neighbour technology deployment and policy changes. Electricity trade is another possible spillover channel, especially in highly interconnected systems such as Europe, where treatment in one country may affect comparison countries. We therefore also control for electricity-market integration using trade intensity. Technology-cost spillovers raise a related but distinct concern, addressed separately below.

Cost endogeneity Deployment is intrinsically linked to the cost of the technology, while learning curves show us that more deployment leads to cost decreases. We argue that in our cases, national extra deployment is unlikely to influence technology prices. We exclude technology cost measures from the baseline specification but treat cost-controlled estimates as conservative robustness checks.

In both technologies, year fixed effects absorb common global cost movements. For solar, this concern is mitigated by the global structure of the PV supply chain. Solar modules are traded internationally in a market strongly shaped by Chinese manufacturing scale (Kavlak et al. 2018; Nemet 2019; Helveston et al. 2022). While learning-by-doing is substantial, with estimated module learning rates around 20% per doubling of cumulative capacity (Lafond et al. 2018; Way et al. 2022), most national policy episodes are too small to materially shift global costs on their own. Even the largest early episode in our sample, Germany's EEG-driven solar expansion in 2010–2012, would imply only a modest contribution to global module cost decline relative to worldwide cumulative deployment.

For wind, the corresponding concern is weaker. Cost reductions over our sample period are driven mainly by turbine scaling—larger rotors, taller hub heights, and higher capacity factors—rather than by country-specific cumulative deployment (Wiser, Bolinger, et al. 2023; Wiser, Rand, et al. 2021). Since turbines and design improvements diffuse through global manufacturers, these cost trends are largely common across countries.

4.5 Covariates and policy controls

Covariates and operationalisation. Our event-study specification conditions on a vector of country–year controls, which we denote $\mathbf{X}_{i,t-1}$ to emphasise that they are entered with a one-year lag. The selection is guided by the literature on national technology adoption and renewable deployment[9]. Specifically, we include: (i) macro scale and dynamics - log(GDP per capita) and $\Delta \log(\text{GDP})$ - to absorb income effects on electricity demand and fiscal space; (ii) factor-accumulation and openness - gross fixed capital formation (%GDP) and inward FDI (%GDP)- to proxy investment depth, technology inflows, and exposure to global equipment markets; (iii) hydrocarbon endowments - oil rents (%GDP) - to capture opportunity-cost and political-economy effects; (iv) innovation intensity - R&D (%GDP) - as a proximate driver and mediator of policy impacts (Sadorsky 2009; Przychodzen and Przychodzen 2020). Because renewable build-out is sensitive to financing conditions (Newbery 2016; Khodadadi and Poudineh 2024; OECD 2024), we include a proxy for the country-level cost of capital: the nominal yield on a benchmark sovereign bond (typically 10-year, or the closest available tenor). This proxy captures macro risk premia and policy credibility that transmit to project cost of capital. We control for electricity-system scale using total installed capacity expressed as a share of each country's 2023 electricity capacity level. This proxies "headroom" and grid/EPC processing capability that mechanically constrain feasible build-out; fixing the terminal-year denominator avoids simultaneity with policy-driven additions. We can control for costs through total CAPEX $ per kW but refrain from using this in the main specification due to cyclicality concerns (see Appendix B.7.5 for the robustness checks including cost control).

Other–policy controls. To absorb concurrent *non–focal* policy co-movements without partialling out the focal treatment, we add a block of policy covariates for all families other than the focal one. Specifically, for each non-focal policy family we include, at time t, the one-year *lagged change* in its stringency score (the difference between its level at $t-1$ and at $t-2$). The focal family is excluded from this control vector by construction.[10]

We consider policy events within the window as independent, because their calendars are predetermined relative to the time-t outcome (no bunching), which does not trigger revisions in those instruments. Conditioning on them at post-event horizons does not induce bad-control bias under the assumption that a focal reform does not itself trigger reforms in other policy families. Common shocks are absorbed by country and year fixed effects. We test in a robustness check section (Appendix B.7) that these covariates do not drive the results by removing them and restricting the sample to isolated events with no policy overlap.

[9] We check by method of variance inflation factors that none of our variables cause multi-collinearity issues and find no concerning variables - we therefore proceed with the full set. In handling missing data we linearly interpolate within country before lagging only for short gaps (up to two consecutive years) and never borrow information across countries.

[10] As robustness, we replace these lagged changes with lagged non–focal *event sizes* $S^{(m)}_{i,t-1}$, *active* event dummies $A^{(m)}_{i,t-1}$, or *cumulative* event counts $C^{(m)}_{i,t-1}$. Appendix B.7.3 shows that focal dynamic paths are qualitatively unchanged under these alternatives.

National policy coding should also be understood as a reduced-form measure of exposure. In federal systems, where implementation can vary meaningfully below the national level, this may attenuate estimated effects toward zero rather than recover the exact intensity of subnational treatment.

4.6 Estimating the magnitude of policy effects on diffusion speed

Our event–study estimates relative effects on the diffusion *speed* of wind and solar. In early adoption, cumulative capacity $S_{i,t}$ is well approximated by the linearised logistic (Equation 2), so the first difference of $\log S_{i,t}$ approximates the diffusion speed k. Small shifts in k compound into large differences in deployment, so we report policy magnitudes in installed capacity (MW), i.e., how much $S_{i,t}$ moves under the policy.

We compute this change in three steps: (i) estimate an interaction-weighted *dose–response* variant of Equation 3 in which the event–time indicators are multiplied by the size of the underlying stringency change η_i, so that ν_ℓ is interpretable as the effect on k_{it} per one–unit increase in the policy stringency index (see Equation 18 for more details); (ii) map those aggregate coefficients into *slope shifts* $\Delta k_{i,\ell}$; (iii) construct the policy-on slope path

$$k_{i,t}^{\text{pol}} = \begin{cases} k_{0,i}, & \ell < 0 \text{ or } \ell > M, \\ k_{0,i} + \Delta k_{i,\ell}, & 0 \le \ell \le M, \end{cases} \tag{5}$$

where $k_{0,i}$ is the average pre-treatment slope. By design of the event window, durable effects cannot persist beyond M=5 years.

Two features limit the effects included in $\mathcal{G}$. First, the mapping is *gated*: a family contributes to the slope shift $\Delta k_{i,\ell}$ only if the estimated event-time coefficient points in the direction predicted by the policy change (a more stringent policy increases deployment), and is statistically distinguishable from zero using a one-sided test. Families that fail the pre-trend screen are excluded at all horizons. Thus, $\mathcal{G}$ captures only the subset of policy responses that pass our statistical screens, rather than the effect of the full policy portfolio. Second, we define the no-policy baseline $k_{0,i}$ as the country's average growth rate during the four pre-treatment years (Equation 22) and allow estimated slope shifts to operate for at most $M = 5$ years (the window length of our main specification). Consequently, the calculation omits any acceleration that persists beyond the event-study window.

We then define two counterfactual deployment paths,

$$\widehat{\log S}_{i,t}^{\text{pol}} = \widehat{\log S}_{i,t-1}^{\text{pol}} + k_{i,t}^{\text{pol}}, \qquad \widehat{\log S}_{i,t}^{\text{no}} = \widehat{\log S}_{i,t-1}^{\text{no}} + k_{0,i}, \tag{6}$$

anchored at the last pre-policy observation. Our primary magnitude statistic is the geometric mean capacity ratio at horizon C (spanning N_C years),

$$\mathcal{G}_i(C) = \exp\Bigl(\frac{1}{N_C} \sum_{t \le C} \ln \frac{\widehat{S}_{i,t}^{\text{pol}}}{\widehat{S}_{i,t}^{\text{no}}}\Bigr). \tag{7}$$

This exercise is illustrative rather than structural and rests on additivity of effects, stable non-policy drivers along the baseline, and no general-equilibrium feedbacks; we therefore view the results as short- to medium-run accounting rather than welfare evaluation. For more detail on the specific path construction and the metric discussion see Appendix C.2 and C.3.

5 Results

We organize the evidence around a hierarchy of estimands. Our preferred specification retains the first qualifying reform in each country×instrument×technology cell. This restriction limits contamination from earlier reforms in the same policy family and gives the clearest interpretation as the response to a major policy change. We then compare it with estimates using all qualifying reforms, which capture the average response around policy episodes that may include subsequent adjustments, and with a sample that excludes reforms occurring near another same-instrument reform in the treated country. The latter is a check for overlapping treatment windows, not a design that eliminates bundling across different policy instruments.

5.1 Average effects across policy instruments

The strongest and most consistent result concerns solar FIT prices. A positive reform increases annual solar-capacity growth by 0.327 log points in the preferred first-reform specification, compared with 0.129 using all reforms and 0.211 after excluding nearby same-family reforms. All three estimates are statistically significant. Their differences should not be interpreted as a decomposition of contamination because the sample restrictions change the treated population and event-time weights. Rather, the comparison shows that the positive solar response is not driven exclusively by repeated or closely spaced reforms.

FIT-duration reforms are also followed by higher solar deployment growth, with short-run estimates between 0.103 and 0.180 and medium-run estimates between 0.153 and 0.186. For wind, the response is delayed: short-run estimates are close to zero, whereas medium-run FIT-duration estimates are approximately 0.07–0.08. Wind FIT-price reforms provide weaker evidence, with a positive medium-run estimate only in the first-reform sample.

The remaining policy families do not produce stable average effects over the reported horizons. Planning generates an impact-year increase but no sustained average response; auction estimates are generally positive but imprecise; and coal-exit and carbon-pricing reforms show no robust acceleration in deployment. RD&D estimates are negative in some specifications but are not stable across the sample restrictions.

These averages conceal important differences in timing and pre-treatment behavior. The following section therefore examines the complete event-time paths and associated pre-trend diagnostics. That evidence supports a causal interpretation for solar FIT price and delayed wind FIT duration, while requiring greater caution for solar FIT duration and RD&D.

5.2 Dynamic effects across policy instruments

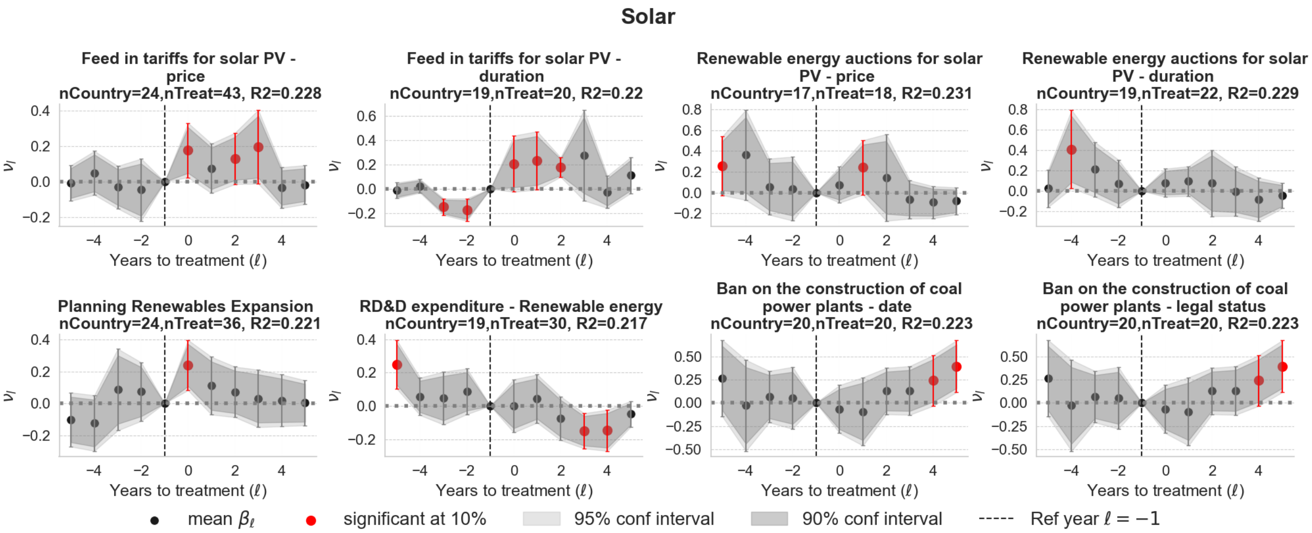
Figure 3 presents the dynamic effects of the principal positive policy reforms. The figure reports all eight policy families rather than selecting panels according to statistical significance. It reports interaction-weighted event-time coefficients $\hat{\nu}_\ell$ for the change in diffusion speed $k_{it} = \Delta \log S_{it}$, estimated following Sun and Abraham (2021). We normalise $\ell = -1$ to zero so coefficients read as deviations from the pre-treatment year. Errors are clustered at the country level. Throughout the main text, we emphasise families with sufficient observations and those that pass a joint Wald pre-trend screen in the leads (baseline window $\ell \in \{-5, -4, -3, -2\}$); families that fail are discussed descriptively with appropriate caveats and detailed appropriately in Appendix B.1.

Under our assumptions and design, *Feed-in-Tariffs* as a policy family yield clear near term accelerations

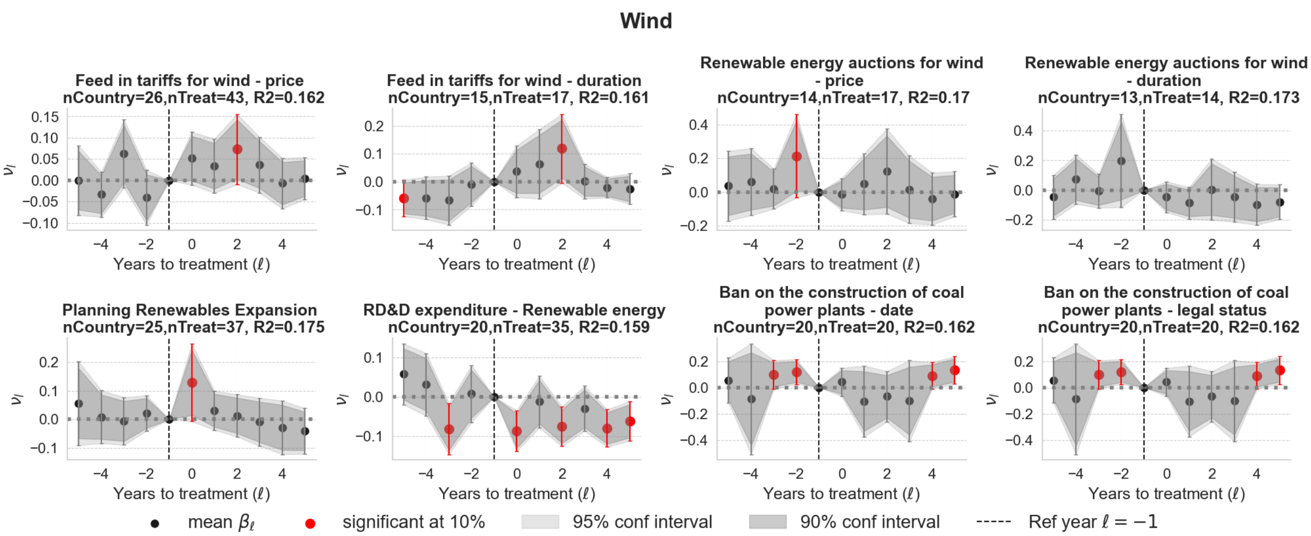
Table 1: Short- and medium-run effects of climate policy instruments on renewable capacity growth

Instrument	Main: Short-run ($\ell \in \{0,1,2,3\}$)	Main: Medium-run ($\ell \in \{2,3,4,5\}$)	First event: Short-run ($\ell \in \{0,1,2,3\}$)	First event: Medium-run ($\ell \in \{2,3,4,5\}$)	No same-family reforms (± 3): Short-run ($\ell \in \{0,1,2,3\}$)	No same-family reforms (± 3): Medium-run ($\ell \in \{2,3,4,5\}$)
Panel A: Solar						
FIT price	0.129** (0.052)	0.087 (0.057)	0.327*** (0.078)	0.325*** (0.093)	0.211*** (0.048)	0.158** (0.066)
FIT duration	0.180*** (0.046)	0.186*** (0.041)	0.103** (0.046)	0.153** (0.062)	0.180*** (0.046)	0.186*** (0.041)
Planning	-0.044 (0.067)	-0.136 (0.089)	0.027 (0.057)	-0.092 (0.065)	0.083* (0.049)	-0.003 (0.055)
Auctions	0.072 (0.075)	0.065 (0.104)	0.121 (0.090)	0.154 (0.126)	0.103 (0.086)	0.141 (0.116)
Coal exit	-0.158 (0.178)	-0.085 (0.197)	-0.171 (0.168)	-0.110 (0.195)	-0.401 (0.262)	-0.331 (0.266)
RD&D	-0.168 (0.102)	-0.186** (0.084)	-0.154 (0.097)	-0.185** (0.083)	-0.073 (0.097)	-0.107 (0.065)
Pricing (ETS / tax)	0.306 (0.254)	0.401 (0.363)	0.192 (0.286)	0.202 (0.278)	0.265 (0.316)	0.408 (0.437)
Panel B: Wind						
FIT price	0.007 (0.027)	0.034 (0.029)	0.015 (0.043)	0.088** (0.042)	0.010 (0.031)	0.043 (0.028)
FIT duration	0.008 (0.028)	0.079** (0.037)	-0.012 (0.029)	0.068* (0.036)	0.008 (0.028)	0.079** (0.037)
Planning	-0.019 (0.034)	-0.057 (0.038)	0.019 (0.035)	-0.047 (0.033)	0.031 (0.023)	-0.027 (0.027)
Auctions	0.032 (0.045)	0.049 (0.051)	0.051 (0.080)	0.084 (0.099)	0.008 (0.064)	0.011 (0.086)
Coal exit	-0.044 (0.095)	-0.071 (0.105)	-0.075 (0.093)	-0.123 (0.113)	-0.062 (0.149)	-0.120 (0.139)
RD&D	-0.002 (0.033)	-0.038* (0.022)	-0.014 (0.039)	-0.040 (0.025)	-0.008 (0.036)	-0.041 (0.025)
Pricing (ETS / tax)	-0.047 (0.043)	-0.057 (0.058)	-0.033 (0.063)	-0.037 (0.075)	0.014 (0.037)	-0.061* (0.035)

Notes: The outcome is Δ log installed capacity. Solar and wind are estimated separately. Each cell reports the equal-weight average $\bar{\nu} = |H|^{-1} \sum_{\ell \in H} \nu_\ell$ of stacked Sun–Abraham interaction-weighted event-time coefficients over the indicated horizon H; parentheses give country-clustered standard errors and stars correspond to the pointwise t-test on $\bar{\nu}$ ($p < 0.10$, $p < 0.05$, $p < 0.01$). The Main columns use all selected positive significant-not-sharp L4 policy shocks; the First event columns restrict treated events to the first observed shock per country×instrument×technology; the Isolated columns further restrict to focal events whose treated country has no other same-instrument event within ± 3 years of the focal year. All six columns use the $[-5, +5]$ stack window, lagged macro and non-focal-instrument policy controls, country, year, and event-stack fixed effects, binary event indicators, and treated countries with at least one pre-event and four post-event observations of the outcome; event time $\ell = -1$ is the omitted reference period. The pointwise null for wind FIT duration in the Main short-run column hides significant within-window dynamics: the joint Wald test for $H_0 : \nu_0 = \nu_1 = \nu_2 = \nu_3 = 0$ rejects at the 5% level (appendix table B.3).



(a) **Solar**



(b) **Wind**

Figure 3: Positive policy reforms and renewable deployment dynamics. Panels plot Sun–Abraham interaction-weighted event-time coefficients for the eight principal policy instruments, separately for solar and wind. The outcome is annual log installed-capacity growth, and coefficients are measured relative to event year $\ell = -1$. Gray bands denote 95% and 90% confidence intervals; red markers identify coefficients significant at the 10% level. Panel headers report the numbers of treated countries and reforms. The specification uses a $[-5, +5]$ event window, country and year fixed effects, lagged macroeconomic controls, and controls for non-focal policy changes. Standard errors are clustered by country.

in adoption speed for both solar and wind. An upgrade of the *price of Feed-in-Tariffs* for solar shows an on-impact step at $\ell = 0$ with positive short lags ($\ell = 2$–3). The pre-treatment coefficients are small and jointly insignificant ($p = 0.633$), supporting the parallel-trends assumption. The profile could indicate a rapid response to improved project revenues, followed by continued deployment as the existing project pipeline is completed. The estimates imply an increase in k of about 0.2 log-points at $\ell = 0$, corresponding to roughly 22% faster annual deployment relative to the pre-policy year. This estimate captures a local, on-impact change in the growth rate, not the cumulative effect of the policy over the diffusion path (which is computed in Section 5.6).

Solar FIT-duration reforms shows a strong post-reform increase as deployment growth rises between $\ell = 0$ and $\ell = 2$. However, these increases are preceded by substantial declines at $\ell = -3$ and $\ell = -2$, and the joint test rejects flat pre-trends. The resulting fall-and-rebound profile is consistent with anticipatory postponement: developers may delay commissioning when a longer support period is expected to become available. It may also reflect endogenous policy timing, however, and the two explanations cannot be distinguished using the event study alone. We therefore interpret the profile as suggestive evidence of anticipation and subsequent catch-up, rather than treating its post-reform coefficients as clean causal estimates.

FIT price and FIT duration show different pre-lead coefficients which is consistent with anticipation effects: in several regimes the tariff *price* can be locked early, but the *duration* is determined at commissioning, so developers have an incentive to delay completion until the longer tenor takes effect. The subsequent rise at $\ell \geq 1$ then aligns with a catch-up in completions once the new rule is active (see Section 6).

Wind responses are generally smaller and emerge later than solar responses. FIT-price reforms generate a positive coefficient around $\ell = 2$, but the surrounding estimates are imprecise and the all-reform multi-year averages are close to zero. FIT duration produces the most coherent wind profile. The pre-treatment coefficients are jointly insignificant ($p = 0.222$), while deployment growth rises most clearly around $\ell = 2$. This delayed response is reflected in the positive average over years 2–5, despite an average close to zero over years 0–3.

This points to a limited or heterogeneous response rather than a broad acceleration following FIT-price reforms. A plausible mechanism is that higher guaranteed prices raise expected project revenues and bring forward near-ready projects for an immediate lift, but new capacity appears only as projects clear the development pipeline.

Renewable-expansion planning generates a different dynamic response. Solar deployment growth rises sharply in the implementation year, but the effect declines thereafter and is close to zero by the later post-reform years. This transient profile is consistent with planning reforms accelerating the completion of projects already near approval, construction, or grid connection, rather than permanently increasing deployment growth. It also explains why the impact-year coefficient is positive while the multi-year averages in Table 1 are close to zero.

The evidence for solar *auctions* is weaker. Auction-price reforms are followed by a positive coefficient at $\ell = 1$, but the response is short-lived and is accompanied by movement in a distant lead. We detect no short-run effect for duration-related auction changes or for wind. The one-year lag is consistent with developers securing revenue certainty through auction awards and completing construction in the next annual build cycle.

We distinguish coal *bans* (prohibitions on new coal capacity, implemented either as explicit dated prohibitions or as changes in legal status) from *phase-outs* (which specify retirement schedules for existing plants, presented in Appendix B.1). In our main specification, bans are associated with positive but delayed effects on solar deployment, emerging at lags $\ell = 4$–5. The timing fits a shift towards solar once the prohibition is in place, with measurable responses only a few years later. For wind, post-event estimates are also positive but coincide with movement in the leads, so we do not make a causal claim. Phase-outs do not show any statistically significant acceleration (Figure B.1), plausibly because retirements are staged and adjustable, which weakens the commitment signal to developers.

Renewables RD&D shows near-zero impact at onset and weakly negative mid-lags, with a declining pre-trend in the leads. Because the pre-treatment dynamics are not flat, we treat these estimates as descriptive rather than causal. The observed negative pre-trend for wind and the small estimated effect may reflect endogenous policy timing: policymakers may increase RD&D support precisely when deployment is falling, which would obscure the underlying causal effect by counteracting further declines.

We present the remaining policy families in Appendix B.1. Emissions trading schemes (ETS) for electricity whether in price or in coverage, and renewable portfolio standards (RPS) do not show any detectable impact on diffusion.

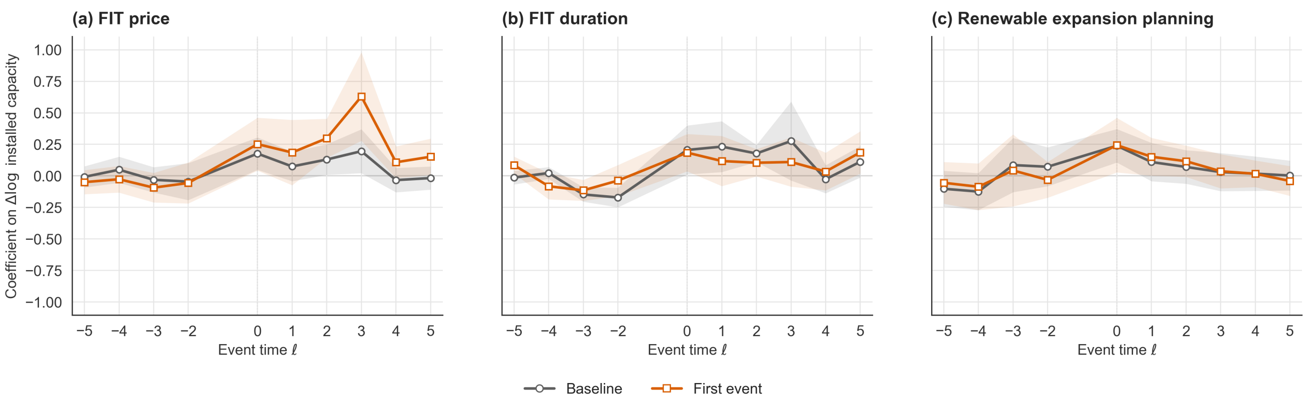
These full-sample estimates establish the broad dynamic policy ordering, but because reforms often arrive in sequences or bundles, they do not by themselves isolate the effect of a single reform. We therefore next turn to stricter treatment definitions that better separate initial rollout effects from repeated within-country policy reforms.

5.3 First and non-overlapping reforms

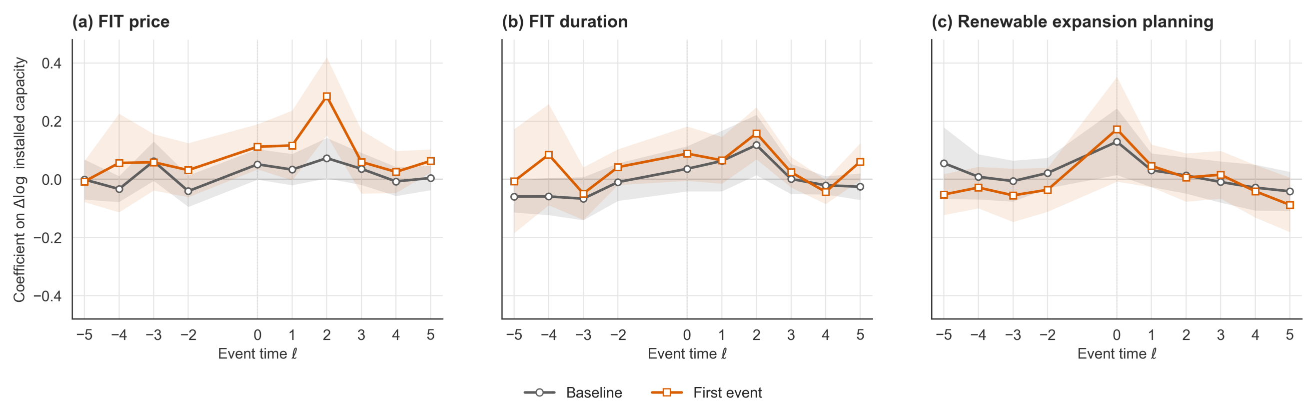
The full event studies estimate the average response around major policy episodes, including subsequent revisions to an existing instrument. Here, we restrict the treated sample to the first qualifying reform in each country×instrument×technology cell. This changes the estimand: it emphasizes initial major reforms and removes contamination from prior same-family events, but it does not necessarily identify a policy enacted in isolation from the rest of the policy portfolio.

Figure 4 shows that the main FIT timing patterns are not created by repeated reforms. For solar FIT price, the first-reform response is larger than the all-reform response but has the same post-reform direction. For wind FIT duration, both samples show a delayed increase around $\ell = 2$. Solar FIT duration retains its pre-reform decline, so the first-reform restriction does not resolve the anticipation or endogenous-timing concern. Planning retains an impact-year increase, but the effect remains transient and does not produce a stable medium-run average.

The final columns of Table 1 provide a complementary check by excluding reforms occurring within three years of another policy in the treated country. Solar FIT price remains positive, as does the medium-run wind FIT-duration estimate. We interpret this as evidence that these findings are not mechanically generated by overlapping same-family event windows. We do not interpret differences between the columns as a decomposition of contamination, because each restriction changes the treated population and event-time weights.

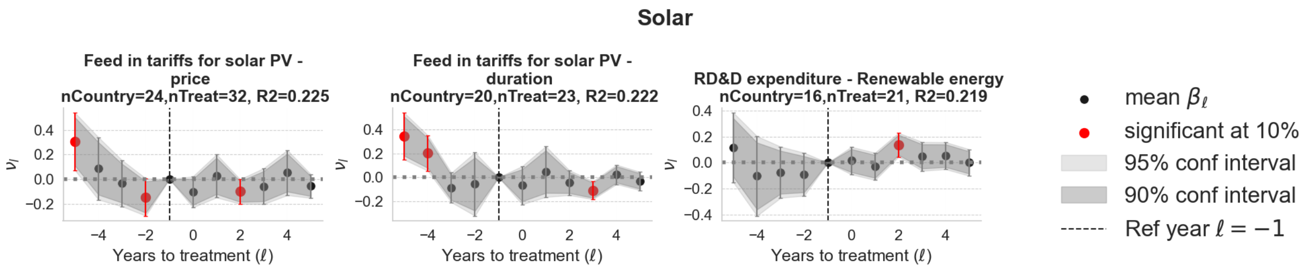


(a) Solar

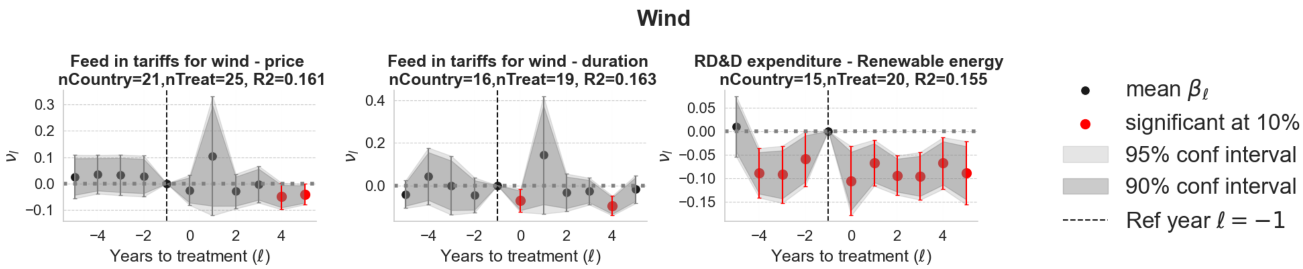


(b) Wind

Figure 4: All reforms versus first reforms for FIT price, FIT duration, and renewable-expansion planning. Each panel overlays the event-time path estimated from all qualifying reforms with the path estimated using only the first qualifying reform in each country, instrument, and technology. Coefficients are measured relative to $\ell = -1$, and shaded bands report 90% confidence intervals.



(a) **solar (negative events)**



(b) **wind (negative events)**

Figure 5: Policy weakenings and diffusion dynamics. Sun–Abraham interaction-weighted coefficients for negative policy shocks ($\eta < 0$). Convention as in Figure 4.

5.4 Negative shocks as a validation exercise

We use policy downgrades (e.g., FiT cuts) as a sign-reversed falsification: if the positive-event profiles reflected background trends or selection rather than policy, downgrades should not produce systematic slowdowns. However, the cuts in policy follow earlier increases in deployment, consistent with governments reducing support after a boom.

We focus on the two families with sufficient of these policy downgrades: Feed-in-Tariffs and renewables RD&D. Other policies either lack a clear "negative" counterpart or have too few events for inference.

Cuts to Feed-in-Tariffs (price) are followed by a slowdown in the diffusion of solar ($\ell = 2$). Reductions in Feed-in-Tariffs (duration) show a similar contraction at $\ell = 3$. Positive leads at $\ell \in [-5, -4]$ for both policies are the tail of a larger pre-run-up visible when widening the window to $[-7, +7]$ (Appendix B.5.3, Figure B.11). This is consistent with feedback from deployment to policy, where support is reduced after a surge in diffusion. Feed-in-Tariffs cuts exhibit a durable slowdown in wind deployment over $\ell = 4$–5 (price reduction) continuing to $\ell = 7$ (Figure B.11) and $\ell = 0$ and 4 (duration reduction). There is no detectable pre-trend. The stronger short-term effect of duration vs. price may reflect the project stage at which each can be locked (see Section 6). Renewables RD&D retrenchments do not translate into short-run deployment changes: impact estimates are close to zero with weakly negative mid-lags and some movement in the leads. We read this as policy reactivity and long-horizon innovation effects, not as an immediate deployment channel.

Taken together, where coverage allows, negative shocks behave as the mirror image of the baseline profiles: tariff cuts reduce near-term diffusion, while RD&D cuts show no immediate impact. If the positive-event findings reflected only background trends, we would not expect these sign reversals. This is unlikely to be mechanical mean reversion: the slowdown appears at the lags where the policy channel operates ($\ell = 2$–5), not on impact at $\ell = 0$ as a reversion to trend would predict. The negative-event patterns, therefore reinforce the interpretation that the tariff results, in particular, capture policy-driven changes in adoption speed.

5.5 Robustness and identification checks

Our estimates are average dynamic responses to national policy reforms, measured relative to contemporaneous never- and not-yet-treated countries, conditional on global trends, lagged covariates, and observed non-focal policies (Equation 3). Three threats are specific to cross-country policy evaluation: reforms arrive in bundles, treatments can spill across borders, and national coding may mismeasure subnational implementation. We address each in turn, then summarise the standard checks on pre-trends, leverage, sample composition, and specification. Every exercise perturbs one element of the design while holding event definitions, fixed effects, covariates, and windows fixed, so that any movement in the estimates can be attributed to the perturbation being tested.

Policy bundling. Energy policies are frequently enacted in packages; the EU's 2007 climate-and-energy package, for example, combined renewable mandates, emissions targets, and efficiency measures. Two features of the design limit the resulting confounding. Year fixed effects absorb common shocks, including EU-wide directives and shared budget cycles, and the baseline specification conditions on lagged stringency changes in all non-focal policy families (Section 4.5), so each coefficient is a marginal effect conditional on observed policy co-movement. We then probe bundling directly with progressively stricter designs. First, the first-reform estimates in Section 5.3 retain only the first qualifying reform in each country–instrument–technology cell; first adoptions are less likely to be bundled with subsequent revisions, and they reproduce—indeed strengthen—the solar FIT-price and wind FIT-duration responses (Table 1). Second, whereas the isolated column of Table 1 excludes nearby reforms of the *same* family, Table 2 imposes *cross-family* isolation: the treated country must experience no other reform from any analysed policy family within ± 1 or ± 2 years of the focal event. Under the ± 1-year filter the short-run solar FIT-price average remains positive though imprecise (0.07 across eight events), and planning and wind FIT-price responses remain positive and significant. The ± 2-year filter leaves at most a handful of eligible events per family; we report it as a demanding stress test and do not draw magnitudes from cells resting on three to five events. Third, Appendix B.7.4 pushes isolation to its limit by retaining only countries with a single qualifying event in a single family over the estimation window; the sign and ordering of the FIT responses survive, at a substantial cost in precision (Table B.22–Table B.23). Finally, the dynamic paths are essentially unchanged when the non-focal policy controls are removed altogether (Appendix B.7.2) or replaced with alternative constructions—event sizes, active-event indicators, or cumulative counts (Appendix B.7.3, Figure B.15)—so the results are not artefacts of how the policy portfolio is partialled out. These checks rule out mechanical confounding from observed policy bundles; they cannot rule out unmeasured or slow-moving complementary reforms, and we accordingly interpret the coefficients as the effect of reforms enacted within typical policy portfolios rather than as single-lever treatment effects.

Table 2: Robustness to cross-family event isolation

	No other reform within ±1 year		No other reform within ±2 years	
Instrument	Short run $\ell = 0$–3	Medium run $\ell = 2$–5	Short run $\ell = 0$–3	Medium run $\ell = 2$–5
Panel A: Solar				
FIT price	0.069 [$N = 8$]	0.015 [$N = 8$]	−0.079 [$N = 5$]	−0.014 [$N = 5$]
Planning	0.169** [$N = 9$]	−0.040 [$N = 9$]	–	–
Auctions	0.085 [$N = 8$]	0.258 [$N = 8$]	0.553*** [$N = 3$]	0.652*** [$N = 3$]
RD&D	0.020 [$N = 6$]	−0.044 [$N = 6$]	0.044 [$N = 4$]	−0.002 [$N = 4$]
Pricing (ETS/tax)	0.879 [$N = 46$]	1.135 [$N = 46$]	−0.214 [$N = 4$]	0.014 [$N = 4$]
Panel B: Wind				
FIT price	0.412** [$N = 6$]	0.352 [$N = 6$]	1.091** [$N = 3$]	1.074** [$N = 3$]
Planning	0.189*** [$N = 10$]	0.057 [$N = 10$]	–	–
Auctions	−0.245*** [$N = 4$]	−0.242*** [$N = 4$]	–	–
RD&D	−0.009 [$N = 6$]	−0.003 [$N = 6$]	0.109* [$N = 4$]	0.144* [$N = 4$]
Pricing (ETS/tax)	0.056 [$N = 48$]	0.016 [$N = 48$]	−0.068 [$N = 3$]	−0.177 [$N = 3$]

Notes: The outcome is Δ log installed capacity. Each cell reports the equal-weight average of the Sun–Abraham event-time coefficients over the indicated horizon and the number of eligible focal events in brackets. The specification otherwise matches the baseline. Isolation requires that the treated country experience no other reform from any analysed policy family within $\pm w$ years of the focal event. Cells with fewer than three eligible events are suppressed. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Cross-country spillovers and SUTVA. Cross-country interference can arise through policy emulation, demonstration effects, shared developers and supply chains, and integrated electricity markets. Year fixed effects absorb common international shocks but not geographically localised or peer-country spillovers, so we condition on the three observable channels one at a time. First, we add the average lagged deployment of the focal technology among each country's K nearest neighbours, for $K \in \{2, 5, 10\}$, capturing deployment spillovers through shared markets and demonstration effects (Appendix B.7.9, Figure B.17). Second, we add the average stance of the focal policy family among the same neighbour sets, capturing policy emulation and coordinated reform timing (Appendix B.7.10, Figure B.18). Third, we condition on lagged net energy imports as a measure of exposure to cross-border energy markets (Appendix B.7.7, Table B.26–Table B.27). The event-time paths under $K = 5$ and $K = 10$ are nearly indistinguishable from the baseline; the most local specification, $K = 2$, moves a few short-run solar FIT coefficients without changing their sign, timing, or interpretation, and the wind paths are essentially unchanged throughout. Conditioning on net energy imports leaves the coefficients unchanged up to rounding. The remaining channel is technology cost: aggressive deployment in one country can lower global equipment costs for all. This spillover, however, benefits treated and comparison countries alike and therefore attenuates rather than inflates the estimated domestic response; over our sample, moreover, module-cost declines are dominated by Chinese manufacturing scale, which year fixed effects absorb as a common shock (Section 4). Consistent with attenuation through a mediating channel, adding technology cost as a covariate compresses magnitudes without altering timing (Appendix B.7.5, Table B.24–Table B.25), and we read the cost-controlled estimates as conservative lower bounds. These exercises do not establish strict SUTVA: they condition on observed spillover proxies and cannot exclude unobserved international interference. The estimand should therefore be read as the acceleration associated with a national reform conditional on the prevailing global technology, cost, and market environment—which is the quantity relevant to a policymaker deciding whether to adopt, taking that environment as given.

Subnational policies and federal systems. In federal countries, support for renewables is partly enacted at the state or provincial level, so a national stringency index measures exposure with error. To the extent this mismeasurement is uncorrelated with deployment shocks, it attenuates estimated effects toward zero, making our national-level estimates conservative for federal systems rather than spurious. Two checks support this reading. Interacting every event-time term with a federal-system indicator leaves the estimated paths essentially unchanged for both technologies, with no sign reversals and no reordering of policy families (Appendix B.7.8, Table B.28–Table B.29). And the leave-one-country-out diagnostics below confirm that no result depends on the large federal countries (e.g. Canada, Germany) or on any other single country.

Pre-treatment dynamics and endogenous timing. Governments may reform in response to deployment trends, and developers may anticipate reforms. We apply a joint Wald test on the pre-treatment leads to every policy family and use the outcome to discipline interpretation: solar FIT price ($p = 0.633$) and wind FIT duration ($p = 0.222$) pass the baseline screen, whereas solar FIT duration, RD&D, and several coal and certificate measures do not. The failing families remain in the eight-panel figures for transparency, but we interpret their post-reform coefficients as dynamic associations or anticipation effects rather than clean causal estimates. The conclusions are unchanged under narrower pre-windows, longer leads, and per-lead inspection (Appendix B.3, Table B.4–Table B.5 and Table B.6–Table B.7). The negative-shock analysis in Section 5.4 complements these screens: FIT cuts produce mirror-image slowdowns, which background trends or selection alone would not generate.

Outliers, leverage, and sample composition. Single influential countries or extreme annual capacity additions could mechanically drive signs or significance. Leave-one-country-out re-estimation of the full event-time path shows that no single-country deletion flips signs or creates or removes country-clustered significance for the FIT, planning, auction, or coal-ban families (Appendix B.6.1, Table B.14–Table B.15, Figure B.12–Figure B.13). Winsorising the outcome at the 1/99 and 2.5/97.5 percentiles preserves the post-treatment timing and rank ordering of effects (Appendix B.6.2, Table B.16–Table B.19). The results are likewise stable across $[-3, +3]$ and $[-7, +7]$ event windows and in a sample restricted to complete $[-5, +5]$ event-time coverage (Appendix B.5; raw-data panels in Figure B.6–Figure B.7, full-window overlay in Figure B.8, window variants in Figure B.9–Figure B.10). Replacing the outcome with smoothed capacity growth or electricity generation preserves direction and timing with modest attenuation (Appendix B.4), and restricting to later stages of adoption attenuates the solar responses, as the S-curve framing predicts (Appendix B.5.4).

Event definitions, covariates, and estimator. Reforms may be gradual or imperfectly dated, so we re-map events using sharp one-year changes, relaxed any-change thresholds, and dose-weighted designs in which event-time indicators are scaled by reform size (Appendix B.8; Table B.31–Table B.32, Table B.35–Table B.36, Table B.33–Table B.34). The qualitative dynamics are unchanged, and the FIT families display an approximately linear short-horizon dose–response, whereas certificates and carbon pricing do not. Blockwise removal of the macroeconomic covariates leaves the paths close to baseline (Appendix B.7.6, Figure B.16). Finally, a conventional two-way fixed-effects event study using the never-treated risk set reproduces the FIT and planning profiles with the expected attenuation and slightly noisier leads, indicating that the findings are not artefacts of the Sun–Abraham cohort weighting (Appendix B.9, Figure B.22).

Summary. Across these designs the central ordering is stable: solar FIT-price reforms produce the clearest short-run acceleration, wind FIT-duration reforms a smaller delayed response, planning a transient impact-year effect, and the remaining families are imprecise or affected by pre-treatment dynamics. Where a check weakens a result—strict cross-family isolation, later-stage samples—the loss is in precision, not in sign or timing. The limitations that remain are those generic to cross-country evaluation: unmeasured complementary reforms, unobserved international spillovers, and national-level policy coding. The first we bound with the isolation and first-reform designs; the latter two, where their direction can be signed, bias the estimates toward zero rather than away from it.

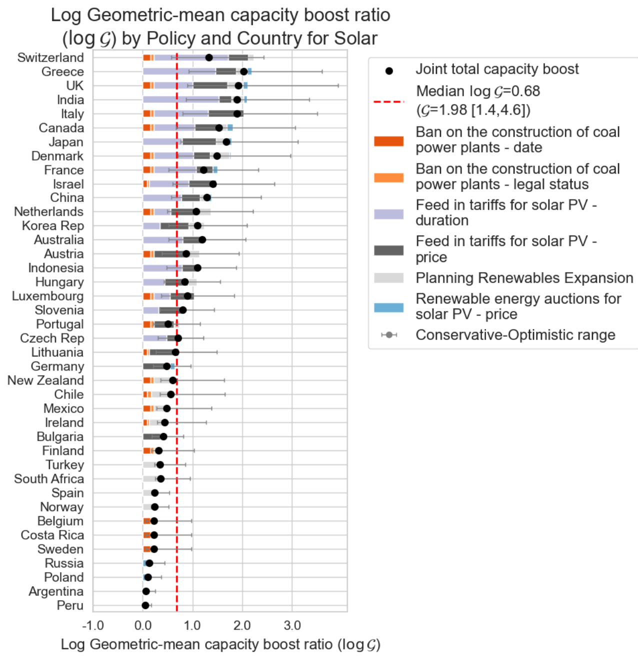
5.6 Magnitude mapping and counterfactual capacity paths

The event-study results show that policies shift diffusion speeds $k_{i,t}$. In this section, we translate those shifts into changes in installed capacity using the *dose–response* specification (see Appendix 15). We summarise the medium-run impact of each policy using the geometric-mean capacity boost $\mathcal{G}$, which captures how much faster cumulative deployment grows under the observed policy path than under a constant-slope baseline. In parallel, we construct a time-equivalent measure of impact by estimating how many years earlier the policy path reaches the 2023 capacity level than the no-policy counterfactual (Appendix C.2.2).

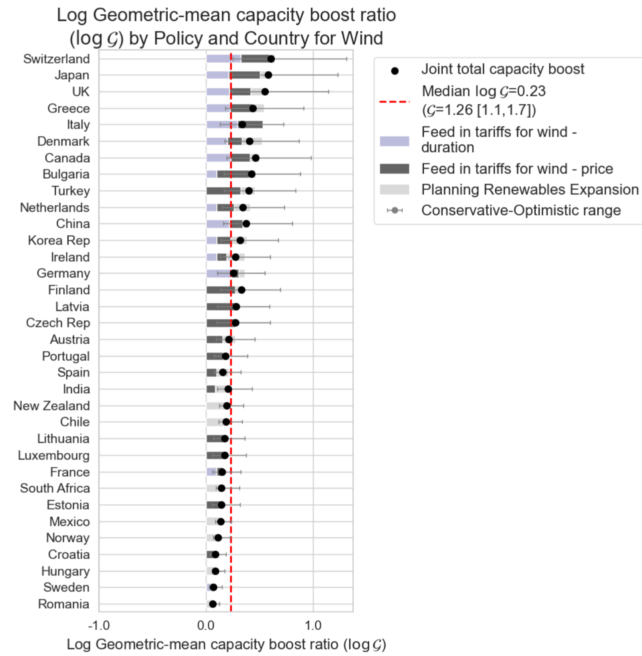
Figure 6 summarizes how policies accelerate diffusion across countries. Solar boosts are more dispersed: many countries show modest effects ($\mathcal{G} \leq 2$) while a subset cluster around $\mathcal{G} \approx 6$. This wide spread likely reflects heterogeneity in both policy design and the set of instruments that pass the identification step. Wind boosts are more tightly clustered and roughly log-normal/skewed, with fewer extremes. In median terms, the solar boost ($\mathcal{G} \approx 2$) is greater than the median wind boost ($\mathcal{G} \approx 1.3$), which means the median deployment is, on average, 100% for solar and 30% for wind higher over the period than it would have been had there been no policy.[11]

Solar results are dominated by FIT price and duration, with renewable expansion planning contributing to a lesser extent. Countries without a salient FIT price/duration episode rarely exhibit a pronounced acceleration in solar adoption speed - consistent with the earlier event study findings. For wind, FITs remain important, but contributions are more diversified: Renewable expansion plans often rival or exceed FIT price in several cases, and auction contract duration contributes in some settings. Coal phase-out status/date and auction duration are statistically significant in the event study yet translate into comparatively smaller geometric-mean boosts for solar.

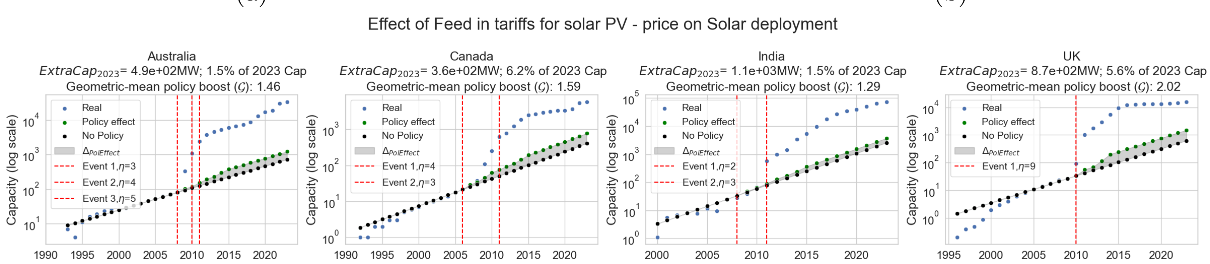
[11] The difference in policy sensitivity between solar and wind is corroborated by the median $\mathcal{G}$ per policy event being higher for solar = 0.51 than wind = 0.36; we show that this difference is statistically significant. Kruskal–Wallis test gives a p-value = 0.0035^{***} between the distributions,meaning that the samples cannot originate from the same distribution (Kruskal and Wallis 1952; Corder and Foreman 2009).



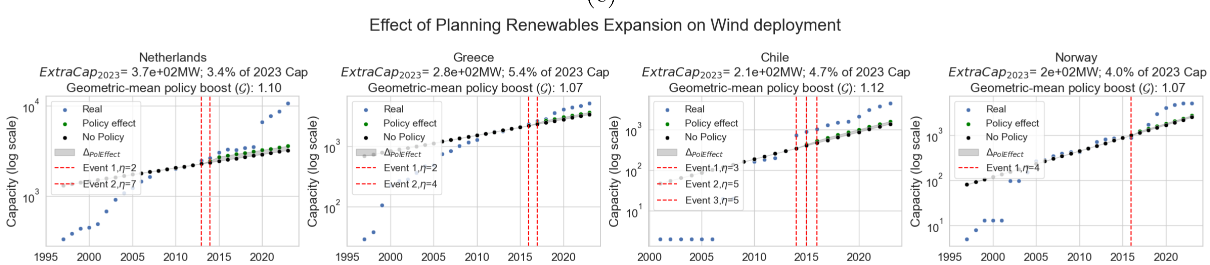
(a)



(b)



(c)



(d)

Figure 6: Policy-induced boosts to diffusion by country. Panels (a) solar and (b) wind show stacked bars of the *log* geometric-mean boost contributed by each policy within each country. We plot logs so that $\log \mathcal{G}_{\text{Total}} \approx \sum_p \log \mathcal{G}_p$ when $\mathcal{G}_{\text{Total}} \approx \prod_p \mathcal{G}_p$. Black dots indicate the joint (combined) capacity boost obtained by summing the log differences between each policy's counterfactual and its policy path; vertical error bars denote one-sided lower and upper bounds (conservative and optimistic estimates that use avg $\pm\sigma$ coefficients - see Section C.2 for details on the construction of the cases). The red horizontal line marks the log median total boost; the legend reports the point, optimistic, and conservative total-boost values. See Appendix C.3, Equation 39 for computation details. Panels (c)–(d) present example time series comparing the counterfactual, the policy path, and observed adoption for (c) solar FIT and (d) Renewable expansion planning across four countries.

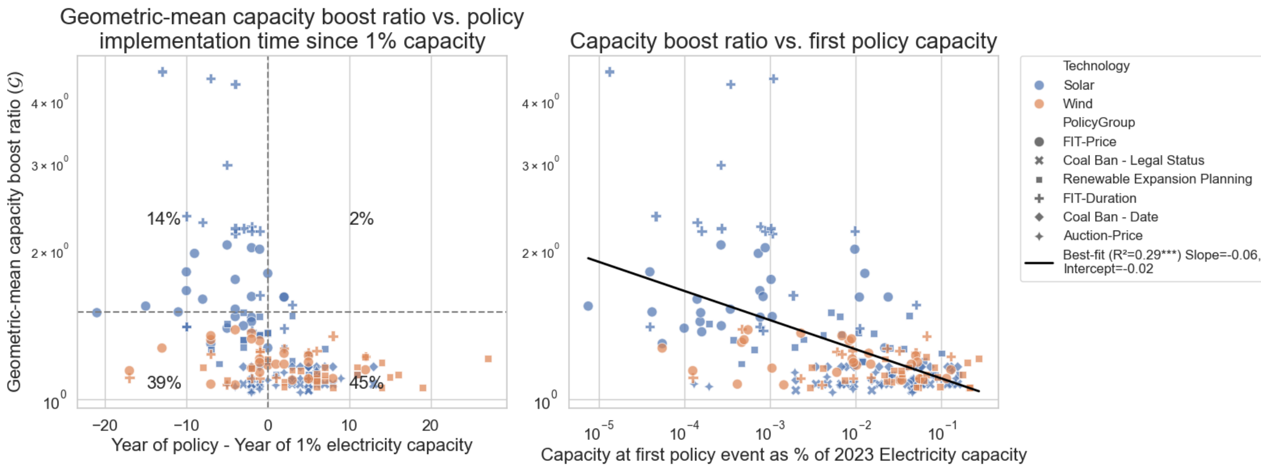


Figure 7: Boost to adoption vs time and early capacity across policies and technologies. Left: $\mathcal{G}$ vs the time of the first policy since the year of reaching 1% electricity capacity for the given technology in each country. A vertical line at 0 and a horizontal at 1.5 ($\approx$50% annual average boost) highlight our chosen critical thresholds. Right: log-log relationship between $\mathcal{G}$ and the capacity at the first policy event for the implemented policy in each country. A line of best fit between this log-log relationship (combined by technology-policy) is shown with the corresponding goodness of fit score and statistical significance. Each plot distinguishes technologies (blue-solar, orange-wind) and policy types (by marker type e.g. circle = FIT-Price).

Policy responsiveness varies markedly by country. Greece, the UK, and Japan fall in the top quartile for both solar and wind. Italy and India respond strongly to FITs. Several countries-such as Argentina, Russia, and Peru in solar-show limited acceleration, consistent with the absence of significant FIT activity in our policy event construction. The UK exhibits a broad and coordinated policy mix (all six statistically significant solar policies), yielding a top-three total solar boost ($\mathcal{G} = 6.8$). As a general pattern, countries with a thinner policy portfolio achieve substantially weaker net boosts.

For wind, Switzerland stands out with a strong overall boost ($\mathcal{G} = 1.8$), driven predominantly by FITs.[12] By contrast, Romania, Sweden, and Hungary benefit little from policy in wind. Croatia shows only a small FIT effect, in part because FIT implementation occurred late in the sample.

Finally, the correlation patterns qualify the magnitude estimates. The rank correlation between solar and wind boosts is moderate (Spearman $\rho = 0.57$), indicating that countries which design effective policy packages for one technology are more likely, but not guaranteed, to do so for the other. By contrast, the association between policy boosts and 2023 penetration (see Figure 2a) is weak (Spearman $\rho = 0.37$ for solar and $\rho = -0.11$ for wind), suggesting that high renewable shares arise from the interaction of policy with baseline diffusion speeds and structural country characteristics. In this interpretation, policy packages primarily shift countries forward along their underlying diffusion paths – sometimes by several years, sometimes only marginally (see Appendix C.2.2) – rather than determining long-run penetration levels on their own.

5.7 Earlier policy implementation is associated with larger capacity boosts ($\mathcal{G}$)

Cross-country variation in $\mathcal{G}$ reflects not only policy design and intensity but also the stage of diffusion at which support is introduced. We therefore examine how capacity boosts vary with the timing of the first major policy episode and with the level of deployment already in place when it occurs.

Figure 7 relates the geometric-mean capacity boost ($\mathcal{G}$) from positive policy events to (i) when the first policy occurs in the diffusion timeline (left) and (ii) the technology's capacity share at that first policy (right; log–log). The quadrant view in panel (left) makes the asymmetry visually clear: most observations are low-boost/later (45%), then low-boost/early (39%) and high-boost/early (14%); high-boost/later events are rare (about 2%). The log–log regression

$$\log_{10} \mathcal{G}_i = \alpha + \beta \log_{10} \left(\text{Cap. at first policy}\right)_i + \varepsilon_i$$

yields a clear negative elasticity in the pooled data ($\hat{\beta} \approx -0.06^{***},\ N = 190$): moving the first substantial policy ten-fold later in the diffusion path is associated with a smaller average boost (about 13% lower; $10^{-0.06} \approx 0.87$). This pattern hints at diminishing returns to "late" intervention.

Disaggregation indicates that this timing gradient is strongest for solar-especially for FIT-style support-while it is much weaker for wind and for many non-FIT instruments (see Section C.4). In short: early, well-designed solar support tends to move the needle; strong late surges are uncommon.

These patterns are descriptive rather than causal: the timing variable bundles cross-country differences in capacity, institutions, and concurrent policies, and $\mathcal{G}$ is only defined for events that raised adoption, so late nonevents are not shown. Even so, the concentration of high-boost observations in early, low-capacity quadrants, together with the negative pooled elasticity, points to a clear descriptive message: across technologies, large proportional boosts are most often associated with early support in thin systems, while strong late surges are rare.

6 Discussion

6.1 Policy design and timing vs. policy presence

Our findings echo and sharpen evidence that *design* matters more than simple policy presence. Work on FITs shows that tariff level and, crucially, contract duration and degression explain much of the cross-country variation in realised build: long, predictable FITs mobilise substantially more investment than short, frequently revised schemes, consistent with the bankability channel emphasised in this literature (Jenner et al. 2013; Smith and Urpelainen 2014; Dijkgraaf et al. 2018). This matches our result that *FIT–price* changes with higher effective remuneration give an immediate boost to k, while *FIT–duration* reforms have smaller impact effects but a delayed lift. Planning reforms in our data also raise k on impact, in line with evidence that clearer procurement and delivery rules reduce delays and increase realisation rates (Winkler et al. 2018).

Comparing instrument families, we find that subsidy-type tools (FIT) clearly outperform trading schemes (ETS, carbon price) on deployment, which is consistent with studies that find strong emissions effects from

[12] For Switzerland–solar, large individual boosts do not sum to a commensurately large joint boost; the joint effect is more modest, suggesting FIT duration dominated while other policies coincided temporally without adding much incremental impact.

carbon pricing via coal-to-gas switching but do not document large induced build of new renewables on their own (Leroutier 2022; Dechezleprêtre et al. 2023).

Timing along the diffusion path adds a dimension that is less explicit in the existing policy–adoption studies. System studies document that, as variable renewables grow, falling captured prices and rising integration costs shift the main constraint from finance to grid and flexibility (Hirth 2013). Our descriptive timing evidence suggests that proportional boosts are larger when major policies arrive earlier in the diffusion path (lower initial capacity) and smaller when introduced later. It is consistent with the theory that early in diffusion, instruments that stabilise revenues and unblock procedures move the relevant margin; later, similar price instruments do less unless paired with grid build-out and permitting reform (Figure 6). For policy transfer this implies a simple rule: diagnose whether finance, administration, or infrastructure is currently binding, and match the instrument to that constraint rather than copying schemes that worked under different system conditions.

6.2 Transient versus permanent effects

The break analysis shows that national diffusion often departs from a smooth log-linear path, even during the early phase of adoption. These breaks are descriptive: they establish that diffusion speeds change, but neither attribute those changes to policy nor identify their duration (Fact 3, section 2.3). The event-time paths give a clearer picture. We see k rises around the reform time and then reverts back toward its pre-policy trend (Figure 4). Thus, the estimated acceleration is temporary. However, its effect on installed capacity is not: integrating the higher growth rate over time shifts the log-capacity path upward, leaving capacity permanently higher even after growth returns to its previous rate (Figure 6).

Our results are therefore consistent with a simple interpretation. Deployment policies mainly re-time when projects reach completion rather than permanently steepening national diffusion trajectories. The longer-run shape of diffusion appears to be governed by slower-moving forces such as technological learning, supply chains, financing conditions, and grid integration. For forecasting, this distinction matters. Policy shocks should be treated as temporary boosts to k unless there is an explicit sequence of follow-on actions that rebuilds the pipeline or relaxes new constraints. Ignoring this and treating every observed acceleration as a permanent slope change will tend to push medium-run deployment projections upward relative to what the data can support.

6.3 Immediate vs. delayed channels and technology heterogeneity

Our results point to clear technology heterogeneity. Solar responds more strongly to policy than wind (Figure 4 – sensitivity to more policy families, Figure 6 – larger effect sizes): cash-flow and planning shocks translate into faster solar commissioning, while wind reacts more weakly and with longer lags. This is consistent with the simple engineering difference between modular, relatively quick-to-build solar and more site-specific, logistics-intensive wind (Wilson et al. 2020).

The dynamic profiles also clarify immediate versus delayed channels in a way that aligns with, and extends, earlier work on design details and bankability. FIT studies find that higher and more predictable compensation increases capacity additions (Jenner et al. 2013; Smith and Urpelainen 2014; Dijkgraaf et al. 2018); Polzin et al. (2019), Steffen (2018), and Alcorta et al. (2024) emphasise that guarantees which stabilise revenues improve bankability and access to debt. In our setting, these mechanisms show up as *immediate* jumps in k for FIT–price and planning reforms, which relax revenue risk and procedural bottlenecks for near-ready projects, and as *delayed* but more persistent lifts for FIT–duration, which improves financing terms as new projects are structured. The slower response to coal bans fits this delayed pattern: most bans arrive late in the sample and operate mainly through expectations, so in systems that already have some renewables in place it takes several years for procurement, permitting, and grid reinforcement to convert the shock into observed solar build. We are not aware of prior work that traces this delayed deployment effect of coal bans at national level; this appears to be a novel contribution.

Finally, the fact that event shapes in capacity (MW) largely carry over to production (GWh), with some attenuation (Section B.4), complements system studies that document how integration issues (curtailment, congestion, variability) erode the value of variable renewables at higher shares (Hirth 2013). Our results show that policy-induced accelerations are not just changes on paper: they translate into additional output, but gaps between capacity and production responses are informative about where integration frictions have started to bind.

FIT–Duration for solar is an interesting case-study of anticipation For solar FIT–duration reforms we observe negative coefficients at $\ell \in \{-3, -2\}$, followed by a short-lag rise in k after implementation. This is consistent with *anticipatory deferral*: preliminary accreditation typically fixes the *price* but not the *duration*, so developers delay commissioning (or conversion to full accreditation) until the longer tenor is in force.[13] Two features support this interpretation and a causal reading: (i) the lead motion is localised to $\ell = -3, -2$ and reverses at impact, which is hard to reconcile with a drifting confounder; and (ii) the post path builds over $\ell \in \{+1, +2\}$, as expected if longer contracts improve leverage and reduce refinancing risk as cohorts roll through financial close. Taken together, these patterns indicate that *FIT–Duration* accelerates diffusion speed k for solar, conditional on anticipatory behaviour around the reform.

6.4 Sequencing and bundles

We do not estimate policy interactions directly, but two features of the results point to sequencing and bundling. First, the largest cumulative boosts in k occur in countries that implement several strong deployment policies over time (FIT reforms, auctions, planning), rather than relying on a single reform (e.g. UK/Italy Figure 6). Second, coal bans (policies implemented much later) only show delayed solar gains, where later procurement and grid policies are present (see Figure B.19 and Figure 4). Both patterns are consistent with policies acting as complements rather than substitutes.

This fits qualitatively with work on policy pathways and mixes. Eskander et al. (2021) show that climate legislation tends to arrive in waves rather than as one-off laws, and Mealy et al. (2025) argue that countries expand their policy portfolios along a feasibility frontier rather than by adding isolated instruments. Grubb et al. (2024) emphasise that effective policy packages evolve with the stage of the transition. Our contribution is not to test these theories directly, but to show that even after controlling for other policies in the regression design, there remains structure in how sequences of "anchor" measures (such as bans) and

[13] Institutional rules support this mechanism. In Great Britain, preliminary accreditation can fix the tariff level by application date, but the support duration is set only at full accreditation/commissioning (Ofgem 2024, §§5.8–5.9, §5.18). In Germany, EEG remuneration runs for 20 years from commissioning and the applicable rate is determined by the commissioning date (Clearingstelle EEG 2022; BNetzA 2025). In Italy's *Conto Energia* I–II, incentives are granted for 20 years from entry into operation (DM 28/07/2005; DM 19/02/2007). In Spain, RD 661/2007 and RD 1578/2008 tie regime assignment and remuneration to registration/operation timing via the pre-allocation registry (RD 661/2007; RD 1578/2008).

“enabling” measures (FITs, auctions, planning, grid reforms) line up with observed accelerations in diffusion. Quantifying optimal sequencing and bundles is beyond the scope of this paper.

Our estimates capture within-country policy effects conditional on: (i) contemporaneous policies in other families (addressing bundling), (ii) global technology cost trends (addressing learning spillovers), and (iii) electricity market integration (addressing trade spillovers).

This is the policy-relevant estimand: policymakers want to know whether adopting a given policy will accelerate deployment, taking as given the broader technological and market environment. Our identification strategy provides credible estimates of these conditional effects while acknowledging that pure SUTVA (independent units) is violated—as it is in most cross-country and cross-state policy evaluations.

7 Conclusions

We quantify how electricity-sector policies shape national solar and wind deployment in OECD+ countries from 1990 to 2023. Using design-based policy stringency from the OECD CAPMF and Sun–Abraham event studies, we estimate dynamic effects of policy strengthenings on diffusion speed and map these into counterfactual capacity paths using an S-curve baseline.

Three results stand out. *First, design and timing dominate simple policy presence.* Strengthenings of FIT price and duration and renewable planning expansions are associated with faster deployment, with sharper and more precisely estimated effects for solar than wind. Coal exit measures show delayed solar gains, while renewables RD&D has no clear short-run deployment effect in our data. By contrast, carbon pricing instruments, ETS reforms, and certificate/portfolio schemes do not generate robust increases in diffusion speed.

Second, speed effects are temporary but accumulate into persistent level differences. Event-time profiles typically rise around reforms and then drift back toward baseline within a few years, consistent with policies accelerating completion of near-ready projects rather than permanently steepening diffusion. Nevertheless, compounding over the diffusion baseline implies large level impacts: in an illustrative accounting exercise and relative to a no-policy counterfactual, we show that the observed policy bundle roughly doubles solar capacity and raises wind capacity by about 30% at the median.

Third, effectiveness depends on diffusion stage and technology. Proportional boosts are larger when major policies arrive earlier, at low penetration, and smaller when introduced later. Solar is more policy-sensitive than wind, consistent with modularity and shorter development cycles. The practical implication is simple: deployment policy works best when it targets the binding constraint in a given system – bankability, administrative throughput, or infrastructure – rather than relying on headline instrument labels.

Four limitations qualify our interpretation. First, the OECD+ sample, chosen for consistent policy and capacity measurement. External validity for emerging and developing economies is uncertain: higher financing costs, weaker administrative capacity, and tighter grid constraints may alter which instruments bind and when. Extending comparable event-study designs will likely require new, harmonised datasets on policy design and deployment outside the OECD context. Second, annual stringency scores allow cross-country comparability but it blurs policy design details (e.g. bankability, contract structure, indexation, curtailment, permitting, grid connection). Measurement error and within-family heterogeneity likely attenuate estimated effects for some instruments, especially those implemented through complex market rules. Third, the design targets medium-run dynamics and does not model longer-run feedbacks such as deployment reshaping later policy, supply chains, and system constraints. Combining this kind of diffusion-based event studies with structural investment or power-system models would be a natural next step, and would help connect deployment capture with evolving system constraints (e.g. transmission, flexibility). Fourth, identification rests on conditional comparisons, not experiments: first-reform, isolation, and spillover checks bound but cannot exclude unmeasured complementary reforms or residual cross-border interference, though the biases we can sign attenuate the estimates, so the headline effects are better read as lower bounds than inflated upper ones.

Declaration of generative AI and AI-assisted technologies in the manuscript preparation process

During the preparation of this work the author(s) used ChatGPT in order to improve the readability and language of the text. After using this tool/service, the author(s) reviewed and edited the content as needed and take(s) full responsibility for the content of the published article.

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Appendix

A Policy data-sources, mapping, and event construction

A.1 Sources, scope, and taxonomy

We use the CAPMF (Nachtigall et al. 2024; OECD 2025) database, which organises climate and energy interventions into a hierarchical taxonomy: L1 (broad area), L2 (domain), L3 (instrument family), and L4 (specific instrument/measure). L4 series are annual country–time variables that the provider transforms to a common percentile-based stringency scale $P \in [0, 10]$ (details below). Our sample is the intersection of countries and years covered in this policy database and in our technology panel.

A.2 Policy selection: documentation and mapping

Selection workflow. We start from L4 instruments tagged by the keywords *Renewables*, *Electricity*, *Coal*, and *Fossil fuels*. From these we keep families repeatedly analysed in the cross-country renewables literature (technology-specific FITs/auctions, renewable certificates or quotas, carbon pricing in power, coal exit, planning targets, and renewable energy innovation support). Instruments targeted at technologies other than wind and solar (e.g., hydrogen, nuclear) or outside the power sector are excluded. Cross-border measures with weak domestic deployment channels (e.g., bans on financing *overseas* coal) are excluded due to insufficient country representation.

We estimate at L4 by default and aggregate to L3 for robustness[14]. We do not go above L3 (L2/L1) because aggregation becomes too coarse to interpret instrument-specific effects; when multiple L4s within a family move together, we comment on the commonality at the L3 level.

Family-to-variable map. Table A.1 shows illustrative L4 labels by family; the complete correspondence (including polarity, inclusion in baseline vs. robustness, and edge-case notes) is noted in (OECD 2025).

Table A.1: Illustrative mapping from policy families to L4 variables

Family	**Illustrative L4 variables**
Feed-in-Tariffs / auctions (tech-specific)	`LEV4_FIT_SOL_{PR,DUR}`, `LEV4_FIT_WIND_{PR,DUR}`, `LEV4_AUCTION_SOL/WIND_{PR,DUR}`
Renewable certificates / quotas	`LEV4_RECS`
Power-sector carbon / pricing	`LEV4_ETS_E_PR`, `LEV4_CARBONTAX_E_PR`, `LEV4_FFS_E`, `LEV4_EXCISETAX_E_{COAL,NATGAS}`
Coal exit measures	`LEV4_PHOUT_COAL_{DATE,STAT}`, `LEV4_BAN_COAL_{DATE,STAT}`
Planning / expansion targets	`LEV4_RENEWABLE_EXP`
Innovation (RES)	`LEV4_RDD_RES`

A.3 From instrument values to a comparable stringency scale

The CAPMF database provides pre-computed stringency measures for each policy (constructed from one or more underlying policy variables). We summarise their construction below.

[14]Note that US data was only available at L3 level, therefore, we do not comment about policy implications for the US.

Percentile-threshold stringency. Each policy variable v is observed as a raw value X_{vct} (e.g. a tax rate, subsidy level, or regulatory threshold) in country c and year t. CAPMF maps these raw values into a discrete stringency level $P_{vct} \in \{0, 1, \dots, 10\}$ using the in-sample distribution across all countries and years.

A value of $P_{vct} = 0$ is assigned if the policy/action is not in place. Otherwise, CAPMF assigns levels using percentile thresholds computed over the empirical distribution of in-place observations for variable v: $P_{vct} = 10$ if X_{vct} is at or above the 90th percentile, $P_{vct} = 9$ if it lies between the 80th and 90th percentile, and so on, down to $P_{vct} = 1$ if it is below the 10th percentile but in place. For categorical variables, the highest category is assigned 10 and remaining categories are linearly mapped into the 1–10 range.

Policy-level stringency for policy j is then computed by averaging the stringency levels of its underlying variables with equal weights:

$$P_{jct} = \frac{1}{|V_j|} \sum_{v \in V_j} P_{vct},$$

where V_j denotes the set of policy variables grouped into policy j. Missing values are set to zero to enable aggregation and are flagged as missing in the database.

Polarity and harmonisation. Larger values are aligned with greater decarbonisation support by multiplying by a family-specific sign $s_f \in \{-1, +1\}$. For instance, the sign is $s_f = +1$ for ETS/carbon tax; and $s_f = -1$ for fossil-fuel *subsidies* so that subsidy reductions raise stringency. An increase in stringency always goes in the direction of limiting climate change.

A.4 Policy events-construction and measurement

Event definition. In the main text we define an event as a policy event at time t if policy stringency has changed by at least $\tilde{\eta}$ over the preceding $\tilde{\tau}$ years,

$$E(t) = \mathbf{1}\{|P(t) - P(t - \tilde{\tau})| \geq \tilde{\eta}\} \times \eta_t, \quad \eta_t = P(t) - P(t - \tilde{\tau}).$$

The sign of η_t captures the direction of the change. Our implementation applies this rule to the *oriented* series P. Hence $E(t) \neq 0$ flags an event, and $\operatorname{sign} E(t) = \operatorname{sign} \eta_t$ distinguishes strengthenings (+) from weakenings (−).

Missingness. If X_{jct} is absent because the instrument does not exist, we set $P_{jct} = 0$ (absent). If numerically undefined (e.g., ETS price pre-launch), we use the provider's pre-launch code (zero on P).

Event types and thresholds. We use three specifications, allowing the change to take place over 1 or 2 years, and having a strictly positive stringency threshold.

Event type	**Short name**	$\tilde{\eta}$	$\tilde{\tau}$
AnyChange-1y	`AC`	0	1
Significant-Gradual-2y (baseline)	`SG`	2	2
Significant-Abrupt-1y	`SA`	2	1

The baseline `SG` follows Stechemesser et al. (2024); `AC` and `SA` serve as robustness checks. Under the percentile scaling, $\tilde{\eta} = 2$ is a 20-percentile move.

The simple and obvious case is of a sharp and sudden increase of stringency of ($\Delta P = 2$) over 1 year. Figure A.1 shows how different situations translate into event definition under the `SG` specification.

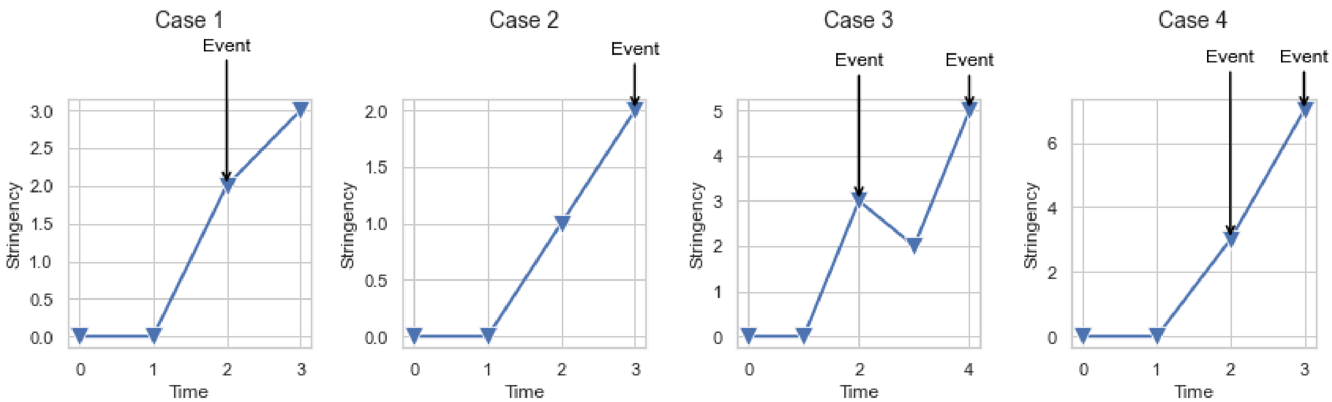


Figure A.1: Event significant not sharp cases

Event-year assignment and spacing. For each country–instrument, the *first* year t that satisfies the rule defines the cohort E_{jc} used in event-study stacking. To avoid double-counting small step-changes, we impose a cooldown of $K = 2$ years: subsequent triggers within $[t+1, t+K]$ for that instrument are ignored. If multiple families trigger in the same year, each forms a separate country–policy cohort; in an "any-instrument" robustness, we keep the earliest positive move and drop others in that year.

B Identification robustness checks

This appendix evaluates the robustness of our findings to reasonable alternatives in sample construction, identification, and inference. For each class of check, we report the full event-study lead–lag profile ($\ell \in [-pre, +post]$) with 95% and 90% confidence intervals and provide concise summaries of the difference with the baseline specification.

B.1 All policies for wind and solar (main specification)

This section compiles the full set of Sun–Abraham interaction–weighted (IW) event studies for every OECD policy family that clears our minimum sample rule (at least ten treated countries or treatments). For each family we plot the event path ν_ℓ for $\ell \in [-5, 5]$ (omitting $\ell = -1$ as the reference point), overlay the baseline in grey with the main robustness specification on top, and show 95%/90% bands; panel headers report `nCountry` - the number of countries with at least one event, `nTreat` the total number of distinct country-events, R^2, and the joint Wald pre–trend p–value.

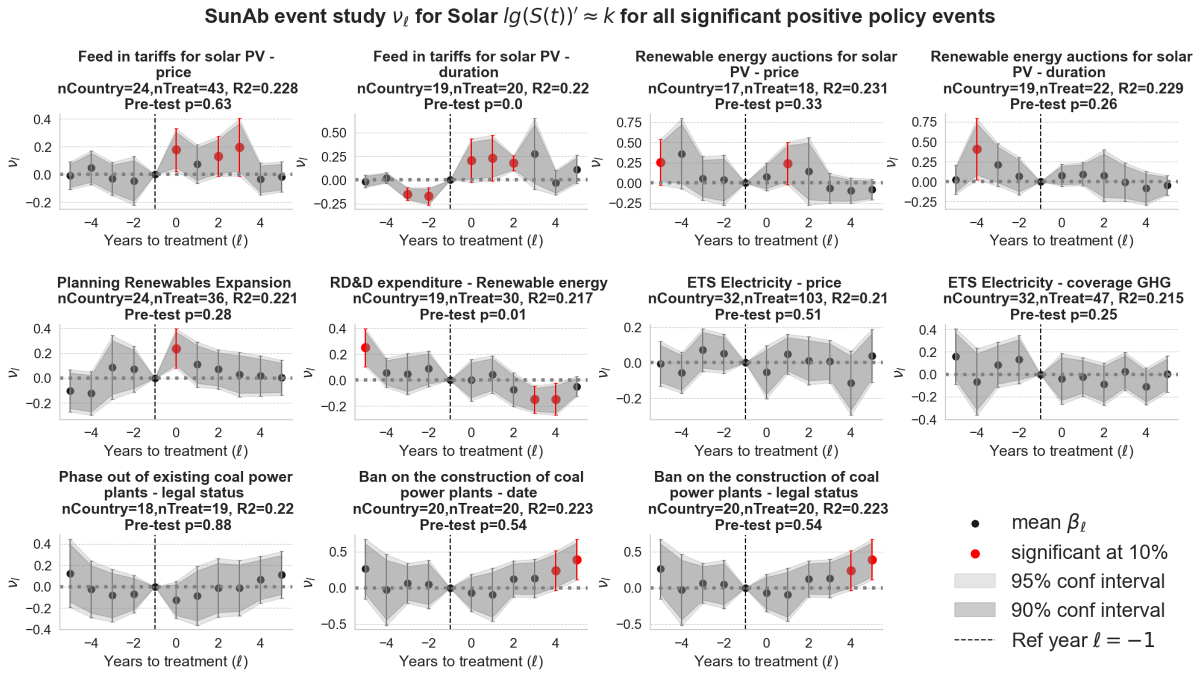
We proceed in three steps. *(i) Level-4 (technology–specific) families*-separately for solar and wind and for positive vs. negative shocks-are in Figure B.1 and Figure B.2. *(ii) Level-3 aggregates*, which pool related instruments, are shown in Figure B.3 as a coarser cross–check of sign and timing. *(iii) Average summaries*-pre and post averages that respect the SUNAB design-appear in Table B.1–Table B.2; these are descriptive complements to the full ν_ℓ profile and can understate delayed or hump–shaped effects. Pre–trend diagnostics for the baseline and long–lead windows are tabulated in Table B.4–Table B.7; we privilege families that pass the joint pre–trend screen and flag small–N categories (listed in figure captions) as non–inferential coverage.

B.1.1 Level 4 policy aggregation

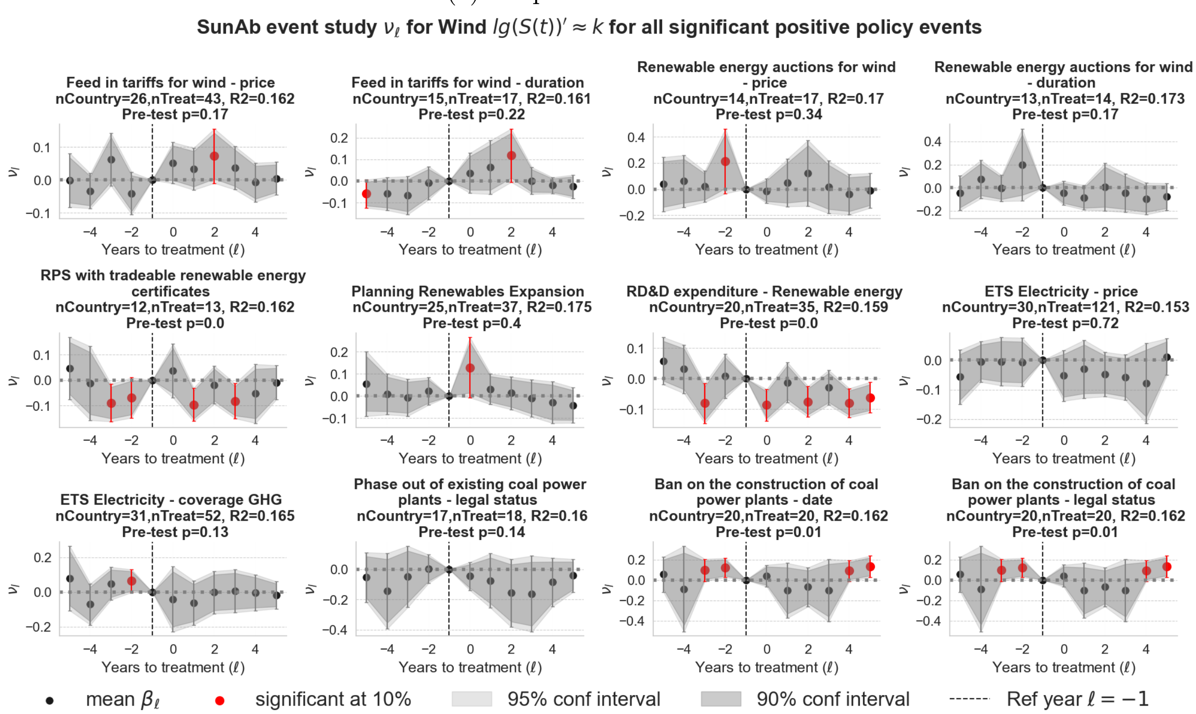
We begin with Level-4 (technology-specific) policy families. Figure B.1 and Figure B.2 show the full Sun–Abraham interaction-weighted (IW) event paths, ν_ℓ for $\ell \in [-5, 5]$ with $\ell = -1$ omitted, separately for solar and wind and split by shock sign. Each panel overlays the baseline (grey) with the main robustness specification (top layer), 95% and 90% confidence bands, and marks coefficients significant at the 10% level. Panel headers report the number of treated countries and total treatments (`nCountry`, number of countries with a least one event, `nTreat`, number of distinct events), the model R^2, and the joint Wald pre-trend p-value.

Figure B.1 shows the **positive** policy events. Four L4 policies are added compared to Figure 4: ETS Electricity price, ETS Electricity coverage GHG, RPS with tradeable renewable energy certification, and Phase out of existing coal power. None of them have significant post-event coefficients with flat pre-trends.

For **negative** policy events (Figure B.2), there are two more policies: FFS reform for fossil fuel production and ETS Electricity price compared to Figure 5.

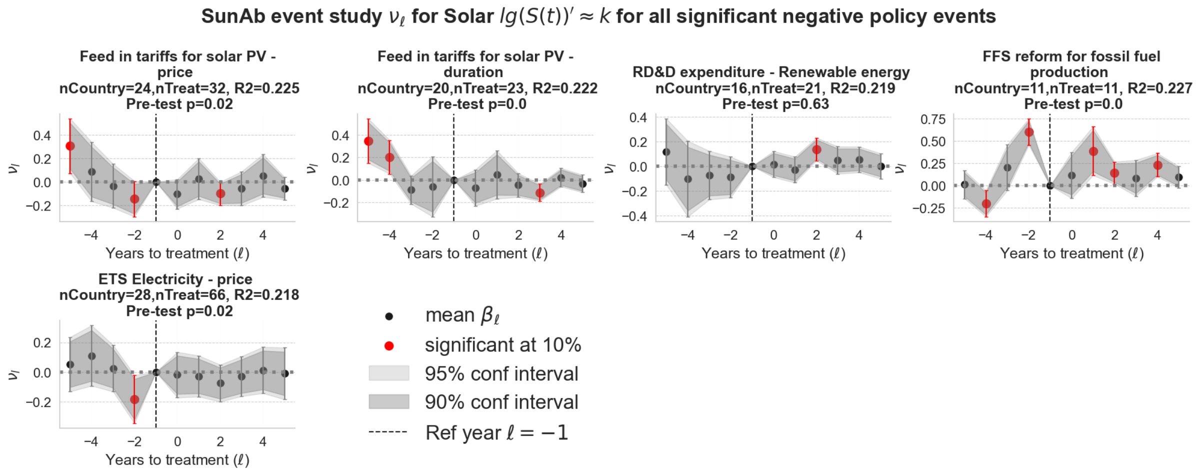


(a) All positive L4 - solar

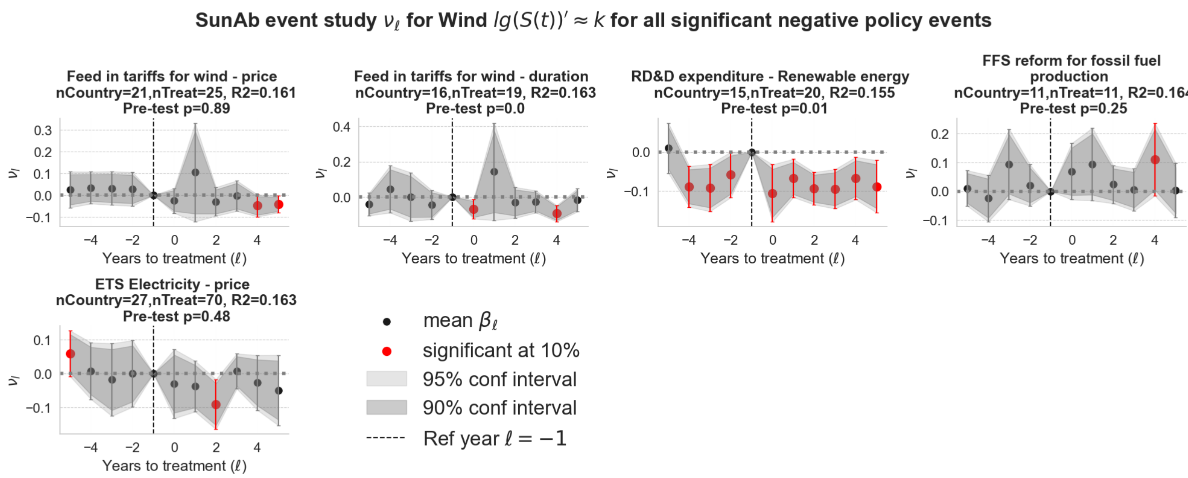


(b) All positive L4 - wind

Figure B.1: All significant **positive** L4 policy events under the main specification. Each panel plots the event-study path ν_ℓ by years relative to treatment (ℓ). Dots show the mean ν_ℓ; red dots mark coefficients significant at 10%. Shaded bands are 95% and 90% confidence intervals, and the dashed vertical line marks the policy introduction. Panel headers show the number of treated countries and total number of treatments (`nCountry`, `nTreat`), model R^2, and the Wald pre-trend test p-value. For statistical confidence include only policies with more than 10 countries or treatments (Excluded: Reform of Fossil Fuel Subsidies - Electricity, Excise taxes on natural gas - Electricity and Excise taxes on coal - Electricity, Carbon Tax - Electricity,Reform of Fossil Fuel Subsidies - Electricity, FFS reform for fossil fuel production.)



(a) All negative L4 - solar



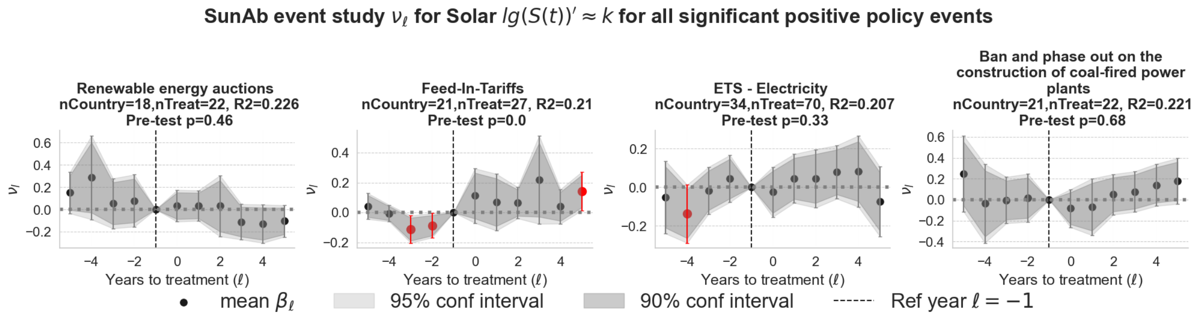
(b) All negative L4 - wind

Figure B.2: All significant **negative** L4 policy events. Layout and notation as in Figure B.1. Negative responses indicate adverse movements in the outcome around policy introduction; the side-by-side comparison highlights whether such effects are technology-specific or broadly sectoral.

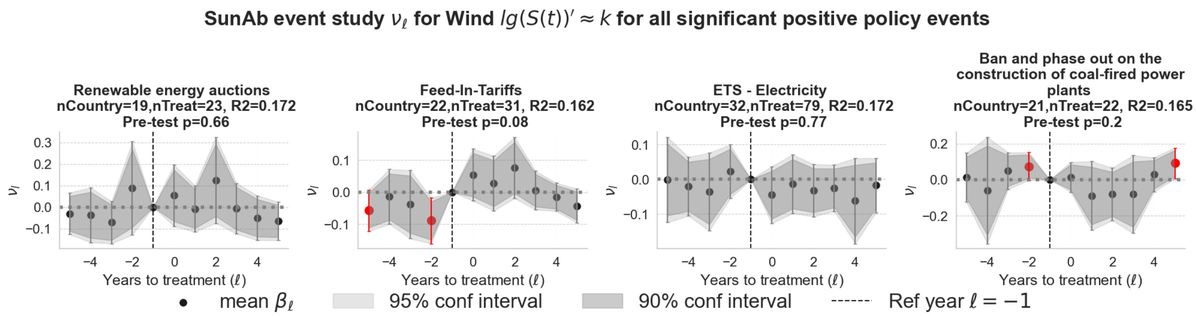
B.1.2 Level 3 policy aggregation

Level-3 aggregates coarsen policy definitions (e.g., pooling price mechanisms or planning tools across technology subclasses). Figure B.3 reports the same IW event paths for all Level-3 families that have more granular counterparts at Level-4. Two patterns emerge.

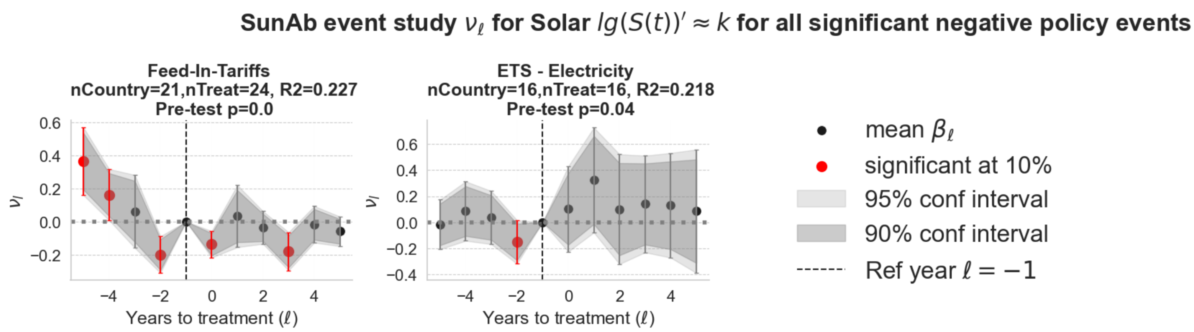
First, where Level-4 results are strong and clean (e.g., FIT–PR and Renewable Expansion), Level-3 aggregates preserve the sign and broad timing of effects, albeit with muted magnitudes and slightly wider bands - an expected result when grouping heterogeneous sub-instruments and technology contexts. Second, for families that exhibit mixed behaviour at Level-4 (e.g., auctions and certificate schemes), the Level-3 pooling washes out sharp dynamics and increases the chance of borderline pre-trend statistics, reinforcing our choice to emphasise Level-4 specificity in the main analysis. In short, Level-3 offers a useful cross-check of direction and rough timing, but identification is tighter at Level-4, and our interpretations follow that hierarchy.



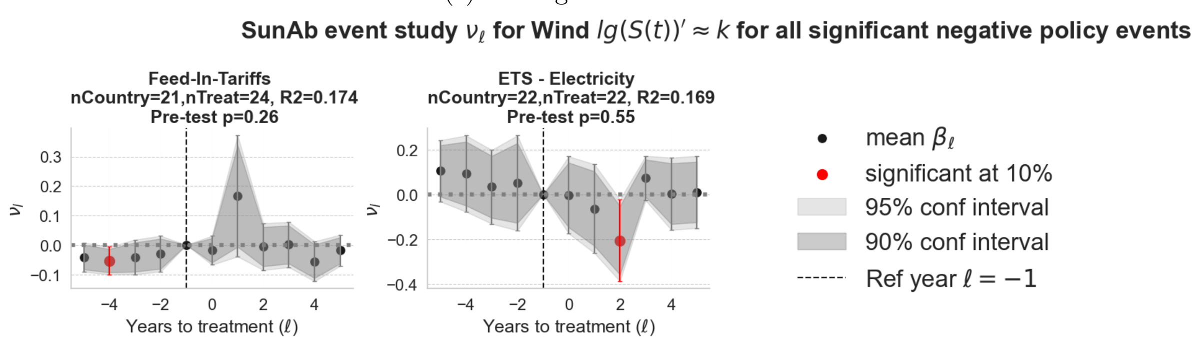
(a) All positive L3 - solar



(b) All positive L3 - wind



(c) All negative L3 - solar



(d) All negative L3 - wind

Figure B.3: L3 policy shocks with greater specificity at L4 under the main specifications. Columns represent technologies (solar, wind), and rows represent scenarios (All positive All negative).

B.1.3 Average post coefficient

To summarise dynamics in a single number per policy family (DiD-like), we report average "pre" and "post" coefficients that respect the Sun–Abraham design. Specifically, let $\hat{\boldsymbol{v}}$ collect the estimated event-time coefficients and $\widehat{\Sigma}$ their robust variance–covariance matrix. Define weights w_ℓ proportional to cohort availability (or, equivalently, the SUNAB exposure shares) at each ℓ. We compute

$$\bar{\nu}^{\,\text{pre}} = \sum_{\ell \in \{-5,-4,-3,-2\}} w_\ell\, \hat{\nu}_\ell, \qquad \bar{\nu}^{\,\text{post}} = \sum_{\ell \in \{0,1,2,3,4,5\}} w_\ell\, \hat{\nu}_\ell,$$

and obtain standard errors by the delta method using $\widehat{\Sigma}$. This construction down-weights sparsely observed leads/lags and yields a compact measure of pre-treatment drift versus average post-treatment response while preserving the correct joint uncertainty. Table B.1 and Table B.2 report these summaries for Level-4 solar and wind. As emphasised in the captions, these scalars are *descriptive complements* to the full event path: they aid readability (and power pooled meta-comparisons across families) but do not replace inspection of dynamic adjustment, persistence, or overshoot that only the full ℓ-profile can reveal.

Table B.1: Average pre- and post-event coefficients (positive events; Level-4 solar policies). Dependent variable: $\Delta \log(S)$.

Policy	Countries	Pre-event avg Coef. (SE) [95% CI]	Post-event avg Coef. (SE) [95% CI]
AUCTION SOL DUR	19	0.179** (0.086) [0.011, 0.346]	0.020 (0.078) [-0.133, 0.173]
AUCTION SOL PR	17	0.178* (0.095) [-0.007, 0.364]	0.037 (0.088) [-0.135, 0.209]
BAN COAL DATE	20	0.088 (0.128) [-0.163, 0.340]	0.121 (0.113) [-0.099, 0.342]
BAN COAL STAT	20	0.088 (0.128) [-0.163, 0.340]	0.121 (0.113) [-0.099, 0.342]
CARBONTAX E PR	6	-0.244*** (0.047) [-0.336, -0.152]	0.079 (0.097) [-0.110, 0.268]
ETS E GHG	32	0.078 (0.082) [-0.083, 0.239]	-0.038 (0.051) [-0.138, 0.063]
ETS E PR	32	0.015 (0.042) [-0.068, 0.098]	-0.010 (0.049) [-0.106, 0.086]
EXCISETAX E COAL	1	-0.163** (0.078) [-0.315, -0.010]	-0.058 (0.047) [-0.150, 0.035]
EXCISETAX E NATGAS	1	-0.163** (0.078) [-0.315, -0.010]	-0.058 (0.047) [-0.150, 0.035]
FFS E	1	0.220*** (0.052) [0.118, 0.321]	0.142*** (0.036) [0.071, 0.213]
FFS PRODUCER	7	0.166 (0.126) [-0.080, 0.412]	0.112 (0.089) [-0.061, 0.286]
FIT SOL DUR	18	-0.075*** (0.026) [-0.127, -0.023]	0.142*** (0.055) [0.035, 0.249]
FIT SOL DUR[a]	14	-0.132*** (0.035) [-0.201, -0.063]	0.151*** (0.050) [0.052, 0.250]
FIT SOL PR	24	-0.009 (0.049) [-0.105, 0.087]	0.088* (0.047) [-0.004, 0.180]
PHOUT COAL DATE	10	0.054 (0.077) [-0.097, 0.204]	0.087 (0.074) [-0.057, 0.231]
PHOUT COAL STAT	18	-0.013 (0.091) [-0.192, 0.167]	-0.009 (0.095) [-0.196, 0.177]
RDD RES	19	0.110** (0.051) [0.010, 0.209]	-0.063 (0.045) [-0.150, 0.025]
RECS	10	-0.055 (0.070) [-0.192, 0.082]	-0.143*** (0.035) [-0.212, -0.074]
RENEWABLE EXP	24	-0.017 (0.066) [-0.146, 0.112]	0.079 (0.068) [-0.056, 0.213]

Notes: Table reports averages of event-time coefficients for positive events (as defined in the main text) for Level-4 solar policies. "Pre-event avg" averages leads $\{\ell \leq -1\}$; "Post-event avg" averages nonnegative lags $\{\ell \geq 0\}$ unless otherwise noted. Coefficients are changes in $\Delta \log(S)$; numbers in parentheses are standard errors; brackets report 95% confidence intervals. Asterisks denote statistical significance based on two-sided tests: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. "Countries" is the count of distinct countries contributing to each policy estimate.

[a] For FIT SOL DUR, the post-event average omits the mechanical impact year and averages $\{\ell \geq +1\}$.

Table B.2: Average pre- and post-event coefficients (positive events; Level-4 wind policies). Dependent variable: $\Delta \log(S)$.

Policy	Countries	Pre-event avg Coef. (SE) [95% CI]	Post-event avg Coef. (SE) [95% CI]
AUCTION WIND DUR	13	0.055 (0.064) [-0.070, 0.179]	-0.058 (0.065) [-0.185, 0.070]
AUCTION WIND PR	14	0.083 (0.066) [-0.046, 0.212]	0.020 (0.079) [-0.134, 0.174]
BAN COAL DATE	19	0.040 (0.082) [-0.120, 0.200]	0.029 (0.070) [-0.108, 0.167]
BAN COAL STAT	19	0.040 (0.082) [-0.120, 0.200]	0.029 (0.070) [-0.108, 0.167]
CARBONTAX E PR	6	0.154*** (0.054) [0.047, 0.261]	-0.017 (0.017) [-0.050, 0.015]
ETS E GHG	31	0.031 (0.037) [-0.041, 0.104]	-0.019 (0.041) [-0.099, 0.060]
ETS E PR	30	-0.018 (0.032) [-0.082, 0.045]	-0.043 (0.033) [-0.107, 0.022]
EXCISETAX E COAL	1	0.018 (0.044) [-0.068, 0.103]	0.038 (0.031) [-0.023, 0.098]
EXCISETAX E NATGAS	1	0.017 (0.044) [-0.068, 0.103]	0.038 (0.031) [-0.023, 0.099]
FFS E	1	0.085** (0.037) [0.013, 0.156]	0.164*** (0.037) [0.090, 0.237]
FFS PRODUCER	6	0.016 (0.036) [-0.055, 0.088]	0.148*** (0.030) [0.089, 0.208]
FIT WIND DUR	14	-0.041 (0.029) [-0.099, 0.016]	0.013 (0.021) [-0.028, 0.054]
FIT WIND PR	26	-0.003 (0.023) [-0.048, 0.042]	0.032 (0.023) [-0.013, 0.077]
PHOUT COAL DATE	10	-0.008 (0.057) [-0.119, 0.103]	-0.003 (0.044) [-0.090, 0.084]
PHOUT COAL STAT	17	-0.060 (0.080) [-0.218, 0.097]	-0.094 (0.078) [-0.247, 0.058]
RDD RES	20	0.004 (0.029) [-0.053, 0.061]	-0.057*** (0.021) [-0.099, -0.016]
RECS	12	-0.031 (0.043) [-0.114, 0.053]	-0.037* (0.021) [-0.078, 0.005]
RENEWABLE EXP	24	-0.010 (0.036) [-0.080, 0.060]	-0.001 (0.035) [-0.070, 0.068]

Notes: Table reports averages of event-time coefficients for positive events (as defined in the main text) for Level-4 wind policies. "Pre-event avg" averages leads $\{\ell \leq -1\}$; "Post-event avg" averages nonnegative lags $\{\ell \geq 0\}$. Coefficients are changes in $\Delta \log(S)$; numbers in parentheses are standard errors; brackets report 95% confidence intervals. Asterisks denote statistical significance based on two-sided tests: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. "Countries" is the count of distinct countries contributing to each policy estimate.

Two practical notes. First, families with very small effective N (after trimming) can produce apparently "precise" averages driven by a narrow ℓ subset; we flag those with small `nCountry`/`nTreat` and refrain from causal statements. Second, when the pre-average is statistically different from zero, we do not interpret the post-average causally in the main text unless the full path shows that pre-trends are small in magnitude and the joint Wald test fails to reject at conventional levels.

The pre–post aggregation is a useful dashboard for comparing policy families at a glance, provided it is read as a complement to the full ν_ℓ paths. In Table B.1–Table B.2, the averages recover the broad ranking implied by the disaggregated coefficients: solar FIT–PR shows a small but positive $\bar{\nu}^{\text{post}}$ with $\bar{\nu}^{\text{pre}} \approx 0$, matching the step-up in ν_ℓ after $\ell = 0$; solar RECs delivers a clearly negative and precise $\bar{\nu}^{\text{post}}$, consistent with persistently negative ν_ℓ; wind RD&D (RDD RES) exhibits a significantly negative $\bar{\nu}^{\text{post}}$, again mirroring the largely negative post-ℓ profile. These are cases where the average summary faithfully tracks the disaggregated evidence and can be cited directly.

The averages obscure a technology-specific, *delayed* solar response. For solar, Ban Coal-Status passes the pre-trend screen and displays statistically positive ν_ℓ only at later lags (notably $\ell \in \{4, 5\}$); pooling $\ell = \{0, \ldots, 5\}$ therefore dilutes these late-onset gains, yielding a small $\bar{\nu}^{\text{post}}$. Ban Coal-Date shows pre-movement in solar; any late positive ν_ℓ (again, concentrated at $\ell \in \{4, 5\}$) should be read descriptively rather than causally. For wind, both Date and Status exhibit positive pre-leads and lack a consistent rise in the post period, so the near-zero (or negative) $\bar{\nu}^{\text{post}}$ is aligned with the event paths. Taken together, coal bans are *suggestive for solar with a lagged onset (especially Status)*, but not for wind; the pre–post average is conservative here because it pools early post years with late effects.

In other families, the averages remain informative but conservative relative to the ν_ℓ dynamics. Solar FIT–DUR has a positive and significant $\bar{\nu}^{\text{post}}$ but also $\bar{\nu}^{\text{pre}} < 0$, echoing the documented pre-trend and reinforcing a descriptive (not causal) reading despite the positive post period. Solar auctions (AUCTION SOL PR/DUR) yield $\bar{\nu}^{\text{post}}$ close to zero even though mid-lag ν_ℓ are often positive; averaging across $\ell = \{0, \ldots, 5\}$ naturally mutes ramp-up patterns. For planning (RENEWABLE EXPANSION) and ETS (PR/GHG) the post averages hover around zero for both technologies, aligning with mostly flat ν_ℓ profiles and indicating limited short-run effects on adoption speed.

A second takeaway is that the averages provide a quick internal check on parallel trends. Families that fail the joint pre-trend test typically display non-zero $\bar{\nu}^{\text{pre}}$ as a visible badge: solar FIT–DUR and RDD RES have $\bar{\nu}^{\text{pre}} \neq 0$; several wind families with coal-phaseout and trading features show elevated pre-leads. Where $\bar{\nu}^{\text{pre}} \neq 0$, we keep the corresponding $\bar{\nu}^{\text{post}}$ in the appendix for transparency but refrain from causal language in the main text unless the full ν_ℓ sequence and the joint test together support it.

Finally, small-N categories (excise taxes; some FFS variants) illustrate a limitation of aggregation: their $\bar{\nu}^{\text{post}}$ can look precise due to narrow sampling of ℓ or few cohorts, yet they are not reliable for inference. Overall, the pre–post summaries corroborate the main patterns: supportive deployment instruments translate most cleanly into solar (FIT–PR, with FIT–DUR positive but pre-trended), negative responses concentrate in RECs and wind RD&D, coal bans are *delayed and solar-specific* (averages are conservative), and several broad instruments (ETS, planning) show limited average short-run impact-exactly what the underlying ν_ℓ paths imply.

B.2 Joint Wald tests on the headline averages

Table B.3 reports the joint Wald p-value p_W for the null hypothesis that all event-time coefficients aggregated into each cell of Table 1 are simultaneously zero. For the short-run column the null is $H_0 : \nu_0 = \nu_1 = \nu_2 = \nu_3 = 0$; for the medium-run column it is $H_0 : \nu_2 = \nu_3 = \nu_4 = \nu_5 = 0$. The Wald statistic is computed from the same country-clustered variance-covariance matrix as the standard errors reported in Table 1, using the Moore–Penrose pseudoinverse and its rank as the degrees of freedom when the relevant covariance block is rank-deficient. The pointwise t-test reported in Table 1 asks whether the *averaged* ATT $\bar{\nu} = |H|^{-1} \sum_{\ell \in H} \nu_\ell$ is distinguishable from zero; the joint Wald test asks the strictly weaker question of whether *any* individual horizon coefficient inside H is distinguishable from zero. The two can therefore diverge in two informative ways.

Table B.3: Joint Wald p-values for the short- and medium-run effects in table 1

	Main		First event		Isolated	
Instrument	Short-run ($\ell \in \{0,1,2,3\}$)	Medium-run ($\ell \in \{2,3,4,5\}$)	Short-run ($\ell \in \{0,1,2,3\}$)	Medium-run ($\ell \in \{2,3,4,5\}$)	Short-run ($\ell \in \{0,1,2,3\}$)	Medium-run ($\ell \in \{2,3,4,5\}$)
Panel A: Solar						
FIT price	0.092	0.073	< 0.001	< 0.001	< 0.001	0.032
FIT duration	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001
Planning	0.391	0.386	0.495	0.360	0.003	0.905
Auctions	0.810	0.343	0.679	0.352	0.641	0.084
Coal exit	0.221	0.086	0.525	0.130	0.216	0.725
RD&D	0.082	0.068	0.101	0.082	0.194	0.201
Pricing (ETS / tax)	0.801	0.303	0.113	0.518	0.035	0.180
Panel B: Wind						
FIT price	0.310	0.306	0.058	0.003	0.109	0.068
FIT duration	0.037	0.006	0.037	0.006	0.037	0.006
Planning	0.193	0.127	0.364	0.076	< 0.001	0.001
Auctions	0.298	0.035	0.204	0.046	0.678	0.646
Coal exit	0.619	0.444	0.668	0.424	0.429	0.367
RD&D	0.455	0.052	0.202	0.003	0.448	0.113
Pricing (ETS / tax)	0.543	0.673	0.281	0.170	< 0.001	0.139

Notes: Each entry is the joint Wald p-value p_W for $H_0 : \nu_{\ell_1} = \cdots = \nu_{\ell_k} = 0$ against the alternative that at least one $\nu_\ell \neq 0$ – the test of whether the event-time coefficients aggregated in table 1 over the indicated horizon are simultaneously zero. The Wald statistic is computed from the same country-clustered variance-covariance matrix as the standard errors in the companion table, using the Moore–Penrose pseudoinverse and its rank as the degrees of freedom when the relevant covariance block is rank-deficient. Specifications and columns match those of table 1.

A small p_W with a pointwise-insignificant $\bar{\nu}$ implies that the dynamic path inside the averaging window has structure of mixed sign which the averaging operator partially cancels out. The clearest example is wind feed-in tariff duration. In the Main specification the short-run averaged ATT is 0.008 (s.e. 0.028) and the pointwise t-test does not reject zero, yet $p_W = 0.037$ rejects the joint null at the 5% level. The same pattern survives under First event ($p_W = 0.037$) and Isolated ($p_W = 0.037$). The four short-run coefficients $\nu_0, \dots, \nu_3$ are not jointly zero – they are individually small with opposite signs – but their equally-weighted mean is. The medium-run joint test rejects more strongly ($p_W = 0.006$ in all three specifications) and the pointwise medium-run ATT becomes positive and significant in Main and Isolated (0.079, $p < 0.05$).

A large p_W with a pointwise-significant $\bar{\nu}$ would imply the opposite – that the averaged ATT is large but no individual horizon contributes precisely enough to reject – and we treat such cells as suggestive rather than decision-grade. Solar feed-in tariff durations reject jointly at $p_W < 0.001$ in every cell of every column, reinforcing the pointwise ATT of 0.18*** (Main) and 0.10–0.15 in the alternative samples. Solar RD&D under Main and First event combines marginally negative pointwise estimates with marginal joint tests (p_W between 0.07 and 0.10), and the joint test does not reject under Isolated – consistent with the qualitative reading that the negative response is real but partly attenuated when same-family histories are removed.

For the headline null cells the joint test also fails to reject in the great majority of cases: the absence of an averaged effect for planning, auctions, coal exits, and carbon pricing on solar – and for everything except feed-in tariff duration on wind – is not produced by mechanical cancellation of significant within-window dynamics, but reflects a genuinely flat event-time path.

B.3 Pre-trends tests

We diagnose parallel trends using a joint Wald test over pre-treatment leads of the Sun–Abraham interaction-weighted (IW) event-time coefficients. Let $\hat{\boldsymbol{v}}$ collect the estimated event-time effects and $\widehat{\Sigma}$ their cluster-robust variance–covariance matrix (clustered by country). With base period $\ell = -1$ omitted, define the pre-treatment index set $\mathcal{L}_{\text{pre}} = \{-5, -4, -3, -2\}$ for the baseline window. The null of parallel trends is

$$H_0 : \ \nu_\ell = 0 \ \ \forall \ell \in \mathcal{L}_{\text{pre}},$$

which we test with the Wald statistic

$$W = (R\hat{\boldsymbol{v}})^\top \left(R\,\widehat{\Sigma}\,R^\top\right)^{-1} (R\hat{\boldsymbol{v}}),$$

where R selects the $\ell \in \mathcal{L}_{\text{pre}}$ coefficients. We report the corresponding p-value in the "Joint p-value" column of Table B.4 and Table B.5. As a diagnostic (not a replacement for the joint screen), we also display per-lead $\hat{\nu}_\ell$ with standard errors and significance stars. Our interpretation rule is conservative: when the joint test fails to reject at 10%, we treat pre-trends as acceptable; when it rejects at 10% or below, we flag the family and refrain from causal claims in the main text.[15]

Table B.4: solar Joint and Per-lead Wald pre-trends

Policy	Joint p-value	-5	-4	-3	-2
AUCTION SOL DUR	0.257	0.023 (0.088)	0.390** (0.187)	0.201 (0.130)	0.069 (0.114)
AUCTION SOL PR	0.328	0.230* (0.129)	0.342 (0.209)	0.053 (0.130)	0.035 (0.148)
BAN COAL DATE	0.537	0.264 (0.211)	−0.028 (0.250)	0.066 (0.141)	0.052 (0.170)
BAN COAL STAT	0.538	0.264 (0.211)	−0.028 (0.250)	0.066 (0.141)	0.052 (0.170)
ETS E GHG	0.251	0.153 (0.121)	−0.063 (0.146)	0.083 (0.101)	0.132 (0.109)
ETS E PR	0.513	−0.006 (0.063)	−0.054 (0.057)	0.069 (0.060)	0.050 (0.055)
FIT SOL DUR	0.000***	−0.013 (0.031)	0.022 (0.027)	−0.140*** (0.033)	−0.162*** (0.045)
FIT SOL PR	0.633	−0.007 (0.045)	0.045 (0.057)	−0.028 (0.055)	−0.041 (0.081)
PHOUT COAL DATE	0.000***	0.111 (0.112)	0.124 (0.098)	−0.080 (0.090)	0.061 (0.093)
PHOUT COAL STAT	0.875	0.125 (0.163)	−0.025 (0.135)	−0.083 (0.125)	−0.068 (0.089)
RDD RES	0.012**	0.216*** (0.065)	0.055 (0.056)	0.045 (0.078)	0.084 (0.068)
RECS	0.011**	−0.111 (0.068)	0.036 (0.118)	−0.114 (0.122)	−0.012 (0.108)
RENEWABLE EXP	0.283	−0.102 (0.087)	−0.124 (0.089)	0.086 (0.130)	0.073 (0.094)

Notes: For each policy joint Wald Pre-trend p-value (with significance stars shown) and Per-lead Wald pre-trend ν coefficients with standard errors in parantheses. Stars indicate significance of pre-trend coefficients: * p<0.10, ** p<0.05, *** p<0.01. Additionally, we note a joint Wald Pre-trend p-value for FIT SOL DUR of 0.436006 under a doughnut pre-trends test such $\ell \notin \{-3, -2, -1\}$ removing anticipation years.

[15] Because per-lead checks involve multiple comparisons and sparse cohorts in far leads, they are read descriptively alongside the joint test rather than as stand-alone inference.

Table B.5: wind Joint and Per-lead Wald pre-trends

Policy	Joint p-value	-5	-4	-3	-2
AUCTION WIND DUR	0.175	−0.047 (0.076)	0.072 (0.084)	−0.005 (0.057)	0.198 (0.159)
AUCTION WIND PR	0.338	0.037 (0.106)	0.061 (0.101)	0.019 (0.061)	0.214* (0.126)
BAN COAL DATE	0.010***	0.054 (0.088)	−0.087 (0.215)	0.097* (0.055)	0.120** (0.048)
BAN COAL STAT	0.010***	0.054 (0.088)	−0.087 (0.215)	0.097* (0.056)	0.120** (0.048)
ETS E GHG	0.133	0.078 (0.093)	−0.068 (0.060)	0.050 (0.048)	0.065* (0.034)
ETS E PR	0.717	−0.056 (0.047)	−0.005 (0.034)	−0.004 (0.041)	−0.007 (0.042)
FIT WIND DUR	0.222	−0.045* (0.025)	−0.059 (0.039)	−0.062 (0.042)	−0.009 (0.037)
FIT WIND PR	0.169	−0.001 (0.039)	−0.032 (0.026)	0.060 (0.039)	−0.039 (0.031)
PHOUT COAL DATE	0.217	−0.041 (0.067)	−0.036 (0.059)	0.012 (0.061)	0.032 (0.053)
PHOUT COAL STAT	0.140	−0.053 (0.084)	−0.143 (0.127)	−0.048 (0.103)	0.002 (0.048)
RDD RES	0.000***	0.048 (0.033)	0.027 (0.035)	−0.074** (0.030)	0.007 (0.034)
RECS	0.003***	0.043 (0.057)	−0.011 (0.068)	−0.089** (0.038)	−0.069* (0.040)
RENEWABLE EXP	0.397	0.056 (0.075)	0.009 (0.047)	−0.006 (0.043)	0.022 (0.032)

Notes: For each policy joint Wald Pre-trend p-value (with significance stars shown) and Per-lead Wald pre-trend ν coefficients with standard errors in parantheses. Stars indicate significance of pre-trend coefficients: * p<0.10, ** p<0.05, *** p<0.01.

Table B.6: solar Joint and Per-lead Wald pre-trends (long-leads)

Policy	Joint p-value	-7	-6	-5	-4	-3	-2
AUCTION SOL DUR	0.280	−0.010 (0.120)	−0.041 (0.149)	0.002 (0.113)	0.373** (0.181)	0.189 (0.125)	0.060 (0.107)
AUCTION SOL PR	0.321	0.063 (0.144)	0.015 (0.154)	0.224 (0.154)	0.338 (0.212)	0.054 (0.129)	0.035 (0.139)
BAN COAL DATE	0.829	−0.012 (0.175)	−0.023 (0.221)	0.223 (0.241)	−0.033 (0.251)	0.061 (0.138)	0.047 (0.163)
BAN COAL STAT	0.829	−0.012 (0.175)	−0.023 (0.221)	0.223 (0.241)	−0.033 (0.251)	0.061 (0.138)	0.047 (0.163)
ETS E GHG	0.555	0.020 (0.096)	0.025 (0.133)	0.141 (0.119)	−0.042 (0.154)	0.092 (0.099)	0.135 (0.107)
ETS E PR	0.633	−0.108 (0.071)	−0.066 (0.064)	−0.009 (0.062)	−0.059 (0.061)	0.066 (0.057)	0.044 (0.053)
FIT SOL DUR	0.000***	0.107*** (0.030)	0.030 (0.034)	0.000 (0.034)	0.033 (0.029)	−0.128*** (0.034)	−0.156*** (0.045)
FIT SOL PR	0.850	0.010 (0.052)	0.009 (0.056)	−0.003 (0.050)	0.049 (0.056)	−0.022 (0.057)	−0.029 (0.080)
PHOUT COAL DATE	0.001***	0.104 (0.099)	0.098 (0.126)	0.140 (0.116)	0.147 (0.093)	−0.062 (0.083)	0.079 (0.086)
PHOUT COAL STAT	0.395	0.030 (0.118)	−0.138 (0.137)	0.059 (0.178)	−0.071 (0.134)	−0.109 (0.123)	−0.090 (0.083)
RDD RES	0.001***	−0.021 (0.035)	0.090** (0.043)	0.217*** (0.074)	0.065 (0.055)	0.046 (0.080)	0.089 (0.066)
RECS	0.001***	−0.055 (0.051)	−0.084 (0.069)	−0.117 (0.074)	0.033 (0.125)	−0.116 (0.117)	−0.014 (0.105)
RENEWABLE EXP	0.226	−0.135 (0.122)	−0.249* (0.134)	−0.167* (0.098)	−0.158 (0.100)	0.057 (0.142)	0.071 (0.084)

Notes: For each policy joint Wald Pre-trend p-value (with significance stars shown) and Per-lead Wald pre-trend ν coefficients with standard errors in parantheses. Stars indicate significance of pre-trend coefficients: * p<0.10, ** p<0.05, *** p<0.01. Additionally, we note a joint Wald Pre-trend p-value for FIT SOL DUR of 0.339490 under a doughnut pre-trends test such $\ell \notin \{-3, -2, -1\}$ removing anticipation years.

Table B.7: wind Joint and Per-lead Wald pre-trends (long-leads)

Policy	Joint p-value	-7	-6	-5	-4	-3	-2
AUCTION WIND DUR	0.147	0.113 (0.106)	−0.037 (0.040)	−0.044 (0.079)	0.075 (0.092)	−0.002 (0.057)	0.203 (0.162)
AUCTION WIND PR	0.167	0.139 (0.102)	0.033 (0.055)	0.055 (0.115)	0.075 (0.110)	0.029 (0.063)	0.226* (0.127)
BAN COAL DATE	0.013**	0.056 (0.048)	0.007 (0.117)	0.056 (0.107)	−0.083 (0.212)	0.102* (0.059)	0.125*** (0.048)
BAN COAL STAT	0.013**	0.056 (0.048)	0.007 (0.117)	0.056 (0.107)	−0.083 (0.212)	0.102* (0.059)	0.125*** (0.048)
ETS E GHG	0.155	−0.137 (0.104)	−0.020 (0.065)	0.036 (0.091)	−0.080 (0.062)	0.039 (0.053)	0.057* (0.033)
ETS E PR	0.717	0.030 (0.050)	0.014 (0.035)	−0.066 (0.047)	−0.002 (0.036)	0.000 (0.041)	−0.003 (0.041)
FIT WIND DUR	0.001***	0.036 (0.023)	−0.069 (0.043)	−0.047* (0.026)	−0.062 (0.040)	−0.067* (0.040)	−0.013 (0.037)
FIT WIND PR	0.000***	0.003 (0.044)	0.061 (0.038)	−0.000 (0.041)	−0.039 (0.030)	0.073** (0.034)	−0.040 (0.032)
PHOUT COAL DATE	0.243	0.010 (0.066)	0.027 (0.057)	−0.034 (0.067)	−0.030 (0.059)	0.016 (0.060)	0.036 (0.050)
PHOUT COAL STAT	0.288	−0.010 (0.058)	−0.003 (0.082)	−0.053 (0.098)	−0.146 (0.127)	−0.050 (0.102)	0.002 (0.048)
RDD RES	0.000***	−0.002 (0.026)	0.026 (0.029)	0.049 (0.033)	0.029 (0.036)	−0.074** (0.031)	0.009 (0.034)
RECS	0.000***	0.038 (0.059)	−0.128** (0.061)	0.044 (0.059)	−0.001 (0.073)	−0.085** (0.040)	−0.062 (0.043)
RENEWABLE EXP	0.374	0.069 (0.055)	0.032 (0.056)	0.073 (0.081)	0.016 (0.056)	0.005 (0.052)	0.024 (0.035)

Notes: For each policy joint Wald Pre-trend p-value (with significance stars shown) and Per-lead Wald pre-trend ν coefficients with standard errors in parantheses. Stars indicate significance of pre-trend coefficients: * p<0.10, ** p<0.05, *** p<0.01.

Windows and sensitivity. The baseline window uses $\ell \in [-5, 5]$ (with $\ell = -1$ omitted) and tests pre-trends on $\{-5, -4, -3, -2\}$. To probe sensitivity, we also ran (i) a shorter pre-window $\{-3, -2\}$ and (ii) a long-leads variant $\{-7, -6, -5, -4, -3, -2\}$. The shorter window yields conclusions consistent with baseline (not tabulated). Long-leads results are reported in Table B.6-Table B.7. As expected, extending to $\ell = -7$ increases sampling noise (fewer cohorts observed that far back) and occasionally triggers rejections driven by isolated far-lead movements; we therefore retain the baseline window for headline inference and treat long-leads as a robustness diagnostic.

Solar: patterns in Table B.4. Most Level-4 solar families pass the joint screen (e.g., Auction SOL PR/DUR, ETS, Renewable Expansion, FIT SOL PR). A minority exhibit significant pre-treatment dynamics: FIT SOL DUR (joint $p<0.01$) and certificate- and RD&D-related families (RECs, RD&D RES) reject at conventional levels; PHOUT COAL DATE also rejects. These cases often show one or two markedly non-zero leads that line up with known announcement or pipeline effects. Additionally, we note a joint Wald Pre-trend p-value for FIT SOL DUR of 0.436006 under a doughnut pre-trends test such $\ell \notin \{-3, -2, -1\}$ removing anticipation years, thus passing the pre-trend test. In the long-leads table (Table B.6), these rejections persist or strengthen for the same families, consistent with additional noise and potential anticipatory dynamics in far leads.

Wind: patterns in Table B.5. Wind displays broadly benign pre-trends for core deployment families: Auction WIND PR/DUR, ETS, FIT WIND PR/DUR, and Renewable Expansion all fail to reject at 10% in the baseline window. In contrast, Ban Coal variants (Date/Status) show significant pre-movement (joint $p \leq 0.01$), as do RECs and RD&D RES. Under long-leads (Table B.7), some families that passed in the baseline window (notably FIT WIND PR/DUR) register rejections at the 1% level, driven by sparse $\ell \in \{-7, -6\}$ cohorts; because the baseline window remains balanced and more precisely estimated, we privilege the baseline screen in our main interpretation.

What we tried when pre-trends appear. For flagged families we implemented two stabilisers: (i) *leave-one-country-out* (LOCO) jackknife to identify leverage points; and (ii) cohort-specific linear pre-trend adjustments (allowing treated-cohort slopes in the pre-period). In several cases these steps either removed isolated outliers (improving the joint p-value) or revealed that rejections were driven by one or two high-variance cohorts. However, for FIT SOL DUR the joint pre-trend rejection persisted across LOCO and cohort-slope specification, and we therefore treat its post-period path as descriptive rather than causal in the main text. The same caution applies to PHOUT COAL DATE (solar) and to Ban Coal and RECs/RD&D (wind).

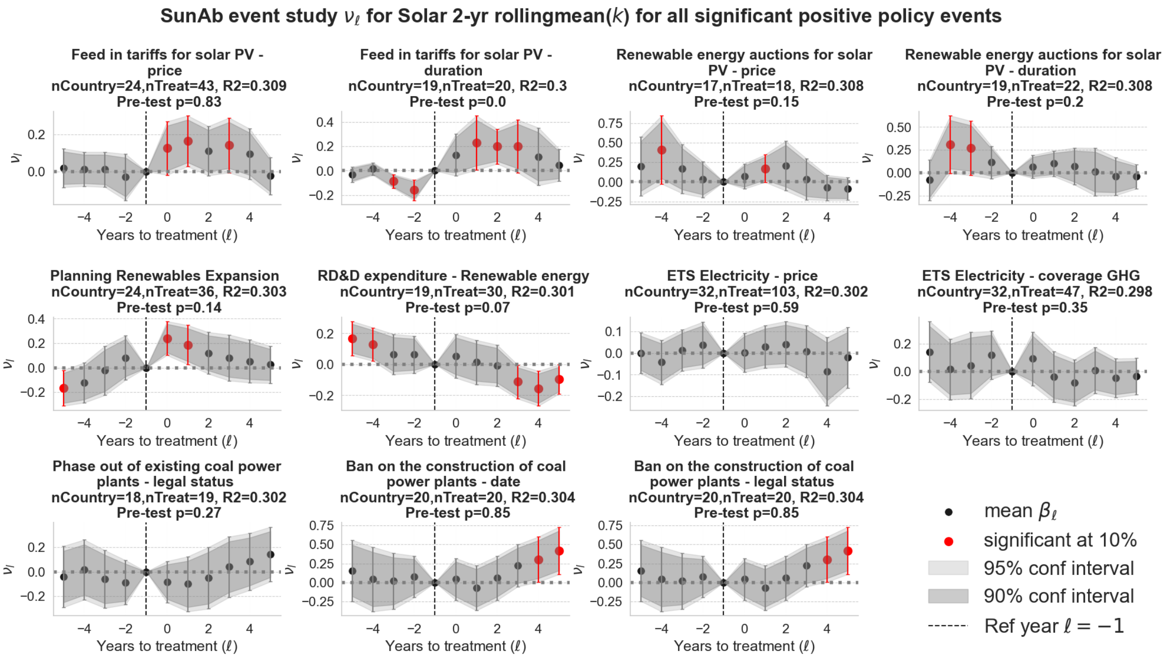
Takeaway. The parallel-trends screen supports causal interpretation for a subset of well-defined deployment and planning policies - notably FIT PR, Auctions, ETS, and Renewable Expansion under the baseline window. Families exhibiting significant pre-movement are transparently reported, retained in figures/tables for coverage, and discussed as *descriptive dynamics* rather than causal effects.

B.4 Alternate dependent variable

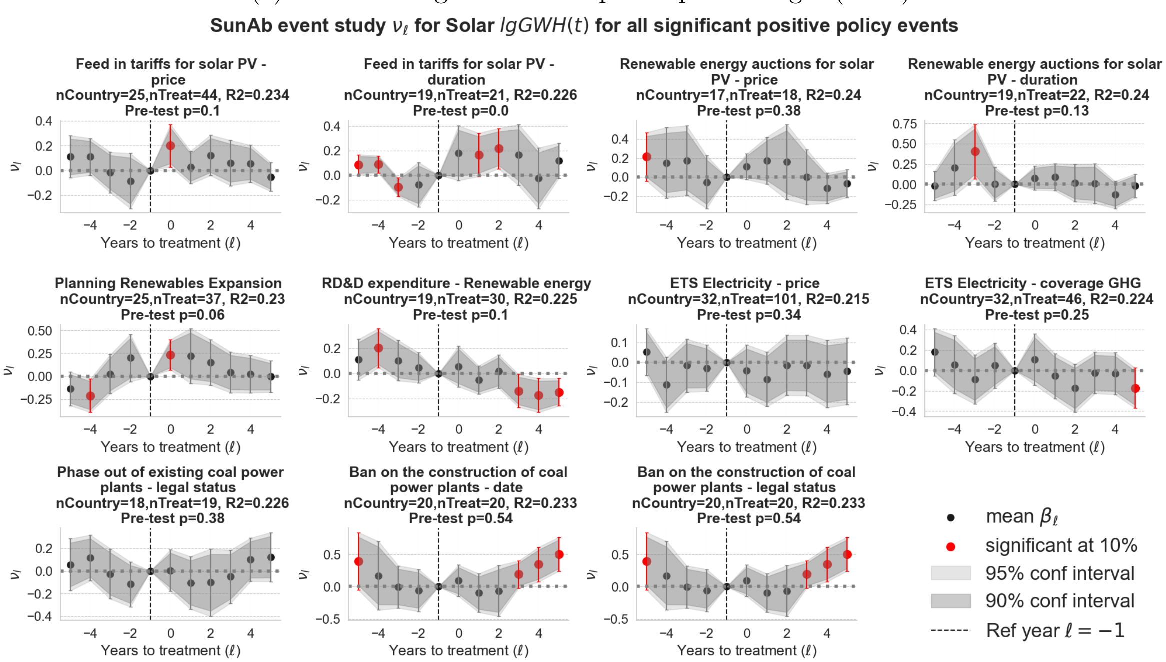
We evaluate robustness to the outcome definition along two dimensions. First, we smooth the baseline adoption-speed series $\Delta \log S$ (capacity in MW) using a two-year rolling mean (RM2) to attenuate transitory volatility from lumpy commissioning and small-base effects.[16] Second, we replace capacity with electricity production (GWh), which incorporates operational realities (capacity factors, outages, curtailment) and tests whether policy effects carry through to delivered generation rather than nameplate capacity.[17]

[16]RM2 is a centred two-year rolling mean applied to the baseline outcome; the transformation and trimming rules otherwise match the main specification.

[17]Production is transformed identically to the baseline outcome (same differenced-log convention and sample trimming), and the Sun–Abraham IW specification, fixed effects, and event window are unchanged. Coverage differences are reflected in the `nCountry`/`nTreat` counts reported on each panel.

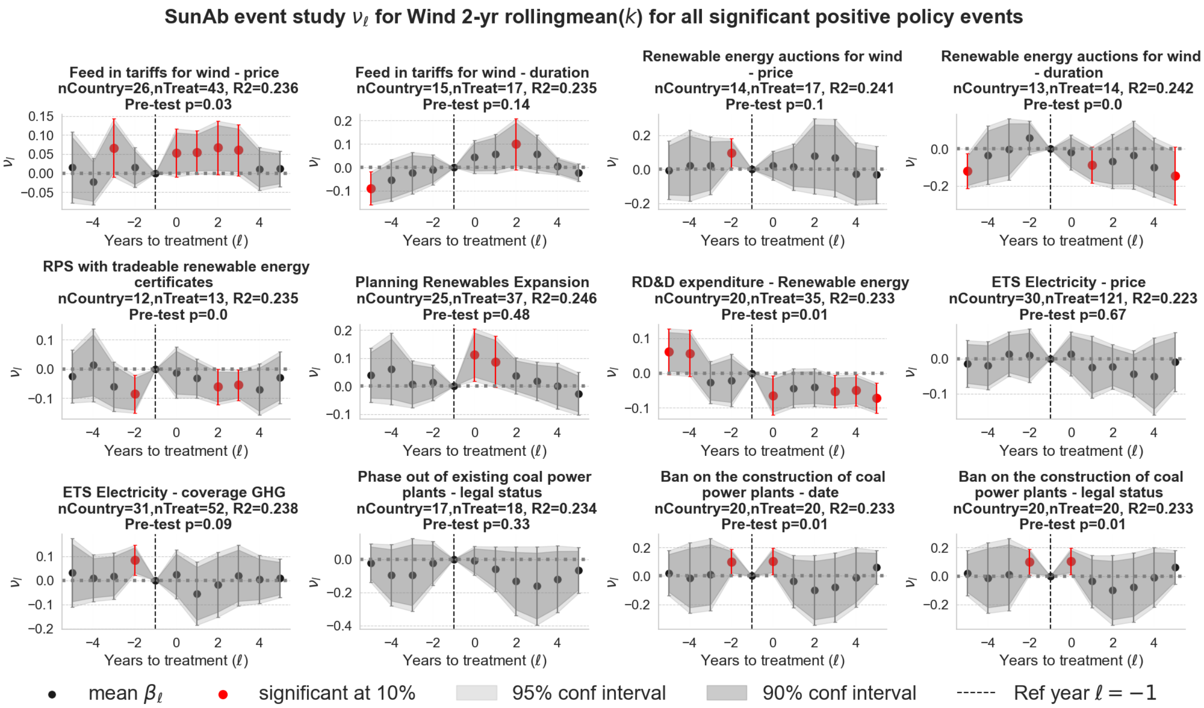


(a) solar - rolling mean of adoption speed $\Delta \log S$ (RM2)

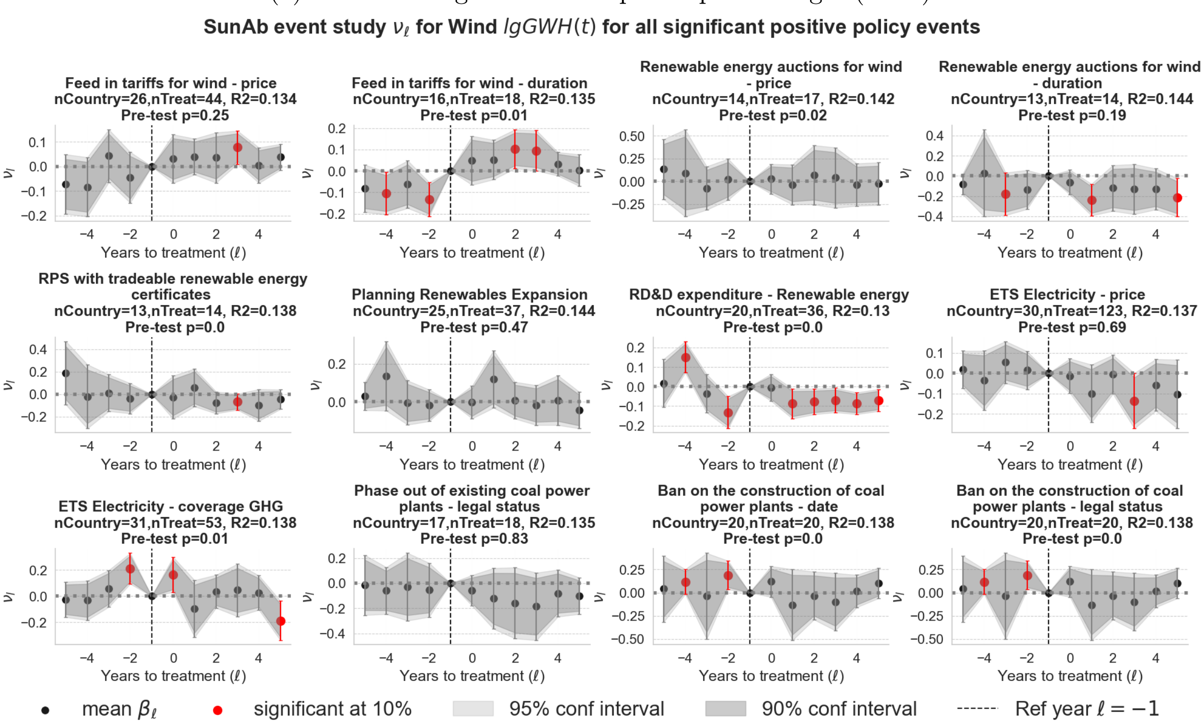


(b) solar - production (GWh)

Figure B.4: solar: event-study estimates for all significant **positive** L4 policy events using two alternative dependent variables. Panel (a) uses the rolling mean (RM2) of the adoption-speed series $\Delta \log S$ to smooth short-run volatility in entry/adoption rates; panel (b) replaces baseline capacity (MW) with production (GWh). This figure verifies whether the timing, sign and significance of the estimated policy responses for solar are sensitive to smoothing the outcome via a rolling mean or to changing the outcome from capacity to production; observed differences mainly reflect changes in scale (MW vs GWh) and the variance-reducing effect of the rolling mean.



(a) wind - rolling mean of adoption speed $\Delta \log S$ (RM2)



(b) wind - production (GWh)

Figure B.5: wind: event-study estimates for all significant **positive** L4 policy events using two alternative dependent variables. Panel (a) uses the rolling mean (RM2) of $\Delta \log S$ to attenuate transitory noise in adoption-speed; panel (b) uses production (GWh) instead of capacity (MW). This figure checks robustness of the wind results to smoothing and to the outcome definition; consistency in sign and timing across panels supports the main inference, while any changes in magnitude or precision reflect outcome scaling and the smoothing effect.

Figure B.4–Figure B.5 replicate the Sun–Abraham interaction-weighted event paths ν_ℓ ($\ell \in [-5, 5]$, omitting $\ell = -1$) for all significant **positive** Level-4 policy families under these two alternative outcomes. Three findings are consistent across solar and wind:

(i) Smoothing reduces noise without changing the story. With RM2, confidence bands narrow and isolated per-lead blips shrink toward zero, as expected when high-frequency noise is averaged out. Peak responses sometimes shift one period closer to $\ell = 0$, reflecting reduced transitory dips in $\ell = 0$–1. Crucially, the *sign* and *timing* of post-treatment responses for core deployment families (e.g., FIT–PR, AUCTIONS, RENEWABLE EXPANSION) are unchanged relative to the baseline. We do not observe cases where smoothing creates a new pre-trend rejection; where the baseline failed the pre-trend screen (e.g., selected certificate or RD&D panels), RM2 generally leaves that diagnosis intact.

(ii) Production (GWh): preserves direction and timing, with modest attenuation. In principle we can interpret $\Delta \log G$ (with $G \propto S \times CF$) where S - Capacity, CF - Capacity factor such that:

$$\Delta \log G \propto \Delta \log S + \Delta \log CF.$$

If $\Delta \log CF$ is mean-zero noise orthogonal to policy, point estimates for ν_ℓ are attenuated and bands widen relative to capacity. If $\Delta \log CF$ co-moves with policy (e.g., curtailment during rapid build-outs, grid constraints that tighten after support shocks), the *timing* of ν_ℓ can shift and late-post lags may dominate. Four nuances follow:

(a) Attenuation and precision. For both technologies, production panels typically preserve the sign and timing seen in capacity but with slightly smaller magnitudes and wider bands-consistent with weather and operational variability embedded in CF. This explains why families that are clearly positive in capacity sometimes appear only 1σ–positive in production.

(b) Commissioning–to–generation lag. Some policies trigger build decisions at $\ell \approx 0$ but generation materialises after Commercial-Operation-Date and grid connection. This mechanical lag can shift mass from early to later post years, making late-post averages (e.g., $\ell \in \{2, 3, 4, 5\}$) more informative for production than a flat $\{0, \ldots, 5\}$ average.

(c) Technology asymmetries. wind production inherits larger inter-annual resource variability than solar (gustiness vs. irradiance), so GWh-based ν_ℓ for wind tend to be noisier than for solar. Conversely, solar production can be more exposed to mid-day curtailment in fast-growing systems, which may mute early post-ℓ effects relative to capacity.

(d) Coverage and selection. Generation series are sometimes shorter or patchier than capacity registries; reduced `nCountry`/`nTreat` in the headers reflects this. Where coverage trims far leads, pre-trend screens are unchanged in spirit but estimated with fewer cohorts; we therefore keep production evidence as *corroboration*, not as a replacement for capacity in the headline.

(iii) Cautionary families remain cautionary. Policy families flagged for pre-treatment drift in the baseline (e.g., RD&D RES, some certificate schemes, and coal-ban variants in wind) do not convert into clean causal cases under either RM2 or production. Their qualitative patterns persist, reinforcing our practice of presenting them transparently while avoiding causal language.

Takeaway. Outcome re-specification - via smoothing (RM2) or switching from capacity to production - does not overturn the main results. Core deployment and planning instruments continue to show post-treatment increases with similar timing, whereas families with baseline pre-trend concerns remain non-causal. For clarity and comparability we retain $\Delta \log S$ (capacity) as the headline outcome in the main text and use RM2/production as robustness evidence in this appendix.

B.5 Window and sample restrictions

Our baseline Sun–Abraham specification estimates dynamic effects $\{\nu_\ell\}$ on a relative-time window $\ell \in [-5, +5]$ using risk sets that require at least one pre-period and (up to) five post-period observations per treated episode, with the fixed effects and covariates held constant throughout. Because staggered adoption with incomplete panels can make dynamic estimates sensitive to the chosen window and to which episodes are eligible to contribute at each ℓ, we subject our results to a suite of *design* checks that vary the window and the sample while preserving the estimator and fixed-effect structure. These checks are intended as stress tests for timing and shape, not as alternative sources of identification. Conditioning on future availability (e.g., complete post windows) or on realised diffusion (e.g., mature-adoption subsets) changes the composition and weighting of cohorts and therefore shifts the target estimand.[18]

We proceed in four steps. First, we plot raw outcome trajectories centered on the policy year to show the un-demeaned data around $\ell = 0$ and to make coverage limitations transparent (Appendix B.5.1). Second, we impose a *full-window* restriction, keeping only events with complete coverage for all $\ell \in [-5, +5]$, and compare paths to the baseline (Figure B.8). Third, we vary the event window to ± 7 and ± 3 years while keeping the same risk-set rules, to check whether the sign and onset of effects survive longer/shorter tails (Appendix B.5.3). Finally, we examine *stage* restrictions: (i) limiting the cross-section to countries that had reached at least 2% penetration by 2023, and (ii) limiting within-country observations to post-1% years; we also contrast the baseline outcome (market-scale deployment) with a version that includes sub-utility/market-scale additions (Appendix B.5.4 and subsequent tables).

How to read the robustness figures/tables. Across all checks we keep the estimator, fixed effects, and policy coding unchanged; only the window or the eligible sample differs. Baseline estimates appear in grey; test estimates/plots in colour. Focus on: (i) the *sign and timing* at $\ell \in \{0, 1, 2\}$; (ii) whether peaks *shift* (selection toward later-stage cohorts can move mass to $\ell = 2$–3); (iii) changes in *precision* (bands widen as effective N falls); and (iv) pre-trend screens recalculated for the relevant leads. Where differences emerge, we interpret them through the lens of composition (survivorship and maturity), not as reversals of the underlying causal pattern. The ± 5 baseline window remains our preferred balance of coverage and precision; the variants below demonstrate that the principal qualitative conclusions are not driven by a particular window choice or by the inclusion/exclusion of early, small-base observations.

B.5.1 Plot of raw data centred around $\ell = 0$

To complement the model-based event-time estimates, we plot the *raw* outcome trajectories centred on the policy year for each Technology $\times$ Policy family. For every qualifying event (c, τ_c) in country c with event year τ_c, we construct the relative-time index $\ell = t - \tau_c$ and trace the baseline outcome series $y_{c,\ell} = \Delta \log S_{c,\tau_c+\ell}$ over a wide window $\ell \in [-7, +7]$ (with no residualisation or fixed-effect demeaning). We display one figure for **positive** events per Technology $\times$ Policy.

Visual encoding follows a simple completeness rule. Episodes with *full* coverage of the window (all $\ell \in [-7, +7]$ observed) are drawn in a primary colour; episodes with *limited* coverage (missing some ℓ in the

[18] In staggered designs, altering eligibility at specific ℓ values changes the implicit weighting and the set of contributing cohorts; see the discussion accompanying each figure/table for the resulting estimand. We re-apply the joint Wald test for pre-trends using the window-specific set of leads.

window) are drawn in a secondary colour at lower opacity.[19] A dashed vertical line marks $\ell = 0$ (the policy year), and a subtle marker is placed at $\ell = 0$ on each series to aid alignment when lines overlap. When a country experiences multiple events in the same family, each event-year is plotted as its own line and label.

These panels are descriptive diagnostics. Pre-period slopes ($\ell < 0$) reveal visible pre-movement; post-period changes ($\ell \geq 0$) indicate raw step-ups/downs without model weighting. Because we do not apply fixed effects in this plot, cross-country level differences are *not* meaningful; inference should focus on within-line changes around $\ell = 0$ and on whether many *full* lines co-move. The subsequent Sun–Abraham estimates translate these visual regularities into ν_ℓ with proper risk sets, fixed effects, and uncertainty.

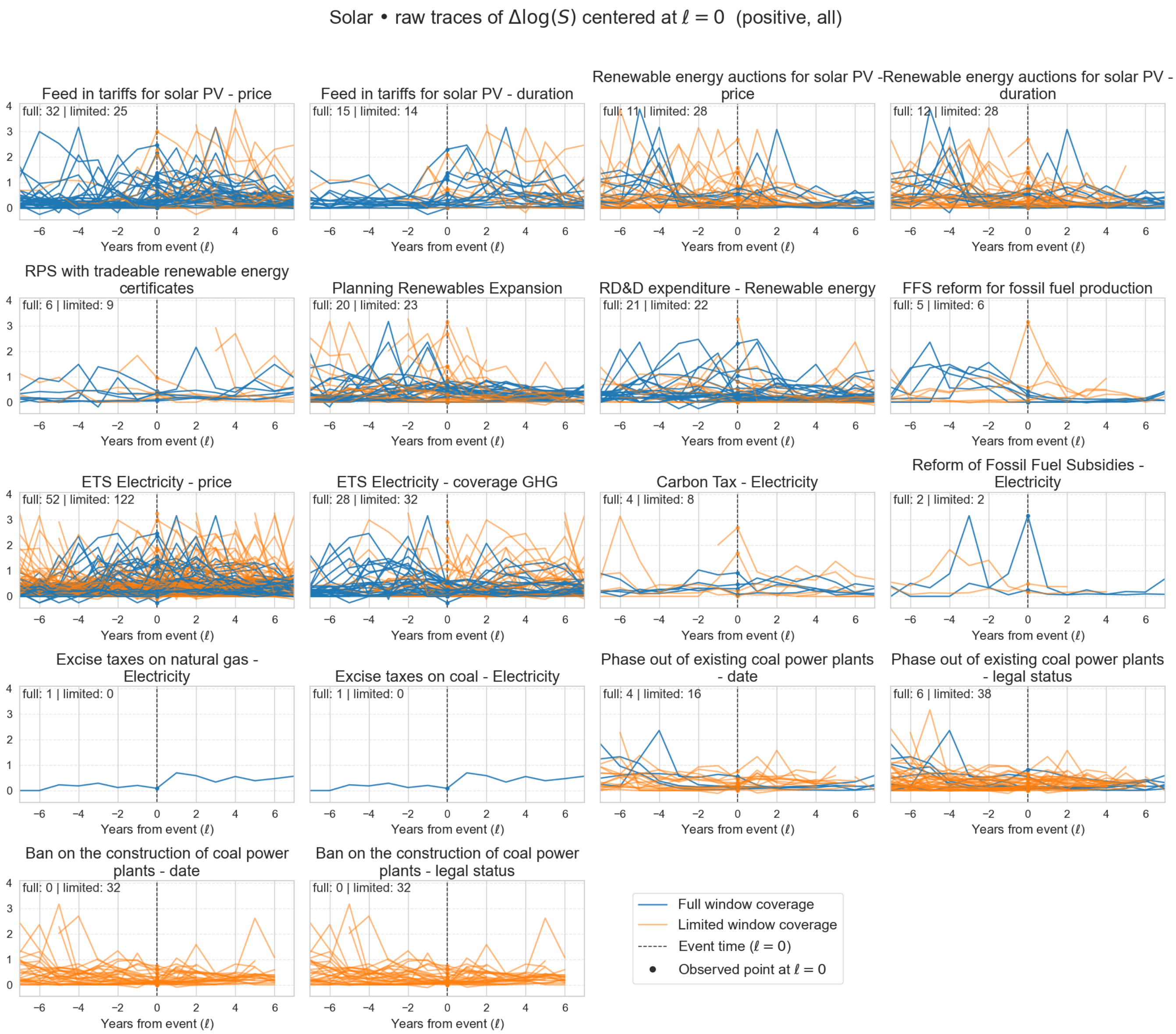


Figure B.6: Plot of raw solar data $\Delta \log(S)$ for positive event windows of ± 7 centered at zero showing countries with full and limited data for each policy. Blue – full length (also annnotated full), orange – partially complete (annotated as limited) for the window.

[19]This "full/limited" distinction anticipates the trimming used in subsequent figures. Series shown as *limited* here are precisely those that will be excluded by observed-cells/risk-set requirements later, so this plot doubles as a transparent provenance list.

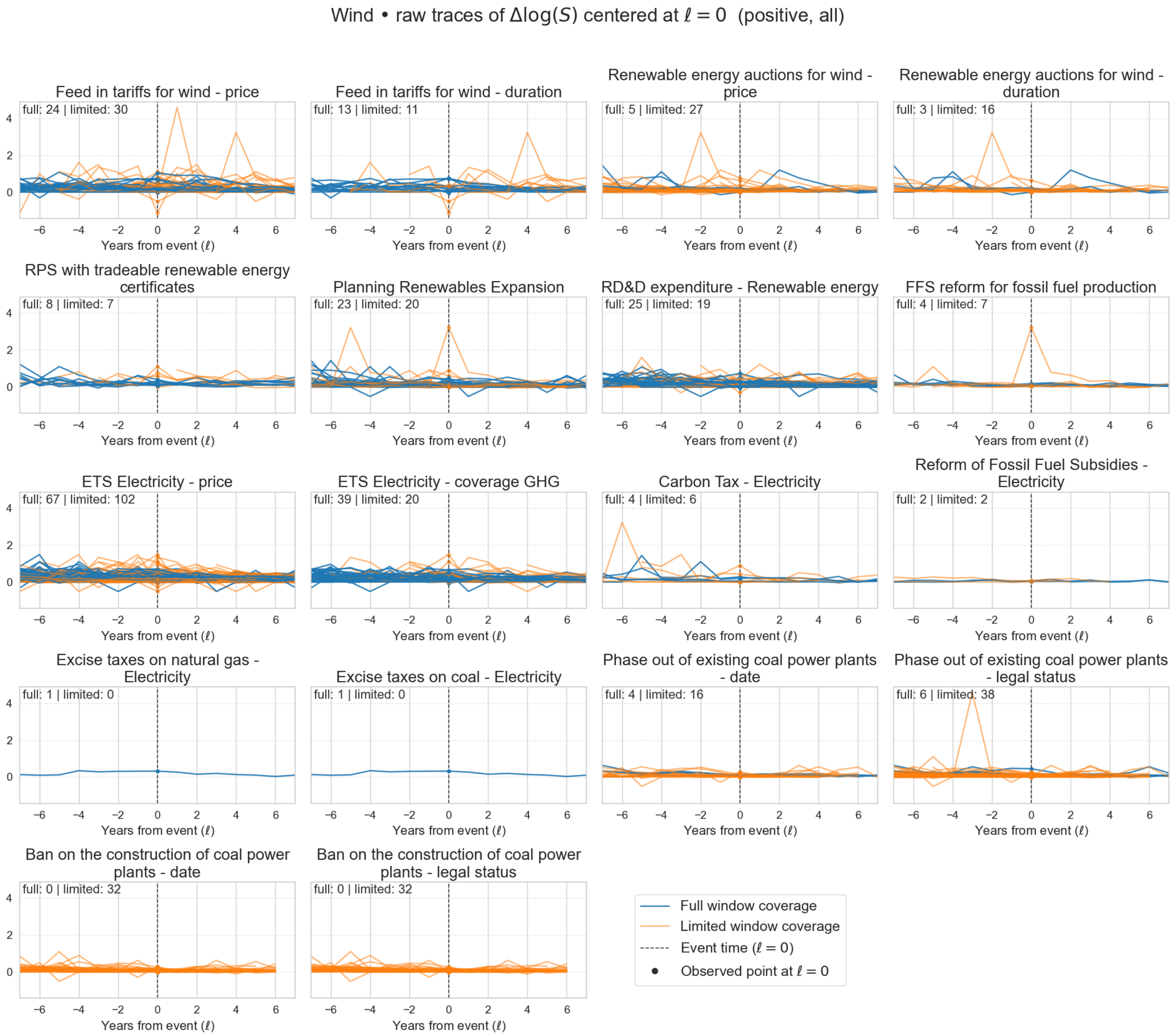
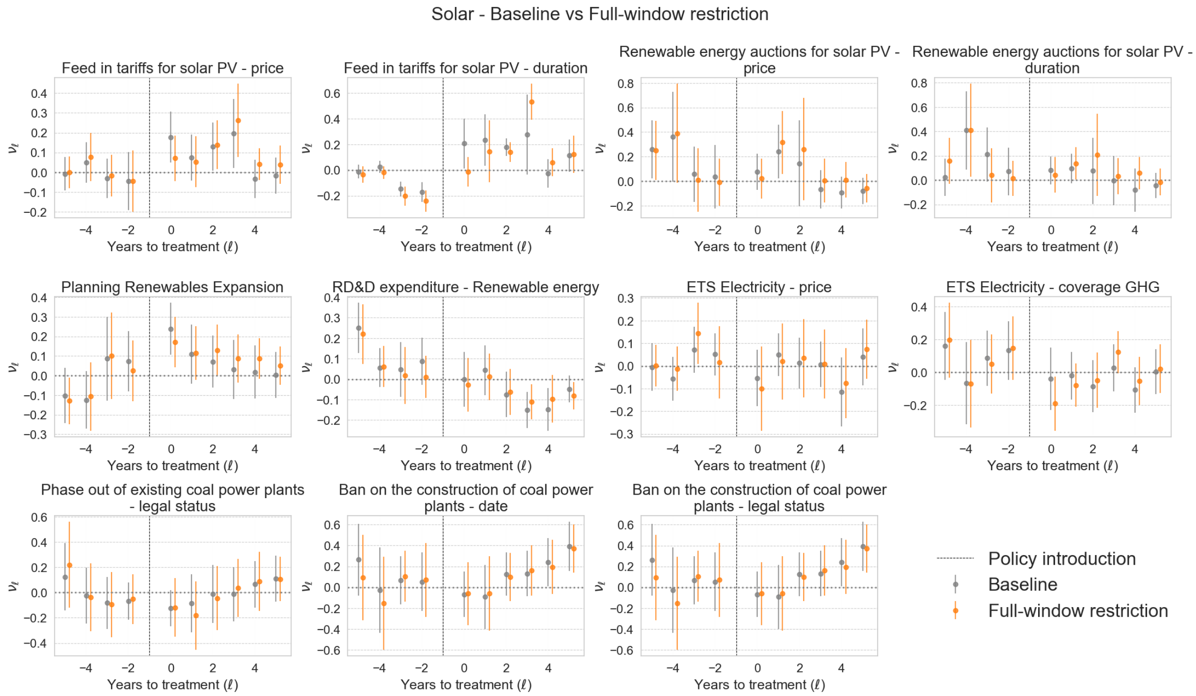
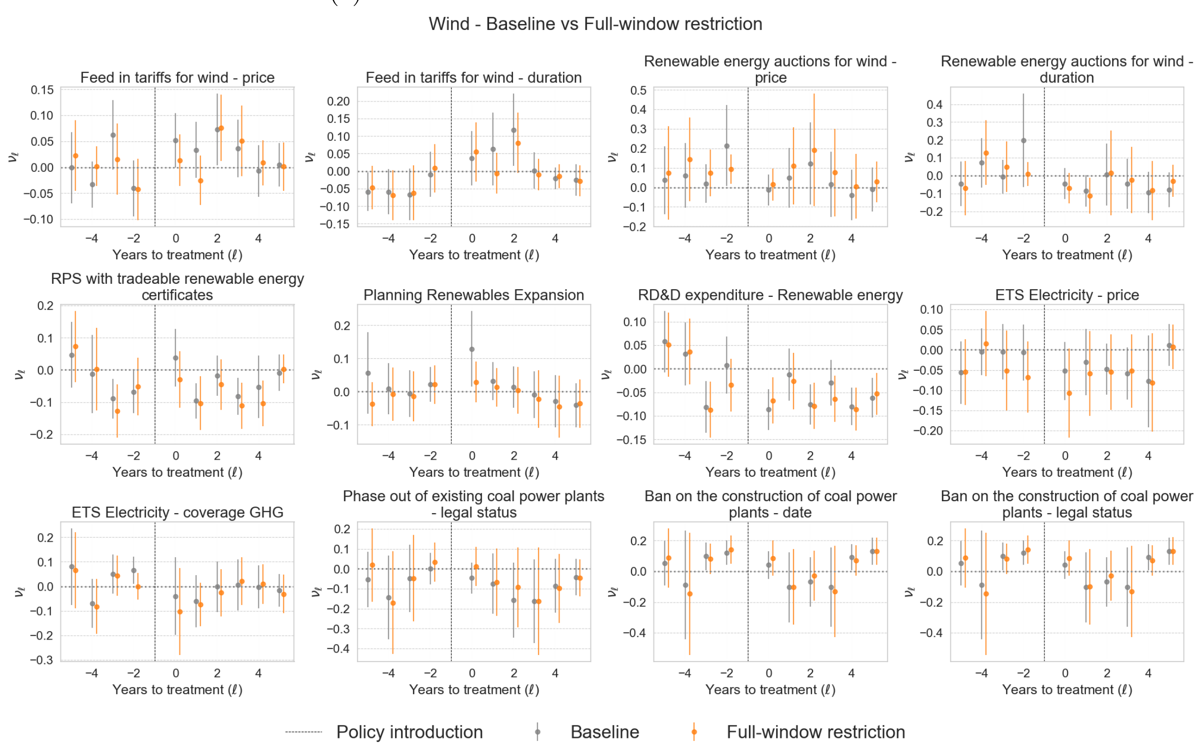


Figure B.7: Plot of raw wind data $\Delta\log(S)$ for positive event windows of ± 7 centered at zero showing countries with full and limited data for each policy

B.5.2 Full window restriction



(a) solar - Baseline vs Full window restriction



(b) wind - Baseline vs Full window restriction

Figure B.8: Comparing the baseline specification to the full-window restriction. Each panel plots the event-study path ν_ℓ by years relative to treatment (ℓ). Dots show the mean ν_ℓ with associated error bands showing the 90% confidence intervals, and the dashed vertical line marks the policy introduction. Grey = Baseline, Orange = test (full-window restriction)

Comparing baseline with full window. The baseline keeps treated episodes that contribute at least one pre period and up to five post years; here we re-estimate the Sun–Abraham IW paths using only events with *complete* coverage of the event window (post the pre-utility filtering), i.e. all $\ell \in [-5, 5]$ are observed. This changes the estimand: instead of an ATT (Average Treatment Effect on the Treated) over all eligible cohorts, we recover an ATT for cohorts that *survive* to $\ell = 5$ with full pre-history. Figure B.8 overlays baseline (grey) and full-window (orange) paths for solar and wind list per-ℓ coefficients.

Potential divergence. Conditioning on future availability (complete post window) systematically tilts the treated sample toward *mature* adopters and later-calendar episodes. Policies that *initiate* diffusion often occur near the left edge of the sample or are followed by data gaps as markets reshape; these episodes are disproportionately *dropped* by the full-window filter. Two mechanical consequences follow: (i) immediate post effects at $\ell = 0$–1 are *attenuated* (strong early ramp-ups are under-represented); (ii) peaks can *shift right* (more weight on cohorts where pipelines materialise later).

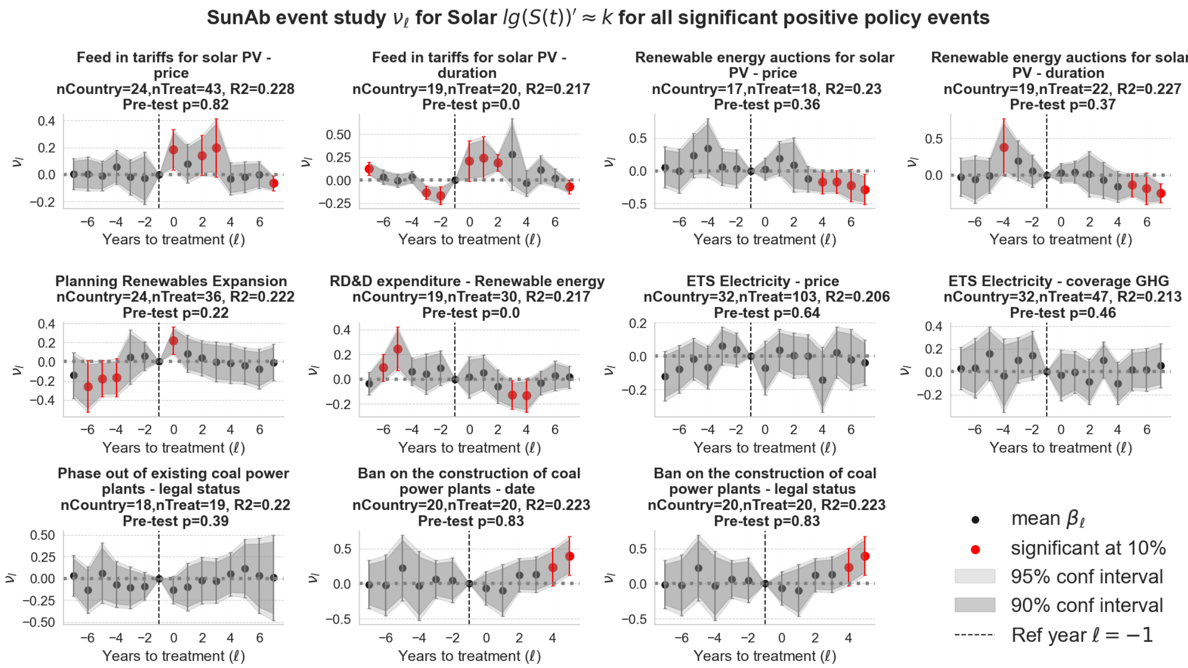
What we see in the data. Consistent with the above, several deployment families show smaller "step-at-zero" under the restriction yet similar or larger mid-lag responses:

- FIT-PR (SOLAR). ν_0 shrinks (baseline 0.18 vs. full-window 0.07), while ν_3 modestly rises (baseline 0.20 vs. 0.26). The shape is preserved but looks more delayed.
- FIT-DUR (SOLAR). The immediate post step largely vanishes ($\nu_0 \approx 0$), but the mid-lag peak grows (baseline $\nu_3 \approx 0.28$ vs. 0.53). This is a textbook case where the restriction *penalises initiators* and emphasises later-stage cohorts.
- AUCTIONS (SOLAR/WIND). Early post coefficients move toward zero and some later lags turn weaker or negative (e.g. solar AUCTION-PR: ν_0 from 0.08 to 0.02; several $\ell \geq 3$ attenuate), consistent with ramp-up heterogeneity being averaged out when early, high-growth episodes are dropped.
- RENEWABLE EXPANSION. Positive post effects remain but are smaller at $\ell = 0$ (solar: $0.24 \rightarrow 0.17$) and decay faster; this aligns with planning impacts arriving through longer pipelines that the restriction partially re-weights. There are still non-zero pre-trends making the interpretating difficult.
- RECS and RD&D. Negative post patterns persist (often slightly stronger at mid-lags), indicating that the sign is not an artefact of incomplete windows.
- COAL BANS/PHASEOUTS. For solar, the late-lag positives remain concentrated at $\ell \in \{4, 5\}$ and look similar across specifications; for wind, near-zero or negative post responses remain near zero, as in the baseline.

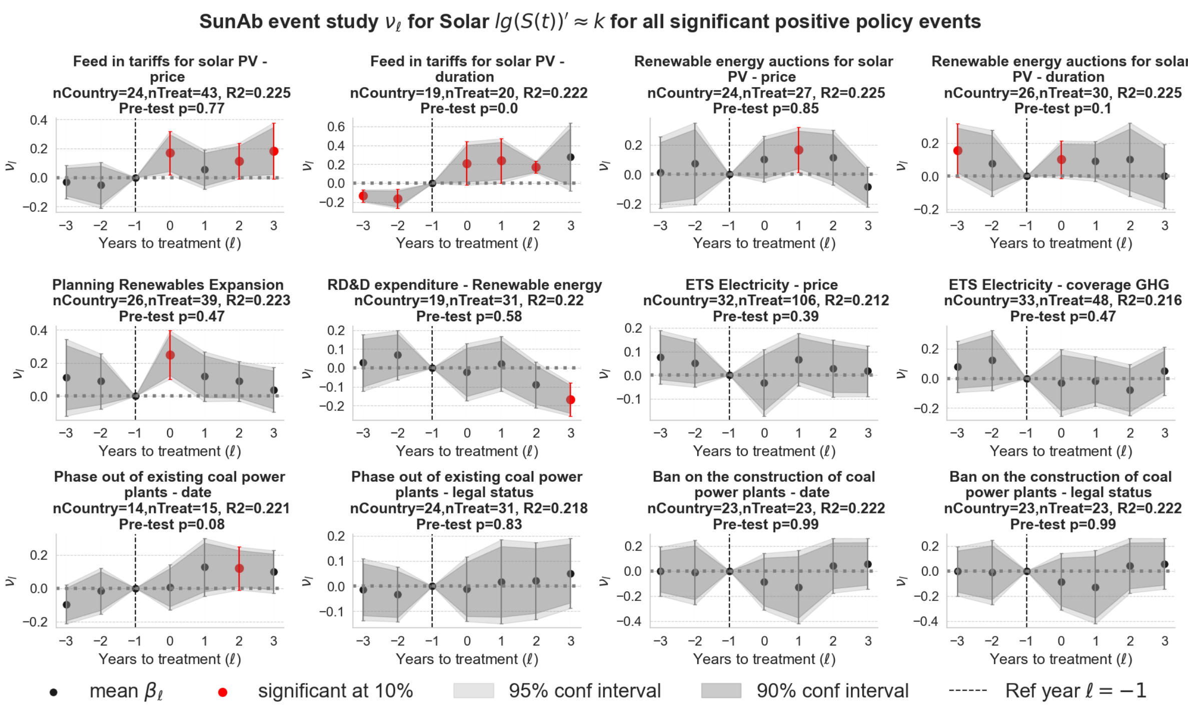
Pre-trends and precision. Because far leads are fully observed, the orange paths (with full-window restriction) display smoother pre-periods and slightly tighter bands at $\ell = -4, -5$; however, families that fail the joint pre-trend in the baseline generally *remain* cautionary under the restriction (the filter changes composition more than it "fixes" trends). At the same time, several ν_0 and ν_1 estimates lose significance-an expected precision trade-off when dropping initiator cohorts that carry large early movements.

Takeaway. The full-window restriction is a useful *stress test* for shape and timing, but it is not neutral: it down-weights the very episodes where policies kick-start diffusion. Where orange and grey agree (sign and profile), the result is robust. Where orange looks smaller at $\ell = 0$ yet similar or larger at $\ell = 2$–3 (e.g. FIT-PR/DUR), we interpret this as selection toward later-stage markets rather than evidence against an immediate policy effect. For headline inference we retain the baseline (risk-set balanced) estimates and use the full-window overlays to document how composition affects the dynamic path $\{\nu_\ell\}_{\ell=-5}^{5}$.

B.5.3 Different window sizes

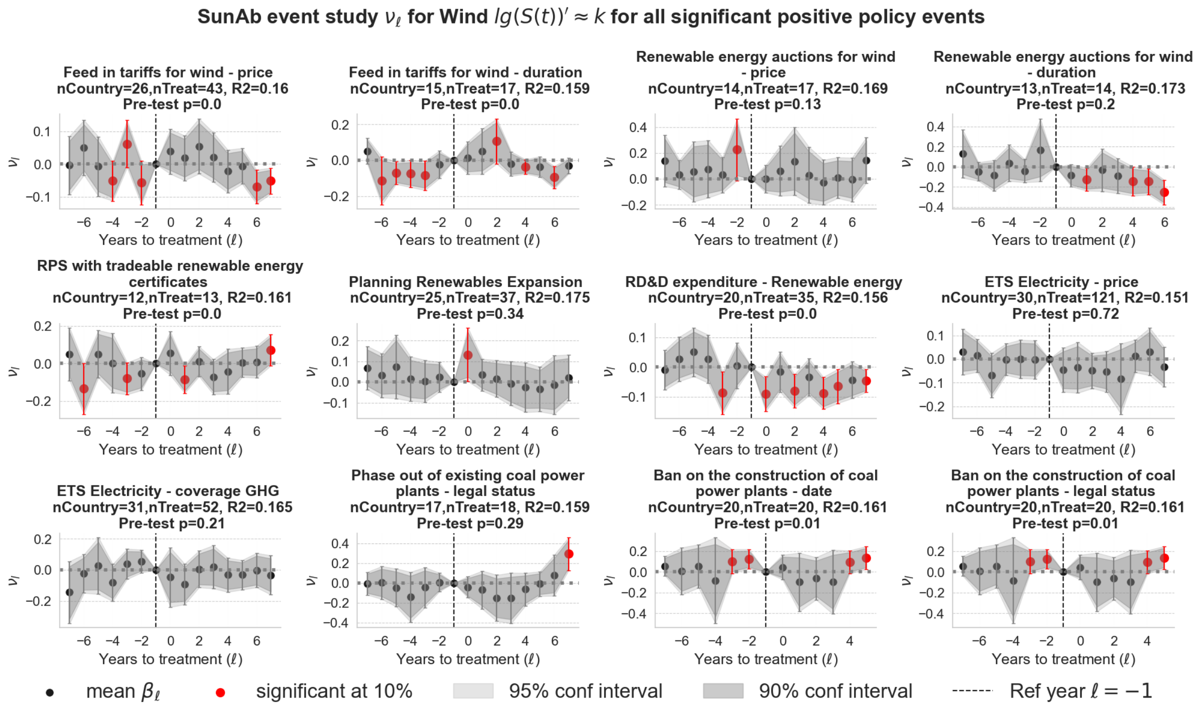


(a) solar, positive events, window ±7

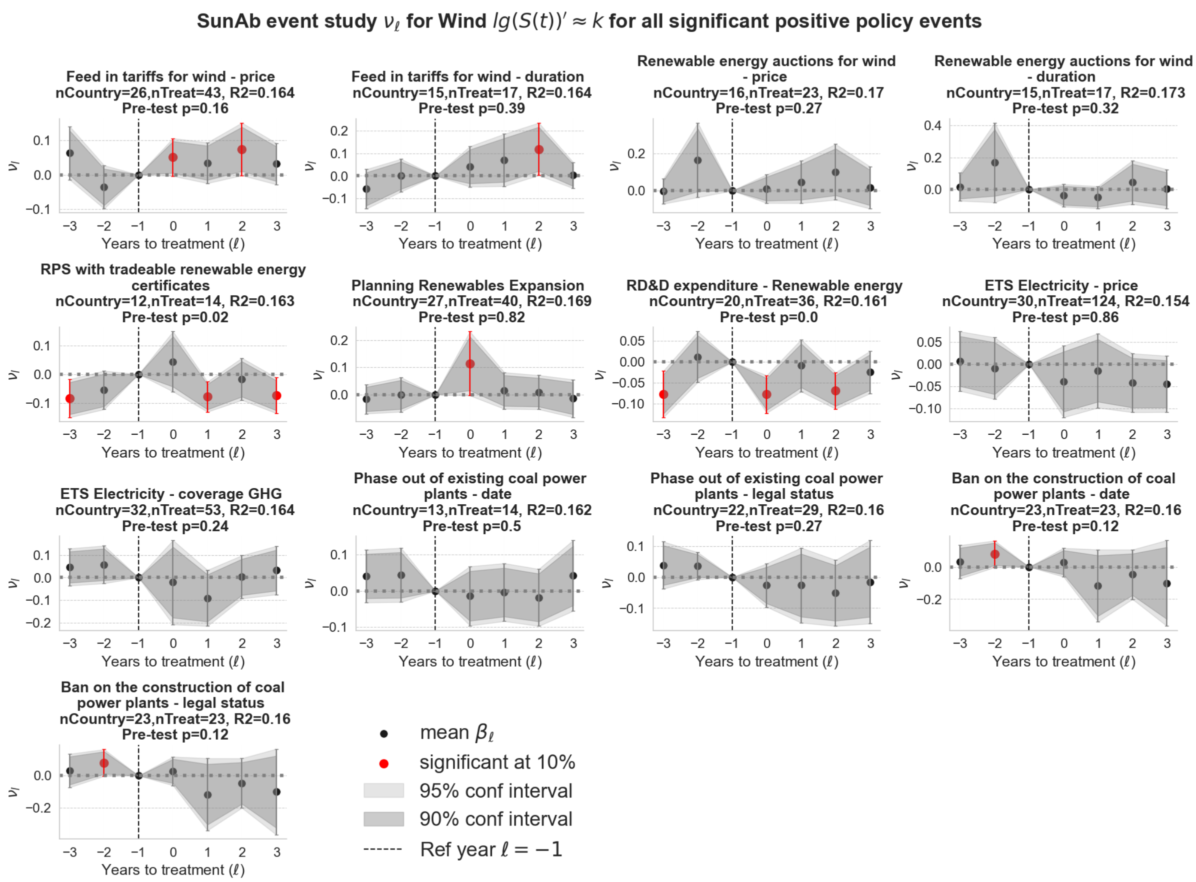


(b) solar, positive events, window ±3

Figure B.9: Robustness checks: event-study estimates for all significant **positive** L4 policy events, shown solar and by expanded pre/post windows (±7 years - top row; ±3 years - bottom row). These appendix plots demonstrate that the qualitative event-study patterns are robust to expanding/contracting the pre/post estimation window.

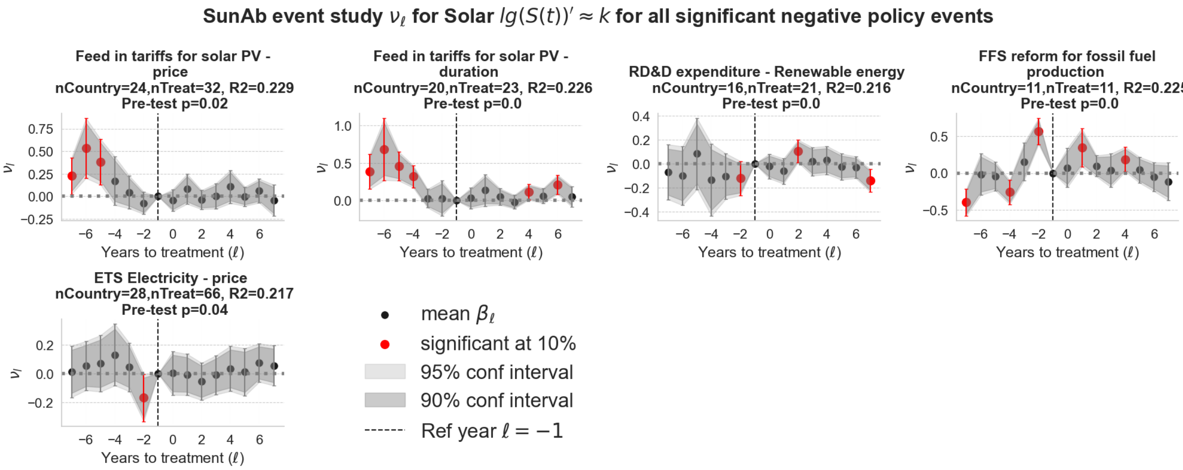


(a) wind, positive events, window ± 7

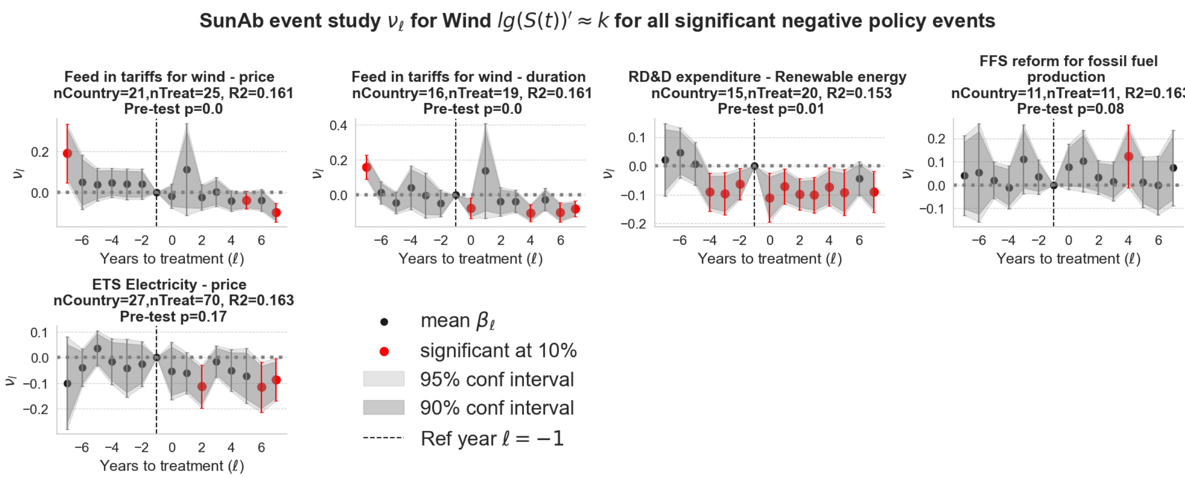


(b) wind, positive events, window ± 3

Figure B.10: Robustness checks: event-study estimates for all significant **positive** L4 policy events, shown for wind and by expanded pre/post windows (± 7 years - top row; ± 3 years - bottom row). These appendix plots demonstrate that the qualitative event-study patterns are robust to expanding/contracting the pre/post estimation window.



(a) solar, negative events, window ±7



(b) wind, negative events, window ±7

Figure B.11: Robustness checks: event-study estimates for all significant **negative** L4 policy events, shown separately by technology (solar - top, wind - bottom). Each panel plots the event-study path ν_ℓ by years relative to treatment ℓ. Dots show the mean ν_ℓ (red dots mark coefficients significant at 10%); shaded bands are the 95% and 90% confidence intervals; and the dashed vertical line marks the policy introduction. Panel headers report the number of treated countries and total treatments (`nCountry`, `nTreat`), model R^2, and the Wald pre-trend test p-value. These appendix plots demonstrate that the qualitative event-study patterns are robust to expanding/contracting the pre/post estimation window.

We probe whether our conclusions hinge on the choice of the event window by re-estimating the Sun–Abraham IW paths with the *same* sample rules but larger (±7) and smaller (±3) windows. Figure B.9–Figure B.11 overlay these variants for solar and wind.

What changes mechanically. With ±7, far leads/lags draw on fewer cohorts, so exposure weights concentrate on early/late adopters; bands widen and occasional far-lead blips appear. With ±3, the tails are truncated, which tightens bands and reduces multiple comparisons, but also mutes delayed responses that materialise after $\ell = 3$.

Core result survives. For deployment and planning families that are central to the main text (e.g., FIT–PR, auctions, renewable-expansion planning), the *sign* and *onset* of the post-policy response are stable across windows: step-ups at $\ell = 0$–1 for explicit price support; gradual increases for planning. Negative

L4 shocks continue to show post-$\ell = 0$ slowdowns, with visible positive pre-movement preceding many downgrades, consistent with policy reacting to overheated markets (Figure B.11).

Interpretation of tails. New pre-trend rejections that emerge only under ± 7 are typically driven by sparse far-lead cohorts; conversely, apparent "loss of effect" under ± 3 reflects the omission of mid/late lags. Two examples matter for reading the appendix: (i) *delayed* solar responses to coal bans (notably $\ell \in \{4, 5\}$) are attenuated in ± 3; (ii) hump-shaped auction profiles with peaks at $\ell = 2$–3 are largely preserved, but late persistence is harder to see.

Why we keep ± 5 as baseline. The ± 5 window balances precision and coverage: it captures the immediate step, the procurement ramp-up, and the delayed tail without over-weighting very sparse far leads/lags. The ± 7 and ± 3 panels corroborate the main patterns rather than overturn them, and they clarify where timing-not sign-drives differences in average post coefficients.

A note on pre-trend screens. For completeness, we apply the joint Wald test to the window-specific pre-sets (baseline: $\{-5, -4, -3, -2\}$; long: $\{-7, \ldots, -2\}$; short: $\{-3, -2\}$). Conclusions are consistent with the baseline screen; where classifications change under ± 7, they trace to far-lead noise rather than a re-timing of the near-lead profile. We therefore privilege the baseline classification in the main text.

B.5.4 Selecting countries/adoption in later stages

Table B.8: Solar, selecting countries with higher adoption (at least 2% of electricity capacity in 2023)

Policy	$\ell = -5$	$\ell = -4$	$\ell = -3$	$\ell = -2$	$\ell = 0$	$\ell = 1$	$\ell = 2$	$\ell = 3$	$\ell = 4$	$\ell = 5$
FIT SOL PR	-0.02** -0.01*	-0.04* 0.05*	-0.06* -0.03*	-0.11 -0.05*	0.13* 0.18*	0.02* 0.07*	0.17* 0.13*	0.18* 0.20	-0.05* -0.03*	-0.01* -0.02*
FIT SOL DUR	-0.10* -0.01**	-0.09* 0.02**	-0.21* -0.15**	-0.30* -0.17**	0.21** 0.21	0.07** 0.23	0.21* 0.18**	0.05 0.28	-0.07* -0.03*	0.15 0.11*
AUCTION SOL PR	-0.09 0.26	-0.25 0.36	-0.10 0.06	0.18 0.04	0.13* 0.08*	0.18* 0.24	-0.13* 0.14	-0.19* -0.07*	-0.07 -0.09*	-0.11* -0.08*
AUCTION SOL DUR	-0.09 0.02*	-0.00 0.41	0.13 0.21	0.17 0.07	0.08* 0.08*	0.15** 0.10*	-0.08* 0.08	-0.10* -0.00	-0.02* -0.08	-0.12** -0.05*
RECS	-0.12 -0.12*	0.01 0.04	-0.10 -0.13	-0.05 -0.01	-0.15* -0.17*	-0.32* -0.22*	0.02* 0.00**	-0.22* -0.22*	-0.27* -0.17*	-0.03* -0.07*
RENEWABLE EXP	-0.03 -0.10*	-0.17 -0.12*	-0.16 0.09	0.02 0.07*	0.19 0.24*	-0.09* 0.11*	-0.10* 0.07*	-0.16 0.03*	-0.13 0.02*	-0.15 0.00*
RDD RES	0.28* 0.25*	0.14* 0.06*	0.10 0.05*	0.05* 0.09*	-0.21* -0.00*	-0.09* 0.04*	-0.10* -0.07*	-0.06* -0.15*	-0.05* -0.15*	-0.08* -0.05**
FFS PRODUCER	0.35 0.32	0.16 -0.04	0.46 0.17	0.38 0.22*	1.05* 0.62	0.38* 0.18	-0.09* -0.10*	0.12* -0.06*	0.09* -0.00*	0.18* 0.04*
ETS E PR	-0.01* -0.01*	-0.13* -0.06*	0.13* 0.07*	0.09* 0.05*	-0.04* -0.05*	0.15* 0.05*	-0.03* 0.01*	0.01* 0.01*	-0.12 -0.11*	-0.08* 0.04*
ETS E GHG	0.18 0.16	-0.08 -0.07	0.05 0.08	0.07 0.13	-0.23 -0.04	-0.08 -0.02*	0.01 -0.09*	0.03 0.03*	-0.12 -0.11*	0.05 0.00*
CARBONTAX E PR	-0.34* -0.33*	-0.33* -0.32*	-0.19* -0.24**	-0.20* -0.09*	0.75* 0.41	0.07* -0.07*	0.17* 0.08	0.23** 0.09	0.05* 0.05	-0.15** -0.08
FFS E	... -0.28*	... -0.08*	... 1.36*	... -0.12*	... 1.27*	... -0.06*	... -0.22*	... 0.10*	... -0.10*	... -0.13*
EXCISETAX E NATGAS	... 0.05*	... -0.11	... -0.16	... -0.43	... -0.53	... 0.08	... -0.01	... -0.11*	... 0.15*	... 0.07*
EXCISETAX E COAL	... 0.04*	... -0.11	... -0.16	... -0.43	... -0.53	... 0.08	... -0.01	... -0.11*	... 0.15*	... 0.07*
PHOUT COAL DATE	0.02 0.11	-0.06 0.12*	-0.07 -0.08*	-0.06 0.06*	0.04* 0.04*	0.17 0.08	0.06* 0.09*	0.10 0.06*	0.01* 0.09*	0.10 0.17*
PHOUT COAL STAT	-0.19 0.12	-0.16 -0.02	-0.12 -0.08	-0.09* -0.07*	-0.05* -0.12*	0.17 -0.09	0.14 -0.01	0.15 -0.01	0.08 0.06	0.06 0.11
BAN COAL DATE	0.29 0.26	0.07 -0.03	0.02 0.07	0.08 0.05	-0.02 -0.07	0.05 -0.09	0.11 0.13	0.15 0.13	0.31 0.24	0.18 0.39
BAN COAL STAT	0.29 0.26	0.07 -0.03	0.02 0.07	0.08 0.05	-0.02 -0.07	0.05 -0.09	0.11 0.13	0.15 0.13	0.31 0.24	0.18 0.39

Notes: Event-time coefficients (Sun–Abraham IW). Coefficients are relative to the year before treatment ($\ell = -1$ omitted).
Stars: * p<0.10, ** p<0.05, *** p<0.01. 95% confidence intervals.
Each cell shows the robustness specification (top) and the baseline (bottom, grey).

Table B.9: wind, Selecting countries with higher adoption (at least 2% of electricity capacity in 2023)

Policy	$\ell = -5$	$\ell = -4$	$\ell = -3$	$\ell = -2$	$\ell = 0$	$\ell = 1$	$\ell = 2$	$\ell = 3$	$\ell = 4$	$\ell = 5$
FIT WIND PR	0.02**	-0.02**	0.08*	-0.04**	0.12**	0.03**	0.08**	0.06**	-0.01**	-0.03**
	-0.00**	-0.03**	0.06**	-0.04**	0.05**	0.03**	0.07**	0.04**	-0.01**	0.00**
FIT WIND DUR	-0.04*	-0.08*	-0.05*	0.02*	0.09*	0.01**	0.04**	-0.01**	-0.02**	-0.06**
	-0.06**	-0.06**	-0.07**	-0.01**	0.04**	0.06*	0.12*	0.00**	-0.02**	-0.03**
AUCTION WIND PR	0.03	-0.03*	-0.02*	0.23	-0.00*	0.09*	0.17	0.06*	0.00*	0.04*
	0.04	0.06	0.02*	0.21	-0.01**	0.05*	0.12	0.02	-0.04*	-0.01*
AUCTION WIND DUR	-0.07*	-0.05*	-0.04*	0.23	-0.02*	-0.06*	0.04	-0.00*	-0.05*	-0.07*
	-0.05*	0.07*	-0.01*	0.20	-0.05*	-0.09*	0.01	-0.05*	-0.10*	-0.08*
RECS	-0.06*	-0.14**	-0.11**	-0.12**	0.06*	-0.14**	-0.03**	-0.09**	-0.05*	-0.02**
	0.05*	-0.01*	-0.09**	-0.07**	0.04*	-0.10**	-0.02**	-0.08**	-0.05*	-0.01**
RENEWABLE EXP	0.04*	-0.05**	-0.04**	-0.01**	0.18*	0.06**	0.03**	0.03**	0.02**	-0.02**
	0.06*	0.01**	-0.01**	0.02**	0.13*	0.03**	0.01**	-0.01**	-0.03**	-0.04**
RDD RES	0.07**	0.05**	-0.06**	0.03**	-0.09**	-0.03**	-0.06**	0.00**	-0.09**	-0.06**
	0.06**	0.03**	-0.08**	0.01**	-0.09**	-0.01**	-0.08**	-0.03**	-0.08**	-0.06**
FFS PRODUCER	0.02**	0.03**	-0.00**	-0.08**	0.67**	0.22**	0.21**	0.16**	0.17**	0.14**
	0.14	-0.03**	0.00**	-0.06**	0.46**	0.14**	0.14**	0.03**	0.08**	0.04**
ETS E PR	-0.07*	-0.04**	-0.01*	-0.05*	-0.04**	-0.07*	-0.10*	-0.09*	-0.13*	-0.03**
	-0.06**	-0.01**	-0.00**	-0.01**	-0.05**	-0.03*	-0.05**	-0.06**	-0.08*	0.01**
ETS E GHG	-0.03	-0.08*	0.05*	0.08**	-0.02	-0.15*	-0.11*	-0.05*	-0.11*	0.00*
	0.08*	-0.07*	0.05**	0.06**	-0.04*	-0.06*	-0.00*	0.01*	-0.00*	-0.02**
CARBONTAX E PR	0.26**	-0.03**	0.13**	-0.15**	0.09**	-0.08**	-0.09**	0.00**	-0.03**	-0.16**
	0.49**	-0.00*	0.09**	0.04	0.07*	-0.04**	-0.05**	-0.01**	-0.01**	-0.07**
FFS E	0.03**	0.00**	0.05**	0.14*	0.06*	0.16*	0.11**	0.22*	0.21*	0.11*
	0.05**	0.03**	0.07*	0.19*	0.11*	0.22*	0.13**	0.21*	0.18*	0.13**
EXCISETAX E NATGAS	-0.10*	0.12**	0.08**	0.07**	0.10*	0.06*	-0.10	0.05**	0.01**	0.02**
	-0.16*	0.14**	0.05*	0.04*	0.13**	0.06*	-0.05*	0.07**	0.00**	0.02**
EXCISETAX E COAL	-0.10*	0.12**	0.08**	0.07**	0.10*	0.06*	-0.10	0.05**	0.01**	0.02**
	-0.16*	0.14**	0.05*	0.04*	0.13**	0.06*	-0.05*	0.07**	0.00**	0.02**
PHOUT COAL DATE	-0.10*	-0.08*	-0.02*	0.01*	-0.03*	0.02*	-0.01*	0.05*	0.06*	0.03**
	-0.04*	-0.04*	0.01*	0.03*	-0.04**	0.00**	-0.06*	0.01*	0.04*	0.03**
PHOUT COAL STAT	-0.19*	-0.23	-0.13	-0.03*	-0.03*	-0.02	-0.08	-0.03*	-0.01*	0.00**
	-0.05*	-0.14	-0.05	0.00**	-0.05**	-0.07*	-0.16	-0.16	-0.08*	-0.04*
BAN COAL DATE	-0.07	-0.28	0.07*	0.12*	0.05*	-0.03	-0.02	0.04*	0.14*	0.16*
	0.05*	-0.09	0.10*	0.12**	0.04*	-0.10	-0.07*	-0.10	0.09*	0.13*
BAN COAL STAT	-0.07	-0.28	0.07*	0.12*	0.05*	-0.03	-0.02	0.04*	0.14*	0.16*
	0.05*	-0.09	0.10*	0.12**	0.04*	-0.10	-0.07*	-0.10	0.09*	0.13*

Notes: Event-time coefficients (Sun–Abraham IW). Coefficients are relative to the year before treatment ($\ell = -1$ omitted).
Stars: * p<0.10, ** p<0.05, *** p<0.01. 95% confidence intervals.
Each cell shows the robustness specification (top) and the baseline (bottom, grey).

Table B.10: solar, Selecting later stages of adoption for each country (after 1% of electricity capacity)

Policy	$\ell=-5$	$\ell=-4$	$\ell=-3$	$\ell=-2$	$\ell=0$	$\ell=1$	$\ell=2$	$\ell=3$	$\ell=4$	$\ell=5$
FIT SOL PR	0.13 -0.01*	0.80 0.05*	-0.06 -0.03*	-0.38 -0.05*	-0.37 0.18*	-0.04 0.07*	-0.32* 0.13*	-0.23* 0.20	-0.25** -0.03*	-0.14** -0.02*
FIT SOL DUR	... -0.01**	0.96 0.02**	0.15 -0.15**	0.11 -0.17**	0.21** 0.21	0.01* 0.23	-0.01 0.18**	-0.13 0.28	-0.04 -0.03*	0.10 0.11*
AUCTION SOL PR	-0.32 0.26	0.06 0.36	-0.05 0.06	-0.06* 0.04	0.06* 0.08*	0.05** 0.24	-0.05* 0.14	-0.13* -0.07*	-0.17* -0.09*	-0.11** -0.08*
AUCTION SOL DUR	-0.26 0.02*	0.05 0.41	-0.02* 0.21	-0.06* 0.07	0.06* 0.08*	0.05** 0.10*	-0.06** 0.08	-0.15* -0.00	-0.16** -0.08	-0.14** -0.05*
RECS	... -0.12*	... 0.04	... -0.13	0.01** -0.01	-0.20 -0.17*	-0.30* -0.22*	-0.39* 0.00**	-0.31** -0.22*	-0.29** -0.17*	-0.26* -0.07*
RENEWABLE EXP	0.03 -0.10*	-0.13* -0.12*	-0.05* 0.09	-0.08** 0.07*	0.06** 0.24*	0.06** 0.11*	0.04** 0.07*	-0.02* 0.03*	0.03** 0.02*	-0.05* 0.00*
RDD RES	... 0.25*	-0.50 0.06*	-0.30 0.05*	-0.23 0.09*	-0.28* -0.00*	-0.32 0.04*	-0.17 -0.07*	-0.18* -0.15*	-0.15** -0.15*	-0.14* -0.05**
FFS PRODUCER	-0.53 0.32	0.10 -0.04	0.07* 0.17	0.17 0.22*	-0.19* 0.62	-0.20** 0.18	-0.26* -0.10*	-0.23** -0.06*	-0.15** -0.00*	-0.12** 0.04*
ETS E PR	0.16 -0.01*	-0.21 -0.06*	-0.12 0.07*	-0.07* 0.05*	0.05** -0.05*	-0.06* 0.05*	-0.22 0.01*	-0.12* 0.01*	-0.23 -0.11*	-0.19** 0.04*
ETS E GHG	... 0.16	... -0.07	... 0.08	-0.21 0.13	-0.12 -0.04	-0.18 -0.02*	-0.10 -0.09*	-0.20* 0.03*	-0.18* -0.11*	-0.10* 0.00*
CARBONTAX E PR	... -0.33*	... -0.32*	-0.05 -0.24**	0.14 -0.09*	-0.11* 0.41	0.06* -0.07*	-0.02* 0.08	-0.03** 0.09	-0.24 0.05	-0.08** -0.08
FFS E	... -0.28*	... -0.08*	... 1.36*	-0.12 -0.12*	-0.09 1.27*	-0.14 -0.06*	-0.24 -0.22*	-0.13* 0.10*	-0.17* -0.10*	-0.12* -0.13*
EXCISETAX E NATGAS	... 0.05*	... -0.11	... -0.16	... -0.43	... -0.53	... 0.08	... -0.01	... -0.11*	... 0.15*	... 0.07*
EXCISETAX E COAL	... 0.04*	... -0.11	... -0.16	... -0.43	... -0.53	... 0.08	... -0.01	... -0.11*	... 0.15*	... 0.07*
PHOUT COAL DATE	0.03 0.11	-0.14 0.12*	-0.06 -0.08*	-0.05* 0.06*	-0.05** 0.04*	-0.01* 0.08	-0.11* 0.09*	-0.10* 0.06*	-0.15* 0.09*	-0.11* 0.17*
PHOUT COAL STAT	-0.11 0.12	-0.06 -0.02	0.02 -0.08	0.03* -0.07*	-0.03** -0.12*	0.06* -0.09	0.04 -0.01	-0.03 -0.01	0.00* 0.06	-0.03 0.11
BAN COAL DATE	-0.22 0.26	0.11 -0.03	0.05 0.07	0.17* 0.05	-0.05* -0.07	-0.01* -0.09	0.12 0.13	0.07* 0.13	0.12* 0.24	0.02 0.39
BAN COAL STAT	-0.22 0.26	0.11 -0.03	0.05 0.07	0.17* 0.05	-0.05* -0.07	-0.01* -0.09	0.12 0.13	0.07* 0.13	0.12* 0.24	0.02 0.39

Notes: Event-time coefficients (Sun–Abraham IW). Coefficients are relative to the year before treatment ($\ell=-1$ omitted).
Stars: * p<0.10, ** p<0.05, *** p<0.01. 95% confidence intervals.
Each cell shows the robustness specification (top) and the baseline (bottom, grey).

Table B.11: wind, Selecting later stages of adoption for each country (after 1% of electricity capacity)

Policy	$\ell=-5$	$\ell=-4$	$\ell=-3$	$\ell=-2$	$\ell=0$	$\ell=1$	$\ell=2$	$\ell=3$	$\ell=4$	$\ell=5$
FIT WIND PR	0.05* -0.00**	0.00** -0.03**	-0.08** 0.06**	-0.00** -0.04**	-0.00** 0.05**	-0.04** 0.03**	-0.02** 0.07**	0.01** 0.04**	-0.05** -0.01**	-0.02** 0.00**
FIT WIND DUR	-0.13** -0.06**	-0.05** -0.06**	-0.01** -0.07**	-0.04** -0.01**	-0.03** 0.04**	-0.10** 0.06*	0.01** 0.12*	0.01** 0.00**	-0.02** -0.02**	-0.04** -0.03**
AUCTION WIND PR	-0.02** 0.04	-0.03** 0.06	0.05** 0.02*	0.04** 0.21	-0.01** -0.01**	0.01** 0.05*	0.07* 0.12	-0.00** 0.02	-0.01** -0.04*	0.05** -0.01*
AUCTION WIND DUR	-0.00** -0.05*	0.00** 0.07*	0.04** -0.01*	0.04** 0.20	0.01** -0.05*	-0.00** -0.09*	0.05** 0.01	0.01** -0.05*	-0.01** -0.10*	-0.00** -0.08*
RECS	-0.04** 0.05*	-0.11** -0.01*	-0.12** -0.09**	-0.15** -0.07**	-0.06** 0.04*	-0.08** -0.10**	-0.07** -0.02**	-0.13** -0.08**	-0.15** -0.05*	-0.02** -0.01**
RENEWABLE EXP	-0.01** 0.06*	-0.06** 0.01**	-0.04** -0.01**	0.02** 0.02**	0.02** 0.13*	0.02** 0.03**	0.02** 0.01**	0.02** -0.01**	0.01** -0.03**	-0.01** -0.04**
RDD RES	0.05* 0.06**	-0.04** 0.03**	-0.02** -0.08**	-0.03** 0.01**	-0.09** -0.09**	-0.06** -0.01**	-0.06** -0.08**	-0.05** -0.03**	-0.06** -0.08**	-0.03** -0.06**
FFS PRODUCER	0.01** 0.14	0.01** -0.03**	0.01** 0.00**	-0.00** -0.06**	0.02** 0.46**	0.08** 0.14**	0.10** 0.14**	0.03** 0.03**	0.07** 0.08**	0.09** 0.04**
ETS E PR	0.04** -0.06**	0.02** -0.01**	-0.00** -0.00**	0.05** -0.01**	-0.01** -0.05**	-0.01** -0.03*	-0.00** -0.05**	-0.05** -0.06**	-0.07** -0.08*	-0.05** 0.01**
ETS E GHG	0.01* 0.08*	-0.06* -0.07*	0.10** 0.05**	0.04** 0.06**	0.07** -0.04*	-0.00** -0.06*	-0.02** -0.00*	0.02** 0.01*	-0.07* -0.00*	-0.00** -0.02**
CARBONTAX E PR	-0.12** 0.49**	-0.07** -0.00*	-0.03** 0.09**	-0.05** 0.04	0.01** 0.07*	-0.03** -0.04**	-0.03* -0.05**	0.04** -0.01**	0.01** -0.01**	-0.07* -0.07**
FFS E	0.00** 0.05**	-0.05** 0.03**	-0.02** 0.07*	0.14** 0.19*	0.06** 0.11*	0.16** 0.22*	0.09** 0.13**	0.11** 0.21*	0.10** 0.18*	0.12** 0.13**
EXCISETAX E NATGAS	-0.11* -0.16*	0.09* 0.14**	0.03** 0.05*	-0.01** 0.04*	0.02** 0.13**	-0.00** 0.06*	-0.04** -0.05*	0.01** 0.07**	-0.06** 0.00**	-0.08** 0.02**
EXCISETAX E COAL	-0.11* -0.16*	0.09* 0.14**	0.03** 0.05*	-0.01** 0.04*	0.02** 0.13**	-0.00** 0.06*	-0.04** -0.05*	0.01** 0.07**	-0.06** 0.00**	-0.08** 0.02**
PHOUT COAL DATE	-0.06** -0.04*	-0.04** -0.04*	0.00** 0.01*	0.01** 0.03*	-0.07** -0.04**	-0.03** 0.00**	-0.06** -0.06*	-0.00* 0.01*	0.01** 0.04*	-0.02** 0.03**
PHOUT COAL STAT	-0.07** -0.05*	-0.04** -0.14	0.02** -0.05	0.00** 0.00**	-0.06** -0.05**	-0.02** -0.07*	-0.07* -0.16	-0.07* -0.16	-0.02** -0.08*	-0.01** -0.04*
BAN COAL DATE	-0.03* 0.05*	0.00** -0.09	0.03** 0.10*	0.05** 0.12**	-0.03* 0.04*	-0.03* -0.10	-0.09* -0.07*	-0.04* -0.10	0.05* 0.09*	0.12* 0.13*
BAN COAL STAT	-0.03* 0.05*	0.00** -0.09	0.03** 0.10*	0.05** 0.12**	-0.03* 0.04*	-0.03* -0.10	-0.09* -0.07*	-0.04* -0.10	0.05* 0.09*	0.12* 0.13*

Notes: Event-time coefficients (Sun–Abraham IW). Coefficients are relative to the year before treatment ($\ell=-1$ omitted).
Stars: * p<0.10, ** p<0.05, *** p<0.01. 95% confidence intervals.
Each cell shows the robustness specification (top) and the baseline (bottom, grey).

We probe stage heterogeneity in two complementary ways. First, we restrict the *cross-section* to countries that had already reached at least 2% of total electricity capacity from the focal technology by 2023 (Table B.8–Table B.9). Second, we restrict *within country* to observations that occur only *after* the country crosses a 1% penetration threshold (Table B.10–Table B.11). Both exercises re-estimate the Sun–Abraham IW paths using the same baseline trimming and fixed effects. Because these filters condition on realised diffusion, they change the composition and exposure weights of cohorts and should be read as *descriptive* heterogeneity rather than new identification.[20]

Solar-deployment effects attenuate in later stages. For FIT-PR and FIT-DUR, post-treatment coefficients are smaller and less precise in the high-adoption country subset and, more starkly, when we only keep post-1% observations within countries. In several lags the signs weaken or turn mixed relative to the baseline (cf. Table B.8 and B.10). This pattern is consistent with FITs being *most powerful in early diffusion*-unlocking pipelines and learning-by-doing-while marginal acceleration diminishes as systems approach higher penetration (grid constraints, siting scarcity, balancing costs). Auctions retain their hump-shaped profile but

[20] Two mechanical consequences follow: (i) effective sample sizes shrink (see `nCountry`/`nTreat`), widening bands; (ii) late-adopter and mature-market cohorts receive greater weight, which can dilute early, policy-timed responses and emphasise saturation dynamics. We reapply the joint pre-trend test under each restriction and report the window-appropriate p-values in the tables.

with muted peaks; Renewable-Expansion planning remains directionally supportive at $\ell = 0$ in the country filter, yet persistence weakens once we condition on being past 1%. RECs continue to exhibit negative post averages and often become *more* negative in mature samples, consistent with certificate schemes replacing rather than augmenting direct deployment support. RD&D-RES stays difficult to interpret (lead movement and weakly negative posts), and we keep it as non-causal context.

Wind-incremental responses survive at the margin, but persistence fades. For FIT-PR (and to a lesser extent FIT-DUR), the immediate post-policy lags ($\ell = 0$–2) can be comparable or slightly larger in the high-adoption subset (Table B.9), suggesting some *incremental* boost even in mature wind markets; however, persistence beyond $\ell \approx 2$ is weaker and precision falls once we condition on post-1% within countries (Table B.11). Auctions remain modestly positive around their usual mid-lags but with smaller peaks; planning effects (RENEWABLE EXPANSION) retain direction at $\ell = 0$ yet diminish quickly thereafter. As in the baseline, RECs and RD&D-RES remain negative on average.

Interpretation. Two forces generate the systematic attenuation: (i) *economics of saturation*-as adoption rises, the binding margin shifts from price support to integration constraints (permitting, land, network upgrades), so the same policy dose yields smaller $\Delta \log S$; and (ii) *statistical composition*-restricting to mature observations de-emphasises early post-treatment lags where dynamic responses are largest and increases variance via smaller effective N. Consequently, when results under these later-stage filters are less significant (or smaller in magnitude) than in the baseline, we read that as evidence that the policy's acceleration effect is *concentrated in the early diffusion phase.* Where point estimates remain positive but lose 90/95% significance while clearing a 1σ bar, we explicitly note the precision trade-off from conditioning on later stages.

Caveat. Because these restrictions are based on realised outcomes, they are not neutral with respect to policy assignment (countries that reach higher penetration may also adopt complementary instruments). We therefore treat the tables as transparent stage-heterogeneity diagnostics: they help localise when instruments are most effective (early vs. mature systems) but do not alter our causal classification from the baseline pre-trend screens.

Takeaway. Deployment price instruments (FIT-PR/DUR) and planning are strongest at low penetration; their marginal effect generally shrinks as systems mature. Certificate trading and wind RD&D-RES remain associated with adverse post responses across stages. This stage pattern aligns with the main text's interpretation: early-stage policies kick-start diffusion; later stages require complementary integration policies to sustain acceleration.

B.5.5 Keeping sub utility/market scale projects

Table B.12: solar, Using all capacity data (including pre-utility/market scale); positive shocks only

Policy	$\ell=-5$	$\ell=-4$	$\ell=-3$	$\ell=-2$	$\ell=0$	$\ell=1$	$\ell=2$	$\ell=3$	$\ell=4$	$\ell=5$
FIT SOL PR	-0.03*	0.01*	-0.04*	-0.11*	0.16*	0.07*	0.08*	0.14	-0.07*	-0.00*
	-0.01*	0.05*	-0.03*	-0.05*	0.18*	0.07*	0.13*	0.20	-0.03*	-0.02*
FIT SOL DUR	-0.01**	0.02**	-0.09*	-0.18**	0.20	0.23	0.15*	0.27	-0.05*	0.10*
	-0.01**	0.02**	-0.15**	-0.17**	0.21	0.23	0.18**	0.28	-0.03*	0.11*
AUCTION SOL PR	0.22	0.29	0.03	0.06	0.09*	0.26	0.15	-0.04*	-0.07*	-0.06*
	0.26	0.36	0.06	0.04	0.08*	0.24	0.14	-0.07*	-0.09*	-0.08*
AUCTION SOL DUR	0.02	0.37	0.20	0.09	0.10*	0.12*	0.10	0.04	-0.05	-0.01*
	0.02*	0.41	0.21	0.07	0.08*	0.10*	0.08	-0.00	-0.08	-0.05*
RECS	-0.08*	-0.02	-0.02	-0.02*	-0.23*	-0.23*	0.03*	-0.24*	0.01	-0.01*
	-0.12*	0.04	-0.13	-0.01	-0.17*	-0.22*	0.00**	-0.22*	-0.17*	-0.07*
RENEWABLE EXP	-0.09*	-0.11*	0.09	0.07*	0.32	0.18	0.09*	0.04*	0.03*	0.01*
	-0.10*	-0.12*	0.09	0.07*	0.24*	0.11*	0.07*	0.03*	0.02*	0.00*
RDD RES	0.23*	0.03**	0.03*	0.06*	-0.02*	0.03*	-0.08*	-0.18*	-0.16*	-0.05**
	0.25*	0.06*	0.05*	0.09*	-0.00*	0.04*	-0.07*	-0.15*	-0.15*	-0.05**
FFS PRODUCER	0.30	-0.06	0.16	0.20*	0.61*	0.17	-0.11*	-0.06*	0.02*	0.05*
	0.32	-0.04	0.17	0.22*	0.62	0.18	-0.10*	-0.06*	-0.00*	0.04*
ETS E PR	-0.01*	-0.05*	0.07*	0.06*	-0.06*	0.07*	0.05*	0.00*	-0.10*	0.04*
	-0.01*	-0.06*	0.07*	0.05*	-0.05*	0.05*	0.01*	0.01*	-0.11*	0.04*
ETS E GHG	0.23	-0.11	0.04	0.14	-0.05	-0.03*	-0.06*	0.02*	-0.10*	0.01*
	0.16	-0.07	0.08	0.13	-0.04	-0.02*	-0.09*	0.03*	-0.11*	0.00*
CARBONTAX E PR	-0.36*	-0.33*	-0.25**	-0.11*	0.40	-0.07*	0.08	0.09	0.06	-0.07
	-0.33*	-0.32*	-0.24**	-0.09*	0.41	-0.07*	0.08	0.09	0.05	-0.08
FFS E	-0.30*	-0.08*	1.31*	-0.12*	1.23*	-0.05*	-0.21*	0.10*	-0.08*	-0.13*
	-0.28*	-0.08*	1.36*	-0.12*	1.27*	-0.06*	-0.22*	0.10*	-0.10*	-0.13*
EXCISETAX E NATGAS	0.09*	-0.15	-0.12	-0.41	-0.49	0.04	0.03	-0.08*	0.19*	0.10*
	0.05*	-0.11	-0.16	-0.43	-0.53	0.08	-0.01	-0.11*	0.15*	0.07*
EXCISETAX E COAL	0.09*	-0.15	-0.12	-0.41	-0.49	0.04	0.03	-0.08*	0.19*	0.10*
	0.04*	-0.11	-0.16	-0.43	-0.53	0.08	-0.01	-0.11*	0.15*	0.07*
PHOUT COAL DATE	0.08	0.10	-0.07*	0.08*	0.05*	0.09	0.08*	0.05*	0.10*	0.18
	0.11	0.12*	-0.08*	0.06*	0.04*	0.08	0.09*	0.06*	0.09*	0.17*
PHOUT COAL STAT	0.13	-0.02	-0.07	-0.05*	-0.12*	-0.09	-0.02	-0.02	0.07	0.11
	0.12	-0.02	-0.08	-0.07*	-0.12*	-0.09	-0.01	-0.01	0.06	0.11
BAN COAL DATE	0.10	-0.02	0.08	0.07	-0.06	-0.09	0.12	0.13	0.25	0.41
	0.26	-0.03	0.07	0.05	-0.07	-0.09	0.13	0.13	0.24	0.39
BAN COAL STAT	0.10	-0.02	0.08	0.07	-0.06	-0.09	0.12	0.13	0.25	0.41
	0.26	-0.03	0.07	0.05	-0.07	-0.09	0.13	0.13	0.24	0.39

Notes: Event-time coefficients (Sun–Abraham IW). Coefficients are relative to the year before treatment ($\ell = -1$ omitted).
Stars: * p<0.10, ** p<0.05, *** p<0.01. 95% confidence intervals.
Each cell shows the robustness specification (top) and the baseline (bottom, grey).

Table B.13: wind, Using all capacity data (including pre-utility/market scale); positive shocks only

Policy	$\ell=-5$	$\ell=-4$	$\ell=-3$	$\ell=-2$	$\ell=0$	$\ell=1$	$\ell=2$	$\ell=3$	$\ell=4$	$\ell=5$
FIT WIND PR	-0.06** -0.00**	-0.07* -0.03**	0.01* 0.06**	-0.08* -0.04**	-0.02** 0.05**	-0.04** 0.03**	0.04* 0.07**	-0.03** 0.04**	0.03* -0.01**	0.02** 0.00**
FIT WIND DUR	-0.13** -0.06**	-0.15** -0.06**	-0.11* -0.07**	0.05 -0.01**	-0.06* 0.04**	-0.00* 0.06*	0.10* 0.12*	-0.06** 0.00**	0.13 -0.02**	0.01** -0.03**
AUCTION WIND PR	0.00 0.04	0.06 0.06	0.02* 0.02*	0.22 0.21	-0.01* -0.01**	0.06 0.05*	0.14 0.12	0.05 0.02	-0.03* -0.04*	0.00* -0.01*
AUCTION WIND DUR	-0.05 -0.05*	0.08* 0.07*	0.01* -0.01*	0.21 0.20	-0.02* -0.05*	-0.05* -0.09*	0.03 0.01	-0.02 -0.05*	-0.06* -0.10*	-0.03* -0.08*
RECS	0.09* 0.05*	0.02* -0.01*	0.08* -0.09**	-0.07** -0.07**	0.22 0.04*	-0.05* -0.10**	-0.02* -0.02**	-0.12** -0.08**	-0.07** -0.05*	-0.03** -0.01**
RENEWABLE EXP	0.12* 0.06*	0.00* 0.01**	0.00* -0.01**	0.10* 0.02**	0.12* 0.13*	0.03* 0.03**	0.01* 0.01**	-0.02* -0.01**	-0.04* -0.03**	-0.07** -0.04**
RDD RES	0.09** 0.06**	0.00** 0.03**	-0.12** -0.08**	0.03** 0.01**	-0.09** -0.09**	-0.04** -0.01**	-0.10** -0.08**	-0.04** -0.03**	-0.08** -0.08**	-0.07** -0.06**
FFS PRODUCER	0.25* 0.14	-0.05** -0.03**	0.04** 0.00**	-0.03** -0.06**	0.52** 0.46**	0.18** 0.14**	0.18** 0.14**	0.07* 0.03**	0.12** 0.08**	0.09** 0.04**
ETS E PR	-0.02** -0.06**	-0.05* -0.01**	0.03* -0.00**	-0.03* -0.01**	-0.05* -0.05**	-0.03* -0.03*	-0.04** -0.05**	-0.13* -0.06**	-0.12* -0.08*	-0.04* 0.01**
ETS E GHG	0.02* 0.08*	-0.03* -0.07*	0.17* 0.05**	0.11** 0.06**	0.01* -0.04*	-0.05* -0.06*	-0.05* -0.00*	0.07* 0.01*	-0.08* -0.00*	-0.01** -0.02**
CARBONTAX E PR	0.48** 0.49**	-0.05* -0.00*	0.05* 0.09**	-0.00 0.04	0.07** 0.07*	-0.05* -0.04**	-0.06* -0.05**	-0.01** -0.01**	-0.01** -0.01**	-0.09* -0.07**
FFS E	0.06** 0.05**	0.07** 0.03**	0.08* 0.07*	0.21* 0.19*	0.13* 0.11*	0.27* 0.22*	0.17** 0.13**	0.23* 0.21*	0.19* 0.18*	0.17* 0.13**
EXCISETAX E NATGAS	-0.23 -0.16*	0.13* 0.14**	0.10* 0.05*	0.01* 0.04*	0.17** 0.13**	0.09** 0.06*	-0.02* -0.05*	0.11** 0.07**	0.04** 0.00**	0.05** 0.02**
EXCISETAX E COAL	-0.23 -0.16*	0.13* 0.14**	0.10* 0.05*	0.01* 0.04*	0.17** 0.13**	0.09** 0.06*	-0.02* -0.05*	0.11** 0.07**	0.04** 0.00**	0.05** 0.02**
PHOUT COAL DATE	-0.00* -0.04*	0.01* -0.04*	0.06* 0.01*	0.09* 0.03*	0.00* -0.04**	0.05* 0.00**	-0.03* -0.06*	0.04* 0.01*	0.08* 0.04*	0.07* 0.03**
PHOUT COAL STAT	-0.06* -0.05*	-0.15 -0.14	-0.05 -0.05	0.00* 0.00**	-0.05** -0.05**	-0.08* -0.07*	-0.17 -0.16	-0.18 -0.16	-0.09* -0.08*	-0.05* -0.04*
BAN COAL DATE	0.10* 0.05*	-0.07 -0.09	0.11* 0.10*	0.13* 0.12**	0.08* 0.04*	-0.10 -0.10	-0.07 -0.07*	-0.10 -0.10	0.11* 0.09*	0.20* 0.13*
BAN COAL STAT	0.10* 0.05*	-0.07 -0.09	0.11* 0.10*	0.13* 0.12**	0.08* 0.04*	-0.10 -0.10	-0.07 -0.07*	-0.10 -0.10	0.11* 0.09*	0.20* 0.13*

Notes: Event-time coefficients (Sun–Abraham IW). Coefficients are relative to the year before treatment ($\ell=-1$ omitted).
Stars: * p<0.10, ** p<0.05, *** p<0.01. 95% confidence intervals.
Each cell shows the robustness specification (top) and the baseline (bottom, grey).

Our main specification trims the outcome to *market-scale* deployment, dropping very small ("sub-utility/market-scale") additions that typically reflect pilots, rooftop PV, or opportunistic early connections. Here we re-estimate the Sun–Abraham IW event paths including *all* capacity data and compare them to the baseline for **positive** shocks (Table B.12–B.13).

Why this matters. Differenced logs of capacity, $\Delta \log S$, are highly sensitive when S_{t-1} is small. Including sub-utility observations introduces (i) *small-base volatility* (a handful of MW can produce large $\Delta \log S$), (ii) *heterogeneous measurement* (early distributed data are uneven across countries/years), and (iii) *policy timing leakage* (pre-policy pilots/rooftop rollouts can show up as positive pre-leads). Each of these tends to widen bands and make joint pre-trend tests more conservative, without necessarily changing the post-treatment *shape*.

Solar-levels shift modestly; rankings persist. With sub-utility data included, FIT-PR and FIT-DUR retain their post-treatment step-ups and timing; point estimates move slightly (e.g., a somewhat larger $\ell=0$ for FIT-PR and Renewable Expansion), while far leads are a touch more volatile (Table B.12, top vs. bottom cells). RECs remain negative on average; auctions keep their familiar "ramp-up" profile with muted differences. Broad instruments (ETS PR/GHG) stay near zero. The net effect is scale/precision, not sign reversal.

Wind-more negative far leads; core patterns intact. For wind, including sub-utility observations often makes distant leads slightly more negative and widens bands, consistent with small-base noise in the earliest years (Table B.13). Immediate post-policy lags for FIT-PR/DUR remain modestly positive/flat with similar timing; auctions and planning continue to show small, sometimes delayed effects. RECs and RD&D-RES remain adverse on average.

Interpretation. Bringing back sub-utility capacity does *not* overturn the main qualitative findings: policy families that accelerate market-scale adoption still do so; families that do not, still do not. The differences we observe are exactly those expected from adding early, granular observations-more variance in leads and slightly attenuated precision in a few post lags. This supports our baseline choice to focus on market-scale deployment for identification: it reduces small-base artefacts, aligns the outcome with the policy margin of interest (utility-scale investment), and yields cleaner pre-trend diagnostics.

Caveat and use-case. Keeping sub-utility data is informative if the research question is *seeding* and very-early diffusion (e.g., rooftop PV programs, pilot auctions). For causal inference on *market-scale* acceleration-the focus of the main text-the trimmed outcome is preferable. We therefore present the all-capacity estimates as a transparency check: they preserve direction and timing across families, with expected volatility in the far leads, and reinforce the conclusions drawn from the market-scale baseline.

B.6 Outliers

Outlier sensitivity matters in event–time designs because annual additions of solar and wind capacity are lumpy, early-stage denominators are small, and a few country–years can dominate least-squares fits. To ensure our headline results are not artifacts of such leverage or heavy tails in the outcome distribution, we implement two complementary diagnostics that leave the research design intact (same treatment timing, cohorts, covariates, and Sun–Abraham imputation-weighted estimator):

(i) Leverage (LOCO). We re-estimate the model repeatedly after dropping one treated country at a time within each policy family and compare the resulting event path $\hat{\nu}_\ell^{(-i)}$ to the baseline $\hat{\nu}_\ell$. This "jackknife by country" isolates whether inference hinges on any single country. We summarise influence at each relative year ℓ with an RMSE metric benchmarked against the baseline standard error, and visualise per-country effects with heatmaps.

(ii) Heavy tails (winsorisation). We re-estimate after symmetrically trimming only the dependent variable y_{ct} at the 1%/99% and 2.5%/97.5% cut-offs. This reduces the influence of extreme outcome realisations (e.g., one-off commissioning spikes or measurement glitches) while preserving treatment composition and regressors.

Taken together, these checks target distinct failure modes-country leverage versus outcome tails. We interpret robustness as preservation of the sign and broad magnitude of post-treatment coefficients (and the absence of systematic creation or loss of significance across adjacent ℓ), while treating isolated ℓ sensitivities or small-N families with added caution. The next subsections report the LOCO stability diagnostics and the winsorised estimates, with tables and figures documenting both the per-ℓ classifications and the underlying influence patterns.

B.6.1 Leave-One-Country-Out (LOCO) analysis

Table B.14: LOCO robustness: per-ℓstability by policy for solar, all positive events

Policy	n_{treat}	ℓ=-5	ℓ=-4	ℓ=-3	ℓ=-2	ℓ0	ℓ=1	ℓ=2	ℓ=3	ℓ=4	ℓ=5
Feed in tariffs for solar PV - price	24	H	H	H	H	H	H	H	H	H	H
Feed in tariffs for solar PV - duration	19	M	M	H	H	H	H	H	H	H	H
Renewable energy auctions for solar PV - price	17	H	H	H	H	H	H	H	H	H	H
Renewable energy auctions for solar PV - duration	19	H	H	H	H	H	H	H	H	H	H
RPS with tradeable renewable energy certificates	10	H	H	H	H	H	H	L	H	H	H
Planning Renewables Expansion	24	H	H	H	H	H	H	H	H	H	H
RD&D expenditure - Renewable energy	19	H	H	H	H	H	H	H	H	H	H
FFS reform for fossil fuel production	7	L	M	M	M	L	L	M	M	H	M
ETS Electricity - price	32	H	H	H	H	H	H	H	H	H	H
ETS Electricity - coverage GHG	32	H	H	H	H	H	H	H	H	H	H
Carbon Tax - Electricity	6	H	M	M	M	M	L	M	M	M	M
Phase out of existing coal power plants - date	10	H	M	H	H	H	H	H	H	H	H
Phase out of existing coal power plants - legal status	18	H	H	H	H	H	H	H	H	H	H
Ban on the construction of coal power plants - date	20	H	H	H	H	H	H	H	H	H	H
Ban on the construction of coal power plants - legal status	20	H	H	H	H	H	H	H	H	H	H

Note: Per-ℓ stability labels are based on the LOCO RMSE at each ℓ relative to the baseline path SE at that ℓ: *H* ($\leq 0.5\times$SE) - *Stable*, *M* ($\leq 1\times$SE) - *Moderately sensitive* , otherwise *L* - *Sensitive*. Number of treated countries is also shown, and the baseline year $\ell = -1$ is omitted.

Table B.15: LOCO robustness: per-ℓ stability by policy for wind, all positive events

Policy	n_{treat}	ℓ=-5	ℓ=-4	ℓ=-3	ℓ=-2	ℓ0	ℓ=1	ℓ=2	ℓ=3	ℓ=4	ℓ=5
Feed in tariffs for wind - price	26	H	H	H	H	H	H	H	H	H	H
Feed in tariffs for wind - duration	15	H	H	H	H	H	H	H	H	H	H
Renewable energy auctions for wind - price	14	H	H	H	H	H	H	H	H	H	H
Renewable energy auctions for wind - duration	13	H	H	H	H	H	H	H	H	H	H
RPS with tradeable renewable energy certificates	12	H	H	H	H	H	H	H	H	H	H
Planning Renewables Expansion	25	H	H	H	H	H	H	H	H	H	H
RD&D expenditure - Renewable energy	20	H	H	H	H	H	H	H	H	H	H
FFS reform for fossil fuel production	6	M	L	M	M	L	L	L	L	L	L
ETS Electricity - price	30	H	H	H	H	H	H	H	H	H	H
ETS Electricity - coverage GHG	31	H	H	H	H	H	H	H	H	H	H
Carbon Tax - Electricity	6	L	M	L	M	L	L	M	L	L	M
Phase out of existing coal power plants - date	10	H	H	H	H	H	H	H	H	H	H
Phase out of existing coal power plants - legal status	17	H	H	H	H	H	H	H	H	H	H
Ban on the construction of coal power plants - date	20	H	H	H	H	H	H	H	H	H	H
Ban on the construction of coal power plants - legal status	20	H	H	H	H	H	H	H	H	H	H

Note: Per-ℓ stability labels are based on the LOCO RMSE at each ℓ relative to the baseline path SE at that ℓ: *H* ($\leq 0.5\times$SE) - *Stable*, *M* ($\leq 1\times$SE) - *Moderately sensitive* , otherwise *L* - *Sensitive*. Number of treated countries is also shown, and the baseline year $\ell = -1$ is omitted.

Figure B.12: Heatmap of influence of removing each countries on ν_ℓ coefficients. Selected main policies shown

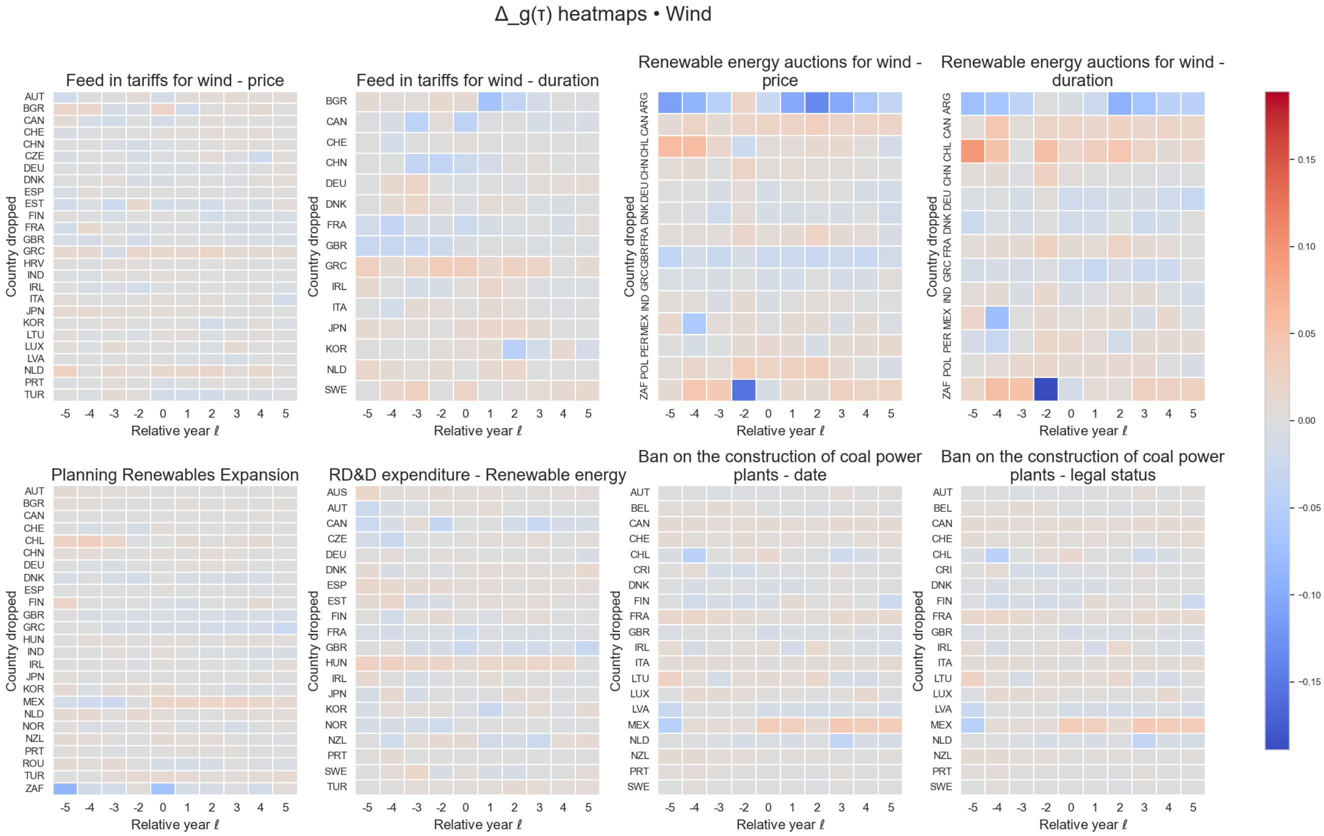


Figure B.13: Heatmap of influence of removing each countries on ν_ℓ coefficients. Selected main policies shown

We assess leverage using a leave-one-country-out (LOCO) diagnostic. For each treated country i within a policy family, we re-estimate the Sun–Abraham IW model on the reduced sample and compare the resulting event path $\hat{\nu}_\ell^{(-i)}$ with the baseline $\hat{\nu}_\ell$. At each ℓ, we summarise dispersion by

$$\mathrm{RMSE}_\ell = \Big(\tfrac{1}{N_{\mathrm{drop}}} \sum_i (\hat{\nu}_\ell^{(-i)} - \hat{\nu}_\ell)^2\Big)^{1/2},$$

and classify stability by benchmarking RMSE_ℓ against the baseline standard error at that ℓ: H ($\leq 0.5 \times \mathrm{SE}$), M ($\leq 1 \times \mathrm{SE}$), else L. Heatmaps visualise per-country influence (Figs. Figure B.12–B.13); Table B.14–B.15 report the per-ℓ stability labels.

Solar. Most Level-4 families exhibit *high* stability across ℓ, including ETS (ELECTRICITY), auctions, planning, and (with some M leads) FEED-IN-TARIFFS (Table B.14). Two nuances stand out. First, for FIT - DURATION, the coefficient at $\ell = 3$ shows offsetting country influence - Denmark pushes it upward and Israel downward - yet the point estimate remains statistically *insignificant* under any single-country deletion; the qualitative conclusion is unchanged (Figure B.12). Second, RECs are broadly stable but show a local sensitivity at $\ell = +2$ (L), consistent with heterogeneity in certificate design and coverage. CARBON TAX (ELECTRICITY) displays M-level stability throughout, reflecting its small-N treated set.

Wind. Stability is likewise high for Feed-in-Tariffs, auctions, planning, ETS, and RECs (Table B.15). The main leverage point concerns Planning: Renewable Expansion at $\ell = 0$, where South Africa exerts notable influence (Figure B.13). Dropping South Africa reduces the baseline significance at $\ell = 0$ but leaves it suggestive at the 1σ level; other lags remain statistically indistinguishable from zero. Small treated sets are predictably fragile: Carbon Tax (Electricity) and Fossil-Fuel Producer Support show L/M labels at several ℓ.

Interpretation. The LOCO diagnostics indicate that the core results do not hinge on any single country. Where sensitivity appears, it aligns with (i) genuinely concentrated policy exposure (e.g., planning episodes with a few early movers), (ii) small effective sample sizes, or (iii) design heterogeneity (e.g., certificates). Importantly, LOCO does not convert an insignificant coefficient into a robustly significant one (or vice versa) for our headline families; it mainly adjusts local precision or the exact ℓ at which effects peak. We therefore read LOCO as a stress test that supports the baseline interpretation while flagging where inference should be read with extra caution (small-N families and isolated ℓ with L labels).

B.6.2 Winsorisation

Table B.16: solar, Dependent variable winsorized at 1%

Policy	$\ell=-5$	$\ell=-4$	$\ell=-3$	$\ell=-2$	$\ell=0$	$\ell=1$	$\ell=2$	$\ell=3$	$\ell=4$	$\ell=5$
FIT SOL PR	-0.00** -0.01*	0.05* 0.05*	-0.03* -0.03*	-0.04* -0.05*	0.18* 0.18*	0.08* 0.07*	0.14* 0.13*	0.18* 0.20	-0.03* -0.03*	-0.02* -0.02*
FIT SOL DUR	-0.02** -0.01**	0.02** 0.02**	-0.15** -0.15**	-0.17** -0.17**	0.20 0.21	0.23 0.23	0.18** 0.18**	0.23 0.28	-0.02* -0.03*	0.11* 0.11*
AUCTION SOL PR	0.26 0.26	0.35 0.36	0.07 0.06	0.01 0.04	0.07* 0.08*	0.22 0.24	0.11 0.14	-0.07* -0.07*	-0.09* -0.09*	-0.08* -0.08*
AUCTION SOL DUR	0.03* 0.02*	0.40 0.41	0.22 0.21	0.05 0.07	0.08* 0.08*	0.10* 0.10*	0.05 0.08	-0.00 -0.00	-0.08 -0.08	-0.05* -0.05*
RECS	-0.12* -0.12*	0.04 0.04	-0.11 -0.13	-0.01 -0.01	-0.17* -0.17*	-0.21* -0.22*	0.00** 0.00**	-0.22** -0.22*	-0.17* -0.17*	-0.07* -0.07*
RENEWABLE EXP	-0.11* -0.10*	-0.12* -0.12*	0.06 0.09	0.07* 0.07*	0.22* 0.24*	0.12* 0.11*	0.08* 0.07*	0.04* 0.03*	0.02* 0.02*	0.01* 0.00*
RDD RES	0.25* 0.25*	0.06* 0.06*	0.07* 0.05*	0.09* 0.09*	0.00* -0.00*	0.05* 0.04*	-0.07* -0.07*	-0.15* -0.15*	-0.14* -0.15*	-0.05** -0.05**
FFS PRODUCER	0.32 0.32	-0.03 -0.04	0.16 0.17	0.22* 0.22*	0.48 0.62	0.18 0.18	-0.10* -0.10*	-0.06* -0.06*	-0.00* -0.00*	0.03* 0.04*
ETS E PR	-0.00* -0.01*	-0.05* -0.06*	0.07* 0.07*	0.05* 0.05*	-0.03* -0.05*	0.04* 0.05*	0.03* 0.01*	-0.00* 0.01*	-0.11* -0.11*	0.04* 0.04*
ETS E GHG	0.17 0.16	-0.06 -0.07	0.10 0.08	0.13 0.13	-0.03 -0.04	-0.03* -0.02*	-0.08* -0.09*	0.03* 0.03*	-0.11* -0.11*	0.01* 0.00*
CARBONTAX E PR	-0.32* -0.33*	-0.31* -0.32*	-0.23** -0.24**	-0.09* -0.09*	0.40 0.41	-0.06* -0.07*	0.08 0.08	0.09 0.09	0.05 0.05	-0.08 -0.08
FFS E	-0.27* -0.28*	-0.08* -0.08*	1.07* 1.36*	-0.11* -0.12*	0.99* 1.27*	-0.06* -0.06*	-0.22* -0.22*	0.09* 0.10*	-0.10* -0.10*	-0.12* -0.13*
EXCISETAX E NATGAS	0.04* 0.05*	-0.11 -0.11	-0.15 -0.16	-0.43 -0.43	-0.53* -0.53	0.10 0.08	0.02 -0.01	-0.11* -0.11*	0.15* 0.15*	0.07* 0.07*
EXCISETAX E COAL	0.04* 0.04*	-0.11 -0.11	-0.15 -0.16	-0.43 -0.43	-0.53* -0.53	0.10 0.08	0.02 -0.01	-0.11* -0.11*	0.15* 0.15*	0.07* 0.07*
PHOUT COAL DATE	0.11 0.11	0.12* 0.12*	-0.08* -0.08*	0.06* 0.06*	0.04* 0.04*	0.08 0.08	0.09* 0.09*	0.06* 0.06*	0.09* 0.09*	0.17* 0.17*
PHOUT COAL STAT	0.10 0.12	-0.01 -0.02	-0.08 -0.08	-0.07* -0.07*	-0.12* -0.12*	-0.07 -0.09	-0.00 -0.01	-0.01 -0.01	0.06 0.06	0.11 0.11
BAN COAL DATE	0.24 0.26	0.01 -0.03	0.06 0.07	0.04 0.05	-0.07 -0.07	-0.07 -0.09	0.12 0.13	0.13 0.13	0.24 0.24	0.39 0.39
BAN COAL STAT	0.24 0.26	0.01 -0.03	0.06 0.07	0.04 0.05	-0.07 -0.07	-0.07 -0.09	0.12 0.13	0.13 0.13	0.24 0.24	0.39 0.39

Notes: Event-time coefficients (Sun–Abraham IW). Coefficients are relative to the year before treatment ($\ell = -1$ omitted).
Stars: * p<0.10, ** p<0.05, *** p<0.01. 95% confidence intervals.
Each cell shows the robustness specification (top) and the baseline (bottom, grey).

Table B.17: wind, Dependent variable winsorized at 1%

Policy	$\ell=-5$	$\ell=-4$	$\ell=-3$	$\ell=-2$	$\ell=0$	$\ell=1$	$\ell=2$	$\ell=3$	$\ell=4$	$\ell=5$
FIT WIND PR	0.00** -0.00**	-0.03** -0.03**	0.06** 0.06**	-0.04** -0.04**	0.05** 0.05**	0.04** 0.03**	0.07** 0.07**	0.04** 0.04**	-0.01** -0.01**	0.01** 0.00**
FIT WIND DUR	-0.06** -0.06**	-0.06** -0.06**	-0.07** -0.07**	-0.01** -0.01**	0.04** 0.04**	0.06* 0.06*	0.12* 0.12*	0.01** 0.00**	-0.02** -0.02**	-0.03** -0.03**
AUCTION WIND PR	0.03 0.04	0.07 0.06	0.02* 0.02*	0.11** 0.21	-0.00** -0.01**	0.06* 0.05*	0.14 0.12	0.03* 0.02	-0.02* -0.04*	0.01* -0.01*
AUCTION WIND DUR	-0.06* -0.05*	0.07* 0.07*	-0.01* -0.01*	0.06* 0.20	-0.04* -0.05*	-0.08* -0.09*	0.02* 0.01	-0.03* -0.05*	-0.08* -0.10*	-0.07* -0.08*
RECS	0.05* 0.05*	-0.01* -0.01*	-0.10** -0.09**	-0.07** -0.07**	0.04* 0.04*	-0.08** -0.10**	-0.02** -0.02**	-0.07** -0.08**	-0.05* -0.05*	-0.01** -0.01**
RENEWABLE EXP	0.01** 0.06*	0.01** 0.01**	-0.01** -0.01**	0.02** 0.02**	0.08** 0.13*	0.04** 0.03**	0.03** 0.01**	0.01** -0.01**	-0.02** -0.03**	-0.03** -0.04**
RDD RES	0.06** 0.06**	0.03** 0.03**	-0.07** -0.08**	0.00** 0.01**	-0.08** -0.09**	-0.01** -0.01**	-0.07** -0.08**	-0.03** -0.03**	-0.07** -0.08**	-0.06** -0.06**
FFS PRODUCER	0.15 0.14	0.00** -0.03**	0.01** 0.00**	-0.04** -0.06**	0.19** 0.46**	0.14** 0.14**	0.15** 0.14**	0.03** 0.03**	0.08** 0.08**	0.05** 0.04**
ETS E PR	-0.04** -0.06**	-0.01** -0.01**	0.00** -0.00**	-0.01** -0.01**	-0.05** -0.05**	-0.03** -0.03*	-0.02** -0.05**	-0.06** -0.06**	-0.05** -0.08*	0.00** 0.01**
ETS E GHG	0.06* 0.08*	-0.06* -0.07*	0.07** 0.05**	0.06** 0.06**	0.00* -0.04*	-0.07* -0.06*	-0.01* -0.00*	-0.01* 0.01*	-0.01* -0.00*	-0.02** -0.02**
CARBONTAX E PR	0.45** 0.49**	0.00* -0.00*	0.06** 0.09**	0.04 0.04	0.07** 0.07*	-0.04** -0.04**	-0.05** -0.05**	-0.01** -0.01**	-0.01** -0.01**	-0.06* -0.07**
FFS E	0.06** 0.05**	0.02** 0.03**	0.06** 0.07*	0.21** 0.19*	0.12** 0.11*	0.22** 0.22*	0.13** 0.13**	0.20* 0.21*	0.16** 0.18*	0.13** 0.13**
EXCISETAX E NATGAS	-0.14* -0.16*	0.14** 0.14**	0.05** 0.05*	0.05* 0.04*	0.13** 0.13**	0.06* 0.06*	-0.01** -0.05*	0.07** 0.07**	-0.00** 0.00**	0.02** 0.02**
EXCISETAX E COAL	-0.14* -0.16*	0.14** 0.14**	0.05** 0.05*	0.05* 0.04*	0.13** 0.13**	0.06* 0.06*	-0.01** -0.05*	0.07** 0.07**	-0.00** 0.00**	0.02** 0.02**
PHOUT COAL DATE	-0.02* -0.04*	-0.02* -0.04*	0.02* 0.01*	0.03* 0.03*	-0.04** -0.04**	-0.00** 0.00**	-0.05* -0.06*	0.01* 0.01*	0.04* 0.04*	0.02** 0.03**
PHOUT COAL STAT	-0.03* -0.05*	-0.07* -0.14	-0.01* -0.05	-0.00** 0.00**	-0.05** -0.05**	-0.08* -0.07*	-0.15 -0.16	-0.14 -0.16	-0.08* -0.08*	-0.04* -0.04*
BAN COAL DATE	0.07* 0.05*	0.03 -0.09	0.08* 0.10*	0.10* 0.12**	0.03* 0.04*	-0.10 -0.10	-0.07* -0.07*	-0.05 -0.10	0.07* 0.09*	0.13* 0.13*
BAN COAL STAT	0.07* 0.05*	0.03 -0.09	0.08* 0.10*	0.10* 0.12**	0.03* 0.04*	-0.10 -0.10	-0.07* -0.07*	-0.05 -0.10	0.07* 0.09*	0.13* 0.13*

Notes: Event-time coefficients (Sun–Abraham IW). Coefficients are relative to the year before treatment ($\ell = -1$ omitted).
Stars: * p<0.10, ** p<0.05, *** p<0.01. 95% confidence intervals.
Each cell shows the robustness specification (top) and the baseline (bottom, grey).

Table B.18: solar, Dependent variable winsorized at 2.5%

Policy	$\ell=-5$	$\ell=-4$	$\ell=-3$	$\ell=-2$	$\ell=0$	$\ell=1$	$\ell=2$	$\ell=3$	$\ell=4$	$\ell=5$
FIT SOL PR	-0.01** -0.01*	0.04* 0.05*	-0.02* -0.03*	-0.03* -0.05*	0.16* 0.18*	0.07* 0.07*	0.16* 0.13*	0.18* 0.20	-0.00* -0.03*	0.00** -0.02*
FIT SOL DUR	-0.01** -0.01**	0.03** 0.02**	-0.15** -0.15**	-0.17** -0.17**	0.17* 0.21	0.17* 0.23	0.19** 0.18**	0.19 0.28	0.01* -0.03*	0.13* 0.11*
AUCTION SOL PR	0.26 0.26	0.29 0.36	0.03 0.06	-0.02 0.04	0.03* 0.08*	0.15* 0.24	0.05 0.14	-0.08* -0.07*	-0.11* -0.09*	-0.10* -0.08*
AUCTION SOL DUR	0.04* 0.02*	0.32 0.41	0.14 0.21	0.02* 0.07	0.05* 0.08*	0.08* 0.10*	0.01 0.08	-0.00 -0.00	-0.09 -0.08	-0.06* -0.05*
RECS	-0.11* -0.12*	0.05 0.04	-0.10 -0.13	-0.00 -0.01	-0.15* -0.17*	-0.20** -0.22*	-0.03** 0.00**	-0.21** -0.22*	-0.16* -0.17*	-0.06* -0.07*
RENEWABLE EXP	-0.11* -0.10*	-0.10* -0.12*	0.03* 0.09	0.07* 0.07*	0.19* 0.24*	0.14* 0.11*	0.09* 0.07*	0.04* 0.03*	0.03* 0.02*	0.01* 0.00*
RDD RES	0.23* 0.25*	0.06* 0.06*	0.06* 0.05*	0.08* 0.09*	0.00* -0.00*	0.03* 0.04*	-0.05* -0.07*	-0.14** -0.15*	-0.13* -0.15*	-0.03** -0.05**
FFS PRODUCER	0.27 0.32	0.02 -0.04	0.21 0.17	0.25* 0.22*	0.31* 0.62	0.21* 0.18	-0.10* -0.10*	-0.06* -0.06*	-0.01* -0.00*	0.03* 0.04*
ETS E PR	0.01* -0.01*	-0.05* -0.06*	0.07* 0.07*	0.05** 0.05*	-0.00* -0.05*	0.04* 0.05*	0.05* 0.01*	0.00* 0.01*	-0.05* -0.11*	0.04* 0.04*
ETS E GHG	0.14 0.16	-0.05 -0.07	0.12* 0.08	0.13* 0.13	-0.03* -0.04	-0.03* -0.02*	-0.06* -0.09*	0.03* 0.03*	-0.10* -0.11*	0.03* 0.00*
CARBONTAX E PR	-0.30* -0.33*	-0.29** -0.32*	-0.21** -0.24**	-0.07** -0.09*	0.31 0.41	-0.04* -0.07*	0.09 0.08	0.10 0.09	0.06 0.05	-0.08 -0.08
FFS E	-0.26* -0.28*	-0.06* -0.08*	0.71* 1.36*	-0.08* -0.12*	0.63* 1.27*	-0.05* -0.06*	-0.22* -0.22*	0.07* 0.10*	-0.11* -0.10*	-0.13* -0.13*
EXCISETAX E NATGAS	0.04* 0.05*	-0.10* -0.11	-0.14 -0.16	-0.40 -0.43	-0.50* -0.53	0.12* 0.08	0.06* -0.01	-0.09* -0.11*	0.17* 0.15*	0.07* 0.07*
EXCISETAX E COAL	0.04* 0.04*	-0.10* -0.11	-0.14 -0.16	-0.40 -0.43	-0.50* -0.53	0.12* 0.08	0.06* -0.01	-0.09* -0.11*	0.17* 0.15*	0.07* 0.07*
PHOUT COAL DATE	0.12 0.11	0.09* 0.12*	-0.07* -0.08*	0.05* 0.06*	0.02* 0.04*	0.06* 0.08	0.07* 0.09*	0.03* 0.06*	0.06* 0.09*	0.15* 0.17*
PHOUT COAL STAT	0.07 0.12	-0.02* -0.02	-0.07 -0.08	-0.07* -0.07*	-0.12* -0.12*	-0.07 -0.09	-0.01 -0.01	-0.04 -0.01	0.04 0.06	0.11 0.11
BAN COAL DATE	0.16 0.26	0.03 -0.03	0.03 0.07	0.05 0.05	-0.09 -0.07	-0.06 -0.09	0.09 0.13	0.10 0.13	0.19 0.24	0.34 0.39
BAN COAL STAT	0.16 0.26	0.03 -0.03	0.03 0.07	0.05 0.05	-0.09 -0.07	-0.06 -0.09	0.09 0.13	0.10 0.13	0.19 0.24	0.34 0.39

Notes: Event-time coefficients (Sun–Abraham IW). Coefficients are relative to the year before treatment ($\ell = -1$ omitted). Stars: * p<0.10, ** p<0.05, *** p<0.01. 95% confidence intervals.
Each cell shows the robustness specification (top) and the baseline (bottom, grey).

Table B.19: wind, Dependent variable winsorized at 2.5%

Policy	$\ell = -5$	$\ell = -4$	$\ell = -3$	$\ell = -2$	$\ell = 0$	$\ell = 1$	$\ell = 2$	$\ell = 3$	$\ell = 4$	$\ell = 5$
FIT WIND PR	-0.00** -0.00**	-0.03** -0.03**	0.04** 0.06**	-0.03** -0.04**	0.05** 0.05**	0.03** 0.03**	0.06** 0.07**	0.04** 0.04**	-0.01** -0.01**	0.01** 0.00**
FIT WIND DUR	-0.06** -0.06**	-0.05** -0.06**	-0.06** -0.07**	-0.01** -0.01**	0.05** 0.04**	0.04** 0.06*	0.10* 0.12*	0.01** 0.00**	-0.02** -0.02**	-0.03** -0.03**
AUCTION WIND PR	0.03 0.04	0.06* 0.06	0.02* 0.02*	0.09** 0.21	0.00** -0.01**	0.05* 0.05*	0.14 0.12	0.04* 0.02	-0.01* -0.04*	0.02* -0.01*
AUCTION WIND DUR	-0.06* -0.05*	0.06* 0.07*	-0.00* -0.01*	0.04* 0.20	-0.03* -0.05*	-0.07** -0.09*	0.02* 0.01	-0.02* -0.05*	-0.08* -0.10*	-0.06* -0.08*
RECS	0.05* 0.05*	-0.00* -0.01*	-0.09** -0.09**	-0.07** -0.07**	0.03* 0.04*	-0.08** -0.10**	-0.01** -0.02**	-0.06** -0.08**	-0.05* -0.05*	-0.01** -0.01**
RENEWABLE EXP	-0.00** 0.06*	0.01** 0.01**	-0.01** -0.01**	0.02** 0.02**	0.08** 0.13*	0.04** 0.03**	0.04** 0.01**	0.02** -0.01**	-0.00** -0.03**	-0.02** -0.04**
RDD RES	0.05** 0.06**	0.04** 0.03**	-0.06** -0.08**	-0.00** 0.01**	-0.07** -0.09**	-0.02** -0.01**	-0.07** -0.08**	-0.03** -0.03**	-0.07** -0.08**	-0.06** -0.06**
FFS PRODUCER	0.13 0.14	0.01** -0.03**	0.02** 0.00**	-0.03** -0.06**	0.13** 0.46**	0.15** 0.14**	0.15** 0.14**	0.04** 0.03**	0.09** 0.08**	0.05** 0.04**
ETS E PR	-0.03** -0.06**	-0.01** -0.01**	0.00** -0.00**	-0.00** -0.01**	-0.03** -0.05**	-0.02** -0.03*	-0.02** -0.05**	-0.06** -0.06**	-0.04** -0.08*	-0.00** 0.01**
ETS E GHG	0.05* 0.08*	-0.06** -0.07*	0.08** 0.05**	0.05** 0.06**	0.01** -0.04*	-0.08* -0.06*	-0.02* -0.00*	-0.01* 0.01*	-0.02** -0.00*	-0.01** -0.02**
CARBONTAX E PR	0.33** 0.49**	0.01* -0.00*	0.02** 0.09**	0.02 0.04	0.08** 0.07*	-0.04** -0.04**	-0.05** -0.05**	0.00** -0.01**	0.00** -0.01**	-0.05* -0.07**
FFS E	0.05** 0.05**	0.02** 0.03**	0.05** 0.07*	0.20** 0.19*	0.11** 0.11*	0.20** 0.22*	0.12** 0.13**	0.18** 0.21*	0.15** 0.18*	0.14** 0.13**
EXCISETAX E NATGAS	-0.13* -0.16*	0.13** 0.14**	0.06** 0.05*	0.05** 0.04*	0.13** 0.13**	0.06** 0.06*	-0.00** -0.05*	0.06** 0.07**	-0.01** 0.00**	0.01** 0.02**
EXCISETAX E COAL	-0.13* -0.16*	0.13** 0.14**	0.06** 0.05*	0.05** 0.04*	0.13** 0.13**	0.06** 0.06*	-0.00** -0.05*	0.06** 0.07**	-0.01** 0.00**	0.01** 0.02**
PHOUT COAL DATE	-0.01* -0.04*	-0.02** -0.04*	0.02* 0.01*	0.03** 0.03*	-0.04** -0.04**	-0.00** 0.00**	-0.04** -0.06*	0.02* 0.01*	0.04* 0.04*	0.01** 0.03**
PHOUT COAL STAT	-0.03* -0.05*	-0.06* -0.14	-0.00* -0.05	0.00** 0.00**	-0.04** -0.05**	-0.05* -0.07*	-0.12* -0.16	-0.11* -0.16	-0.06* -0.08*	-0.04** -0.04*
BAN COAL DATE	0.06* 0.05*	0.05* -0.09	0.07* 0.10*	0.09* 0.12**	0.03** 0.04*	-0.07 -0.10	-0.07* -0.07*	-0.03* -0.10	0.07* 0.09*	0.13** 0.13*
BAN COAL STAT	0.06* 0.05*	0.05* -0.09	0.07* 0.10*	0.09* 0.12**	0.03** 0.04*	-0.07 -0.10	-0.07* -0.07*	-0.03* -0.10	0.07* 0.09*	0.13** 0.13*

Notes: Event-time coefficients (Sun–Abraham IW). Coefficients are relative to the year before treatment ($\ell = -1$ omitted).
Stars: * p<0.10, ** p<0.05, *** p<0.01. 95% confidence intervals.
Each cell shows the robustness specification (top) and the baseline (bottom, grey).

To check that our results are not driven by a handful of extreme year–country observations (e.g., one-off commissioning spikes or data glitches), we re-estimate the main Sun–Abraham specification after trimming the dependent variable's tails. Specifically, we winsorise y_{ct} at two symmetric cut-offs: 1%/99% and 2.5%/97.5%, leaving all regressors and policy indicators unchanged. This preserves treatment timing and composition while reducing leverage from outliers in y_{ct}.

Findings. Across both technologies and both cut-offs, the qualitative patterns are unchanged relative to the baseline (top cells show the winsorised estimates; bottom, in grey, the baseline): (i) for solar, Feed-in Tariff (Price) and FIT (Duration) retain positive post-treatment responses around $\ell \in \{0, +1, +2\}$; auctions remain modestly positive; RECs are weak/negative in the post period; (ii) for wind, FIT and auctions preserve their positive post-treatment profiles, while RECs and coal-phaseout statements remain small or negative. Moving from 1% to 2.5% winsorisation produces the expected mild compression in magnitudes at a few leads/lags, but there are no sign reversals at the headline post-treatment coefficients, and the set of statistically non-zero ν_ℓ is essentially unchanged. Small-N families (e.g., Carbon Tax-Electricity) remain more fragile, but winsorisation neither creates nor removes significance in a systematic way.

Takeaway. Trimming outcome tails does not alter our core conclusions: estimated policy effects around treatment are not artifacts of a few extreme observations. If anything, winsorisation slightly tightens inference in places and attenuates isolated pre- or far-post blips, reinforcing that our headline results are broad-based rather than outlier-driven.

See the solar and wind tables at the 1% cut-off (Table B.16, Table B.17) and at the 2.5% cut-off (Table B.18, Table B.19).

B.7 Covariates robustness

Goal: Show that covariates are essential and do change the results, but also show the results without them.

Policies often arrive amid macro–financial cycles, fossil-energy price swings, and heterogeneous market scale. To separate policy timing from these co-movements-and to improve precision-we estimate event-time effects $\{\nu_\ell\}$ *conditional* on a parsimonious vector of time-varying covariates. The baseline control set includes: Macro (BidYield, FDI/GDP, GFCF), Competition (OilRents/GDP), Innovation (R&D/GDP), Scale (logGDPpc, logPopulation), and a Within-technology state variable (Capacity share, expressed as % of *2023* electricity capacity to fix the denominator over time). We *exclude contemporaneous cost measures* from the baseline because policy can lower costs via induced deployment and learning, making them post-treatment ("bad-control") mediators; cost-augmented estimates are shown only as a robustness lower bound.

The subsections that follow implement three stress tests that bracket identification: (i) re-estimating without *any* policy covariates to assess sensitivity to concurrent instruments; (ii) restricting to *unique* policy histories to minimise policy bundles at the cost of power; and (iii) adding log cost as a covariate to hold local price dispersion fixed. We also report a blockwise leave-one-out analysis. Across these designs, covariates shift magnitudes (notably absorbing diffusion-stage differences via capacity share and attenuating effects when conditioning on costs) but leave the timing and sign of the main policy responses broadly intact. This lets readers see both the conservative, covariate-adjusted estimates and the unconditioned profiles side-by-side.

B.7.1 Distribution of policy events per country

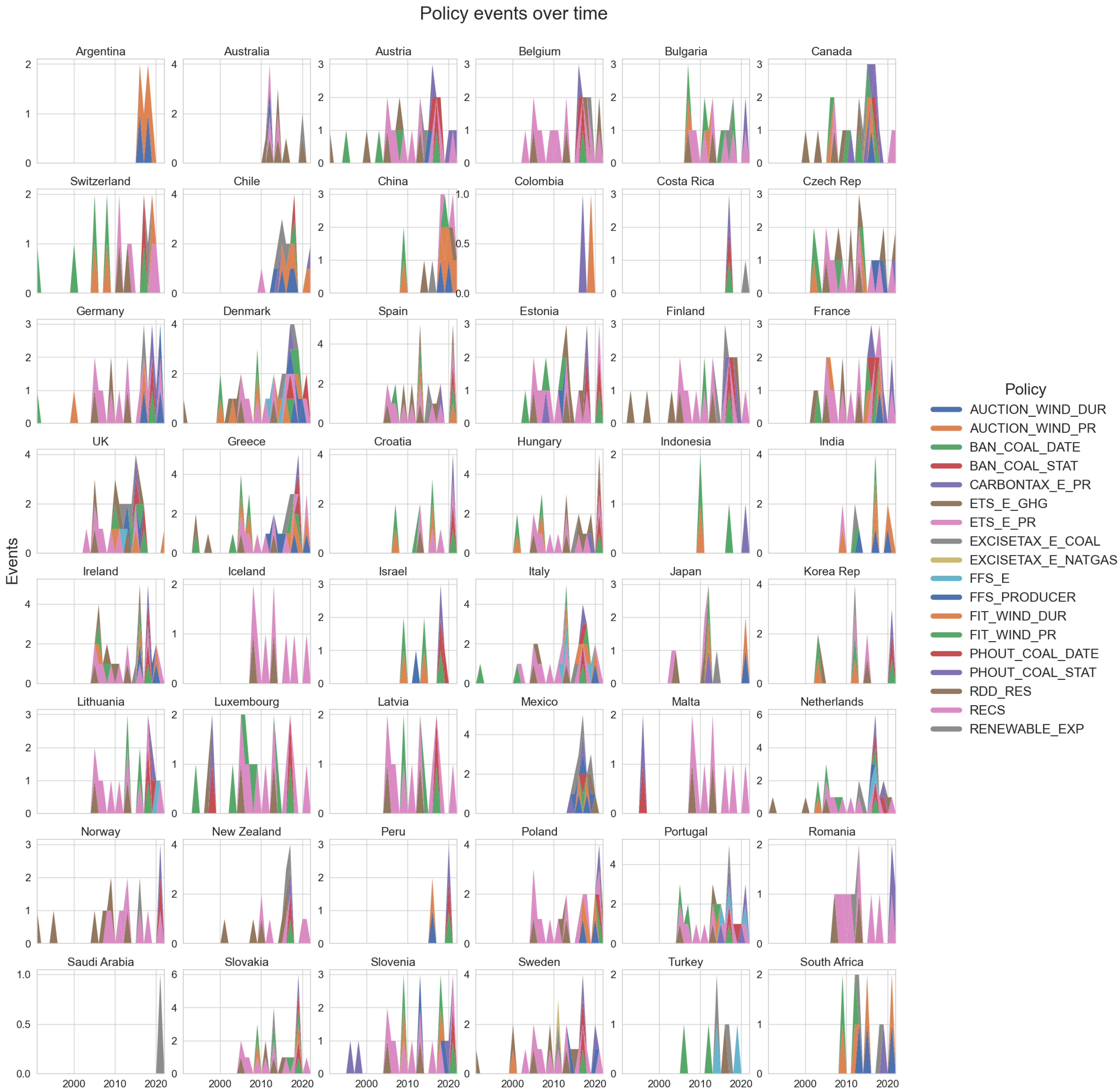


Figure B.14: Annual counts of policy onsets/strengthenings by instrument (colour) for each country. Shaded areas overlay when multiple instruments occur in the same year. The y-axis is the number of events; the x-axis is calendar year. The figure highlights staggered timing and clustered reform waves that underlie our identification strategy.

Figure B.14 visualises the timing and intensity of policy activity by country. Each small panel shows the annual count of *policy events*-introductions or material strengthenings-overlaid by instrument type (colour). Three features stand out. First, timing is clearly staggered across countries, with bursts of activity rather than smooth rollout, providing the variation exploited in our event–study design. Second, many countries exhibit *policy waves*: several instruments (e.g., FITs, RECs, auctions, carbon pricing, coal phase-out measures, fossil-subsidy reforms) arrive in quick succession, consistent with package-style reforms. Third, the instrument mix differs across countries and over time: early episodes commonly involve Feed-in-Tariffs and RECs, whereas later waves more often feature auctions and carbon-pricing expansions, mirroring the global shift from administratively set prices to competitive procurement and market instruments.

Two caveats are useful for interpretation. The plot reports *counts*, not stringency or coverage; a single "event" may vary in intensity across places. And overlays can visually crowd in years with multiple simultaneous actions; our regression framework therefore models event timing at the cohort–relative–time level rather than relying on raw counts alone.

B.7.2 Event study without policy covariates

Table B.20: solar, Without policy covariates

Policy	$\ell = -5$	$\ell = -4$	$\ell = -3$	$\ell = -2$	$\ell = 0$	$\ell = 1$	$\ell = 2$	$\ell = 3$	$\ell = 4$	$\ell = 5$
FIT SOL PR	-0.00* -0.01*	0.06* 0.05*	-0.04* -0.03*	-0.04* -0.05*	0.19* 0.18*	0.10* 0.07*	0.12* 0.13*	0.20 0.20	-0.04* -0.03*	-0.02* -0.02*
FIT SOL DUR	-0.01** -0.01**	0.03** 0.02**	-0.15** -0.15**	-0.18** -0.17**	0.21 0.21	0.23 0.23	0.18** 0.18**	0.27 0.28	-0.03* -0.03*	0.12* 0.11*
AUCTION SOL PR	0.26 0.26	0.36 0.36	0.05 0.06	0.03 0.04	0.07* 0.08*	0.24 0.24	0.15 0.14	-0.06* -0.07*	-0.09* -0.09*	-0.08* -0.08*
AUCTION SOL DUR	0.03* 0.02*	0.41 0.41	0.23 0.21	0.08 0.07	0.08* 0.08*	0.09* 0.10*	0.06 0.08	-0.01 -0.00	-0.09 -0.08	-0.05* -0.05*
RECS	-0.12* -0.12*	0.04 0.04	-0.13 -0.13	-0.01 -0.01	-0.19* -0.17*	-0.23* -0.22*	-0.00** 0.00**	-0.23* -0.22*	-0.18* -0.17*	-0.08* -0.07*
RENEWABLE EXP	-0.10* -0.10*	-0.12* -0.12*	0.08 0.09	0.08* 0.07*	0.22* 0.24*	0.11* 0.11*	0.06* 0.07*	0.01* 0.03*	0.02* 0.02*	0.01* 0.00*
RDD RES	0.25* 0.25*	0.06* 0.06*	0.05* 0.05*	0.09* 0.09*	0.01* -0.00*	0.07* 0.04*	-0.08* -0.07*	-0.15* -0.15*	-0.13* -0.15*	-0.05** -0.05**
FFS PRODUCER	0.34 0.32	-0.02 -0.04	0.18 0.17	0.26 0.22*	0.59 0.62	0.19 0.18	-0.09 -0.10*	-0.05* -0.06*	0.02* -0.00*	-0.00* 0.04*
ETS E PR	-0.01* -0.01*	-0.05* -0.06*	0.07* 0.07*	0.05* 0.05*	-0.05* -0.05*	0.05* 0.05*	0.01* 0.01*	0.01* 0.01*	-0.11* -0.11*	0.04* 0.04*
ETS E GHG	0.13 0.16	-0.05 -0.07	0.10 0.08	0.12 0.13	-0.05 -0.04	-0.05* -0.02*	-0.07* -0.09*	0.03* 0.03*	-0.13* -0.11*	-0.01* 0.00*
CARBONTAX E PR	-0.33* -0.33*	-0.32* -0.32*	-0.24** -0.24**	-0.09* -0.09*	0.41 0.41	-0.07* -0.07*	0.08 0.08	0.09 0.09	0.05 0.05	-0.08 -0.08
FFS E	-0.32* -0.28*	-0.08* -0.08*	1.35* 1.36*	-0.11* -0.12*	1.40* 1.27*	-0.05* -0.06*	-0.19* -0.22*	-0.02* 0.10*	-0.07** -0.10*	-0.03* -0.13*
EXCISETAX E NATGAS	0.05* 0.05*	-0.11 -0.11	-0.15 -0.16	-0.43 -0.43	-0.53* -0.53	0.08 0.08	-0.01 -0.01	-0.11* -0.11*	0.15* 0.15*	0.07* 0.07*
EXCISETAX E COAL	0.05* 0.04*	-0.11 -0.11	-0.15 -0.16	-0.43 -0.43	-0.53* -0.53	0.08 0.08	-0.01 -0.01	-0.11* -0.11*	0.15* 0.15*	0.07* 0.07*
PHOUT COAL DATE	0.10 0.11	0.12* 0.12*	-0.07* -0.08*	0.06* 0.06*	0.05* 0.04*	0.08 0.08	0.09* 0.09*	0.06* 0.06*	0.09* 0.09*	0.17* 0.17*
PHOUT COAL STAT	0.13 0.12	-0.02 -0.02	-0.11 -0.08	-0.08* -0.07*	-0.14* -0.12*	-0.09 -0.09	-0.01 -0.01	-0.01 -0.01	0.05 0.06	0.11 0.11
BAN COAL DATE	0.26 0.26	-0.03 -0.03	0.07 0.07	0.05 0.05	-0.07 -0.07	-0.09 -0.09	0.13 0.13	0.13 0.13	0.24 0.24	0.39 0.39
BAN COAL STAT	0.26 0.26	-0.03 -0.03	0.07 0.07	0.05 0.05	-0.07 -0.07	-0.09 -0.09	0.13 0.13	0.13 0.13	0.24 0.24	0.39 0.39

Notes: Event-time coefficients (Sun–Abraham IW). Coefficients are relative to the year before treatment ($\ell = -1$ omitted).
Stars: * p<0.10, ** p<0.05, *** p<0.01. 95% confidence intervals.
Each cell shows the robustness specification (top) and the baseline (bottom, grey).

Table B.21: wind, Without policy covariates

Policy	$\ell=-5$	$\ell=-4$	$\ell=-3$	$\ell=-2$	$\ell=0$	$\ell=1$	$\ell=2$	$\ell=3$	$\ell=4$	$\ell=5$
FIT WIND PR	-0.00** -0.00**	-0.03** -0.03**	0.07** 0.06**	-0.04** -0.04**	0.05** 0.05**	0.03** 0.03**	0.07** 0.07**	0.04** 0.04**	-0.00** -0.01**	0.00** 0.00**
FIT WIND DUR	-0.06** -0.06**	-0.06** -0.06**	-0.07** -0.07**	-0.02** -0.01**	0.04** 0.04**	0.06* 0.06*	0.12* 0.12*	0.01** 0.00**	-0.02** -0.02**	-0.03** -0.03**
AUCTION WIND PR	0.04 0.04	0.06 0.06	0.02* 0.02*	0.22 0.21	-0.01** -0.01**	0.05* 0.05*	0.11 0.12	0.01 0.02	-0.04* -0.04*	-0.00* -0.01*
AUCTION WIND DUR	-0.05* -0.05*	0.08* 0.07*	-0.00* -0.01*	0.20 0.20	-0.05* -0.05*	-0.09* -0.09*	0.01 0.01	-0.04* -0.05*	-0.09* -0.10*	-0.08* -0.08*
RECS	0.05* 0.05*	-0.01* -0.01*	-0.09** -0.09**	-0.07** -0.07**	0.03* 0.04*	-0.10** -0.10**	-0.02** -0.02**	-0.08** -0.08**	-0.06* -0.05*	-0.01** -0.01**
RENEWABLE EXP	0.06* 0.06*	0.01** 0.01**	-0.01** -0.01**	0.02** 0.02**	0.13* 0.13*	0.03** 0.03**	0.02** 0.01**	-0.01** -0.01**	-0.02** -0.03**	-0.04** -0.04**
RDD RES	0.06** 0.06**	0.03** 0.03**	-0.08** -0.08**	0.01** 0.01**	-0.08** -0.09**	-0.01** -0.01**	-0.07** -0.08**	-0.03** -0.03**	-0.07** -0.08**	-0.07** -0.06**
FFS PRODUCER	0.15 0.14	-0.01** -0.03**	-0.00** 0.00**	-0.04** -0.06**	0.46** 0.46**	0.15** 0.14**	0.16** 0.14**	0.01** 0.03**	0.07** 0.08**	0.03** 0.04**
ETS E PR	-0.06** -0.06**	-0.00** -0.01**	-0.00** -0.00**	-0.01** -0.01**	-0.05** -0.05**	-0.03* -0.03*	-0.05** -0.05**	-0.06** -0.06**	-0.07* -0.08*	0.01** 0.01**
ETS E GHG	0.07* 0.08*	-0.07* -0.07*	0.07** 0.05**	0.07** 0.06**	-0.04* -0.04*	-0.06* -0.06*	0.00* -0.00*	0.00* 0.01*	-0.00* -0.00*	-0.02** -0.02**
CARBONTAX E PR	0.49** 0.49**	-0.00* -0.00*	0.09** 0.09**	0.04 0.04	0.07* 0.07*	-0.04** -0.04**	-0.05** -0.05**	-0.01** -0.01**	-0.01** -0.01**	-0.07** -0.07**
FFS E	0.05** 0.05**	0.03** 0.03**	0.06* 0.07*	0.19* 0.19*	0.14** 0.11*	0.19* 0.22*	0.16** 0.13**	0.18** 0.21*	0.15** 0.18*	0.15** 0.13**
EXCISETAX E NATGAS	-0.16* -0.16*	0.14** 0.14**	0.05* 0.05*	0.04* 0.04*	0.13** 0.13**	0.06* 0.06*	-0.05* -0.05*	0.07** 0.07**	0.00** 0.00**	0.02** 0.02**
EXCISETAX E COAL	-0.16* -0.16*	0.14** 0.14**	0.05* 0.05*	0.04* 0.04*	0.13** 0.13**	0.06* 0.06*	-0.05* -0.05*	0.07** 0.07**	0.00** 0.00**	0.02** 0.02**
PHOUT COAL DATE	-0.04* -0.04*	-0.03* -0.04*	0.01* 0.01*	0.03* 0.03*	-0.04** -0.04**	0.00** 0.00**	-0.06* -0.06*	0.01* 0.01*	0.04* 0.04*	0.03** 0.03**
PHOUT COAL STAT	-0.05* -0.05*	-0.14 -0.14	-0.05 -0.05	0.00** 0.00**	-0.04** -0.05**	-0.07* -0.07*	-0.16 -0.16	-0.16 -0.16	-0.08* -0.08*	-0.04* -0.04*
BAN COAL DATE	0.05* 0.05*	-0.09 -0.09	0.10* 0.10*	0.12** 0.12**	0.04* 0.04*	-0.10 -0.10	-0.07* -0.07*	-0.10 -0.10	0.09* 0.09*	0.13* 0.13*
BAN COAL STAT	0.05* 0.05*	-0.09 -0.09	0.10* 0.10*	0.12** 0.12**	0.04* 0.04*	-0.10 -0.10	-0.07* -0.07*	-0.10 -0.10	0.09* 0.09*	0.13* 0.13*

Notes: Event-time coefficients (Sun–Abraham IW). Coefficients are relative to the year before treatment ($\ell=-1$ omitted).
Stars: * p<0.10, ** p<0.05, *** p<0.01. 95% confidence intervals.
Each cell shows the robustness specification (top) and the baseline (bottom, grey).

We test whether our results hinge on conditioning on *other* policy instruments by re-estimating the baseline Sun–Abraham model after *removing all policy covariates* from the control vector. Country and year fixed effects are retained, as are the non-policy macro controls used in the main specification.[21]

Solar. Table B.20 shows that FIT (PRICE) and FIT (DURATION) remain clearly positive around $\ell \in \{0, +1, +2\}$ with magnitudes very close to the baseline (e.g. $\ell = 0$ moves from 0.18 to 0.19 for FIT–PR). Auction effects stay modest and transient; RECs continue to be weak/negative post-treatment; planning retains small positives; ETS estimates stay near zero to slightly negative. Families with few treated cohorts (e.g. carbon-tax electricity, economy-wide fossil-fuel support) display the same fragility as in the baseline, with occasional wide intervals, but no systematic sign reversals. Lead coefficients are broadly similar, indicating that pre-trend diagnostics are not driven by the presence of policy controls.

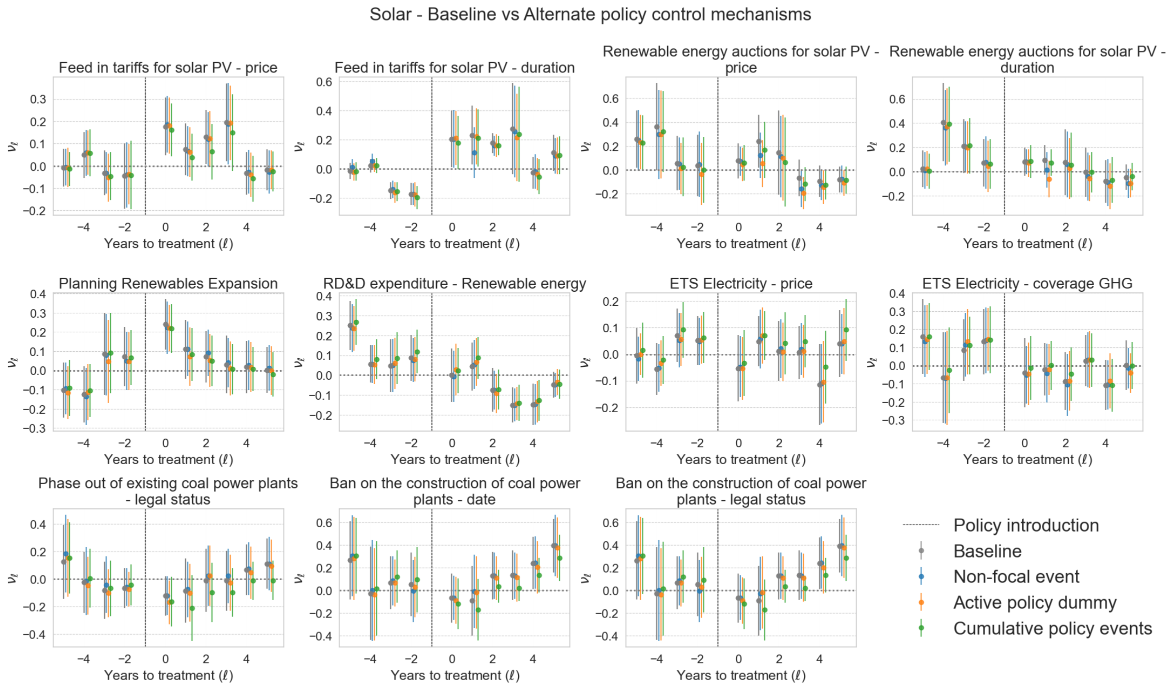
Wind. Table B.21 likewise preserves the policy ordering: FIT families post small, consistent positives near $\ell = 0$–$+2$; auctions are moderate and short-lived; RECs remain weak/negative; planning is small and positive; ETS hovers near zero to mildly negative. Differences relative to baseline are second-order and do

[21] This addresses two opposing concerns: (i) over-controlling for policy bundles that may lie on the causal path, and (ii) omitted-variable bias from concurrent instruments moving with the focal policy. Stability to dropping policy controls suggests limited sensitivity to either.

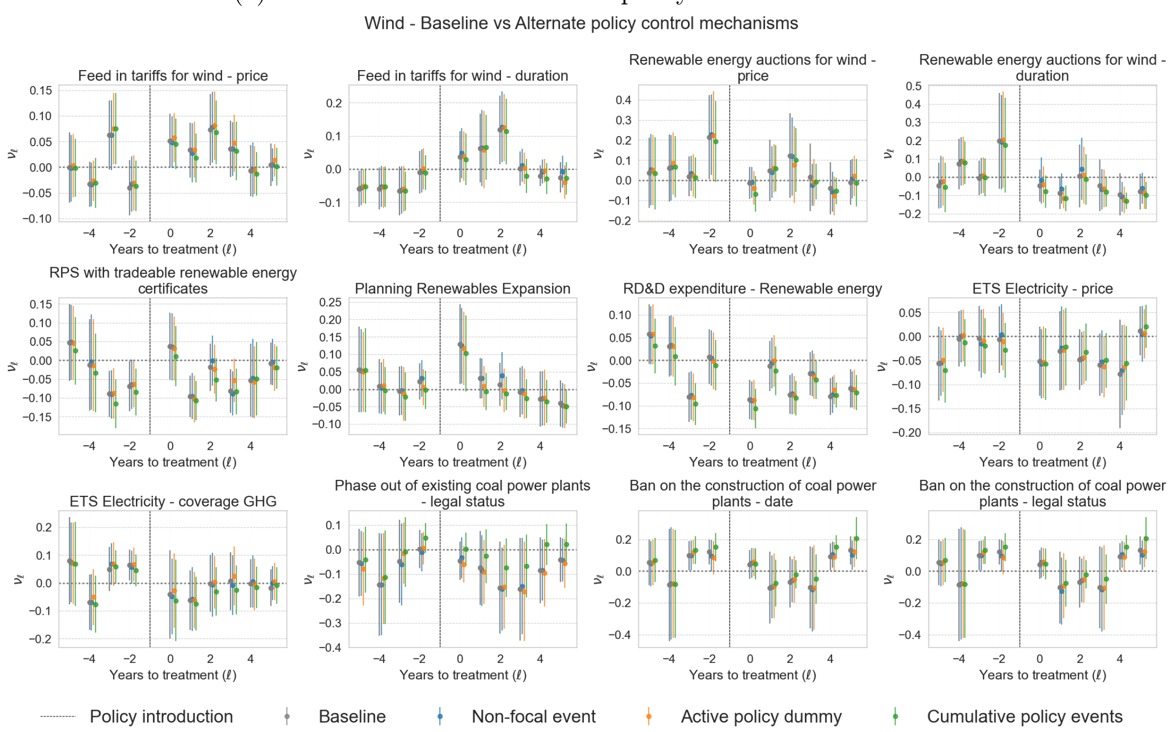
not alter the qualitative narrative. Where leads shift slightly, they do so within the same families already prone to high-frequency "tuning" rather than revealing new systematic pre-trends.

Takeaway. Dropping all policy covariates leaves signs, timing, and relative magnitudes essentially unchanged for both solar and wind. This robustness reduces concern that the main results are artifacts of conditioning on concurrent policy bundles; conversely, it also suggests that omitted concurrent instruments are not masking the focal effects. In short, the focal policy dynamics are first-order with respect to our outcome, and the main conclusions do not depend on including policy covariates.

B.7.3 Alternative specifications for policy controls



(a) solar - Baseline vs. Alternate policy control mechanisms



(b) wind - Baseline vs. Alternate policy control mechanisms

Figure B.15: Blockwise sensitivity of event–study paths ν_ℓ to covariate inclusion. Dots denote mean estimates; bands are 90% CIs; the vertical dashed line marks policy onset. Legend: grey = Baseline; blue = non-focal event; green = Cumulative policy event; orange = Active policy dummy.

To probe whether focal event–study paths ν_ℓ are sensitive to how we partial out *non–focal* policy co-movements, we re-estimate the Sun–Abraham interaction–weighted (IW) event study under three alternative blocks of non–focal policy controls (always *excluding* the focal policy family from the control set to avoid bad–control bias). Let $\mathcal{M}$ denote the set of non–focal families. For country i and year t, define: $P_{it}^{(m)} \in [0, 10]$ (stringency level), $S_{it}^{(m)}$ (event *size*, zero in non-event years), $A_{it}^{(m)} \in \{0, 1\}$ (event occurrence), and $C_{it}^{(m)} = \sum_{\tau \le t} \mathbf{1}\{S_{i\tau}^{(m)} \neq 0\}$ (cumulative event count). We then vary the lagged non–focal policy block

$$X_{i,t-1}^{(\mathrm{pol})} \in \Big\{ \underbrace{\{\Delta P_{i,t-1}^{(m)}\}_{m\in\mathcal{M}}}_{\texttt{Stringency (baseline)}}, \underbrace{\{S_{i,t-1}^{(m)}\}_{m\in\mathcal{M}}}_{\texttt{Event (non-focal)}}, \underbrace{\{A_{i,t-1}^{(m)}\}_{m\in\mathcal{M}}}_{\texttt{Active}}, \underbrace{\{C_{i,t-1}^{(m)}\}_{m\in\mathcal{M}}}_{\texttt{CumEvents}} \Big\}.$$

The focal policy enters only via the event–time terms $g_{i\ell}(t)$ (unweighted or η_i–weighted), with unit and calendar–time fixed effects and activity covariates (e.g. $\Delta \log \mathrm{GDPpc}_{i,t-1}$, $\Delta \log \mathrm{Pop}_{i,t-1}$). All covariates are standardized prior to estimation.

Figure B.15 overlays the IW paths ν_ℓ from the baseline (`Stringency` differences; grey) against the three alternatives (blue/orange/green). Bands are 90% CIs. Across solar and wind, the dynamic shapes are strikingly similar: no sign reversals at policy onset, overlapping bands in most ℓ, and only small level shifts at a few horizons. This indicates that our focal effects are not artifacts of concurrent non–focal policy packaging.

Solar: robustness by instrument family.

- *Feed-in-Tariffs (price, duration).* The immediate post-onset jump and persistence through $\ell = 1$–3 are preserved across all control blocks. The `CumEvents` block mildly attenuates the $\ell = 0$–1 peak for FIT price (grey vs. green), but estimates remain positive with overlapping intervals.

- *Auctions.* Early post-onset responses remain modest and become mixed or slightly negative at later horizons under all control choices, consistent with the baseline pattern. The coefficient at $\ell = 1$ is less statistically significant with other policy control specifications, however, the coefficient still higher than at previous ℓ.

- *Trading instruments (ETS, carbon taxes) and certificates (RECs).* Paths remain close to zero or negative at several horizons; switching to `Event`/`Active`/`CumEvents` does not deliver systematic positive re-interpretations.

- *Coal phase-out (dates/status) and public R&D.* Magnitudes vary somewhat across blocks, but qualitative timing (small, delayed, or mixed effects) is unchanged; intervals remain broadly overlapping with baseline.

Wind: robustness by instrument family.

- *Feed-in-Tariffs (price, duration).* Positive post-onset responses at $\ell = 1$–2 are stable across specifications, with very small level changes across `Event`, `Active`, and `CumEvents`.

- *Auctions.* Estimates remain noisier and more specification-sensitive at later ℓ, yet the broad baseline reading (limited near-term acceleration) survives.

- *Trading instruments and taxes.* Estimates cluster near zero or are mildly negative at several horizons; alternative non–focal blocks do not overturn this.

- *Coal measures and other non-market instruments.* Results remain mixed with no systematic upward re-interpretation relative to baseline.

Takeaways. Across both technologies, replacing the baseline non–focal *stringency-difference* block with sparse event blocks (`Event`, `Active`) or with a smoother stock variable (`CumEvents`) yields nearly identical focal dynamic paths. The core conclusions are therefore robust: (i) well-designed *subsidy-like* instruments (notably FIT price/duration reforms) show clear, near-term accelerations in adoption speeds; (ii) *trading* and *tax* instruments do not display comparably strong dynamic responses in our data; and (iii) non–market coal measures exhibit delayed or mixed effects that are insensitive to how we net out other policy families. The overlap of confidence bands across specifications in Figure B.15 supports the view that focal estimates are not driven by coincident policy packaging.

B.7.4 Event study with unique policy events

Table B.22: solar, Unique policy events only

Policy	$\ell=-5$	$\ell=-4$	$\ell=-3$	$\ell=-2$	$\ell=0$	$\ell=1$	$\ell=2$	$\ell=3$	$\ell=4$	$\ell=5$
FIT SOL PR	0.02* -0.01*	-0.02** 0.05*	-0.06** -0.03*	-0.48* -0.05*	0.01* 0.18*	-0.28* 0.07*	-0.32** 0.13*	-0.01 0.20	-0.25* -0.03*	-0.19* -0.02*
FIT SOL DUR	0.54 -0.01**	0.31* 0.02**	0.09 -0.15**	-0.09 -0.17**	-0.17* 0.21	-0.10* 0.23	-0.23* 0.18**	0.04* 0.28	-0.36** -0.03*	0.08* 0.11*
AUCTION SOL PR	1.28 0.26	-0.14 0.36	-0.15* 0.06	0.21 0.04	0.03* 0.08*	3.04 0.24	0.71 0.14	0.42 -0.07*	0.15 -0.09*	-0.57 -0.08*
AUCTION SOL DUR	-0.22* 0.02*	0.05* 0.41	0.27* 0.21	-0.05** 0.07	0.03* 0.08*	-0.02* 0.10*	-0.14 0.08	0.44 -0.00	-0.33 -0.08	0.02 -0.05*
RECS	-0.18** -0.12*	-0.06* 0.04	-0.26* -0.13	-0.04* -0.01	-0.14** -0.17*	-0.22** -0.22*	0.01** 0.00**	-0.25* -0.22*	-0.22* -0.17*	-0.09* -0.07*
RENEWABLE EXP	0.12 -0.10*	-0.06 -0.12*	0.03 0.09	-0.02* 0.07*	-0.03* 0.24*	-0.06 0.11*	0.03* 0.07*	-0.06* 0.03*	-0.06* 0.02*	0.01 0.00*
RDD RES	0.21* 0.25*	0.08* 0.06*	-0.09* 0.05*	0.05* 0.09*	0.03* -0.00*	0.10** 0.04*	0.00* -0.07*	-0.13* -0.15*	-0.09 -0.15*	-0.05** -0.05**
FFS PRODUCER	0.07* 0.32	-0.06 -0.04	-0.03 0.17	-0.04 0.22*	-0.25* 0.62	-0.22* 0.18	-0.21* -0.10*	-0.27 -0.06*	-0.21* -0.00*	-0.08 0.04*
ETS E PR	0.01* -0.01*	-0.17* -0.06*	-0.04* 0.07*	-0.11* 0.05*	-0.08 -0.05*	-0.00 0.05*	-0.11* 0.01*	-0.18* 0.01*	-0.11* -0.11*	0.01* 0.04*
ETS E GHG	-0.26 0.16	0.01* -0.07	0.16* 0.08	0.03* 0.13	-0.07* -0.04	-0.04* -0.02*	0.03* -0.09*	-0.07* 0.03*	-0.17* -0.11*	-0.28** 0.00*
CARBONTAX E PR	-0.27* -0.33*	-0.31** -0.32*	-0.22* -0.24**	-0.11* -0.09*	0.50* 0.41	0.17* -0.07*	0.44* 0.08	0.54** 0.09	0.46** 0.05	0.22** -0.08
FFS E	-0.46 -0.28*	-0.55 -0.08*	-0.09 1.36*	-0.19* -0.12*	2.75* 1.27*	-0.14 -0.06*	-0.26* -0.22*	0.05* 0.10*	-0.07** -0.10*	-0.13* -0.13*
EXCISETAX E NATGAS	... 0.05*	... -0.11	... -0.16	... -0.43	... -0.53	... 0.08	... -0.01	... -0.11*	... 0.15*	... 0.07*
EXCISETAX E COAL	... 0.04*	... -0.11	... -0.16	... -0.43	... -0.53	... 0.08	... -0.01	... -0.11*	... 0.15*	... 0.07*
PHOUT COAL DATE	... 0.11	... 0.12*	... -0.08*	... 0.06*	... 0.04*	... 0.08	... 0.09*	... 0.06*	... 0.09*	... 0.17*
PHOUT COAL STAT	-0.34 0.12	-0.31 -0.02	0.21 -0.08	-0.45 -0.07*	-0.24 -0.12*	-0.44* -0.09	-0.59* -0.01	-0.44* -0.01	-0.24 0.06	... 0.11
BAN COAL DATE	... 0.26	... -0.03	... 0.07	... 0.05	... -0.07	... -0.09	... 0.13	... 0.13	... 0.24	... 0.39
BAN COAL STAT	... 0.26	... -0.03	... 0.07	... 0.05	... -0.07	... -0.09	... 0.13	... 0.13	... 0.24	... 0.39

Notes: Event-time coefficients (Sun–Abraham IW). Coefficients are relative to the year before treatment ($\ell=-1$ omitted).
Stars: * p<0.10, ** p<0.05, *** p<0.01. 95% confidence intervals.
Each cell shows the robustness specification (top) and the baseline (bottom, grey).

Table B.23: wind, Unique policy events only

Policy	$\ell = -5$	$\ell = -4$	$\ell = -3$	$\ell = -2$	$\ell = 0$	$\ell = 1$	$\ell = 2$	$\ell = 3$	$\ell = 4$	$\ell = 5$
FIT WIND PR	0.05* -0.00**	-0.07** -0.03**	0.11* 0.06**	-0.07* -0.04**	0.19* 0.05**	0.11** 0.03**	0.14* 0.07**	0.01* 0.04**	0.04* -0.01**	0.01** 0.00**
FIT WIND DUR	-0.00* -0.06**	-0.04* -0.06**	-0.09* -0.07**	0.04** -0.01**	0.11** 0.04**	0.11** 0.06*	0.17** 0.12*	0.04** 0.00**	0.08** -0.02**	-0.02** -0.03**
AUCTION WIND PR	0.77** 0.04	0.27* 0.06	0.12** 0.02*	-0.13** 0.21	0.25* -0.01**	1.26* 0.05*	0.82* 0.12	0.55* 0.02	0.32* -0.04*	0.06* -0.01*
AUCTION WIND DUR	... -0.05*	... 0.07*	... -0.01*	... 0.20	... -0.05*	... -0.09*	... 0.01	... -0.05*	... -0.10*	... -0.08*
RECS	0.07 0.05*	0.01 -0.01*	-0.12** -0.09**	0.02* -0.07**	0.05* 0.04*	-0.12** -0.10**	-0.05* -0.02**	-0.13** -0.08**	-0.01* -0.05*	-0.03* -0.01**
RENEWABLE EXP	0.40** 0.06*	0.01** 0.01**	-0.10* -0.01**	-0.08** 0.02**	-0.05** 0.13*	-0.05** 0.03**	-0.05** 0.01**	-0.08** -0.01**	-0.14** -0.03**	-0.13** -0.04**
RDD RES	0.07* 0.06**	0.03** 0.03**	-0.07** -0.08**	0.05** 0.01**	-0.07** -0.09**	0.05* -0.01**	-0.09** -0.08**	-0.03** -0.03**	-0.08** -0.08**	-0.08** -0.06**
FFS PRODUCER	-0.06** 0.14	-0.01** -0.03**	0.03* 0.00**	-0.04** -0.06**	0.09** 0.46**	0.09* 0.14**	0.14** 0.14**	0.12* 0.03**	0.17* 0.08**	0.32* 0.04**
ETS E PR	-0.07** -0.06**	0.03* -0.01**	-0.03** -0.00**	-0.06** -0.01**	-0.02* -0.05**	0.02** -0.03*	-0.06** -0.05**	-0.09** -0.06**	-0.04** -0.08*	-0.03** 0.01**
ETS E GHG	0.10** 0.08*	0.08** -0.07*	0.16** 0.05**	-0.17** 0.06**	0.06* -0.04*	-0.05* -0.06*	-0.09** -0.00*	-0.03* 0.01*	-0.14** -0.00*	-0.12** -0.02**
CARBONTAX E PR	0.80** 0.49**	0.02** -0.00*	-0.04* 0.09**	0.47** 0.04	0.03** 0.07*	-0.03** -0.04**	0.02** -0.05**	-0.06** -0.01**	-0.03* -0.01**	0.02* -0.07**
FFS E	-0.11** 0.05**	-0.08* 0.03**	-0.01* 0.07*	0.14** 0.19*	0.05** 0.11*	0.15* 0.22*	0.07** 0.13**	0.13** 0.21*	0.12** 0.18*	0.15** 0.13**
EXCISETAX E NATGAS	... -0.16*	... 0.14**	... 0.05*	... 0.04*	... 0.13**	... 0.06*	... -0.05*	... 0.07**	... 0.00**	... 0.02**
EXCISETAX E COAL	... -0.16*	... 0.14**	... 0.05*	... 0.04*	... 0.13**	... 0.06*	... -0.05*	... 0.07**	... 0.00**	... 0.02**
PHOUT COAL DATE	... -0.04*	... -0.04*	... 0.01*	... 0.03*	... -0.04**	... 0.00**	... -0.06*	... 0.01*	... 0.04*	... 0.03**
PHOUT COAL STAT	-0.16* -0.05*	-0.09** -0.14	-0.14** -0.05	-0.23** 0.00**	-0.19* -0.05**	-0.18** -0.07*	-0.22** -0.16	-0.17** -0.16	-0.16* -0.08*	... -0.04*
BAN COAL DATE	... 0.05*	... -0.09	... 0.10*	... 0.12**	... 0.04*	... -0.10	... -0.07*	... -0.10	... 0.09*	... 0.13*
BAN COAL STAT	... 0.05*	... -0.09	... 0.10*	... 0.12**	... 0.04*	... -0.10	... -0.07*	... -0.10	... 0.09*	... 0.13*

Notes: Event-time coefficients (Sun–Abraham IW). Coefficients are relative to the year before treatment ($\ell = -1$ omitted).
Stars: * p<0.10, ** p<0.05, *** p<0.01. 95% confidence intervals.
Each cell shows the robustness specification (top) and the baseline (bottom, grey).

We further stress–test the baseline by restricting attention to *unique* policy histories: countries that (i) experience exactly one qualifying positive event for a *single* policy family (e.g. FIT–PR) that passes the significance threshold and (ii) record no other qualifying events in any policy family over the estimation window. Denote this set by $\mathcal{U}$. We then re-estimate the Sun–Abraham imputation–weighted event study on the restricted panel

$$y_{c,t} = \alpha_c + \gamma_t + X'_{c,t}\beta + \sum_{\ell \neq -1} \nu_\ell D^{(\ell)}_{e,t} + \varepsilon_{c,t}, \qquad c \in \mathcal{U},$$

retaining the same trimming, cohort window, omitted base ($\ell = -1$), non-policy controls $X_{c,t}$, and country–clustered standard errors as in the main specification.[22] This design maximally reduces concern about concurrent policy *bundles* but necessarily shrinks the treated cohort count and may induce composition differences (late adopters, smaller systems), so estimates are expected to be noisier and pre-trend tests underpowered.

Solar. Table B.22 shows that qualitative ordering broadly survives the uniqueness filter but with greater volatility. FIT–PR and FIT–DUR retain on-impact/near-term positives relative to the baseline row (grey),

[22]By construction, policy covariates for *other* families drop out. The never-treated pool comprises countries with zero qualifying events; not-yet-treated risk sets are defined within $\mathcal{U}$ in the standard way.

albeit with occasional negative leads and wider bands, consistent with reduced power and selection into smaller markets. Auction coefficients display sporadic large spikes at a few horizons, reflecting tiny cohort counts rather than a systematic change in dynamics. RECs remain weak/negative post-treatment; planning retains small positives; pricing instruments (ETS/Carbon tax) are mixed with wide intervals. Dots in some cells indicate empty risk sets under the strict uniqueness filter.

Wind. Patterns in Table B.23 are similar. FIT families continue to post small-to-moderate positives around $\ell \in \{0, +1, +2\}$, with modest attenuation and greater dispersion relative to the baseline. Auctions again show occasional large point estimates driven by very few cohorts; RECs remain weak/negative in the post period; planning is small/positive; ETS estimates hover near zero to slightly negative. As in solar, several families thin out enough that some horizons are not estimable.

Takeaway. The uniqueness filter delivers a much harsher verdict than the baseline. By construction it selects atypical countries (those that enact a single qualifying change and nothing else), collapses overlap, and slashes treated–cohort counts. The result is fragile identification: (i) precision deteriorates (wide intervals, many empty cells), (ii) point estimates become unstable with occasional sign flips and leverage-driven spikes (especially for auctions), and (iii) pre-trend diagnostics often weaken, with visible lead action that we cannot cleanly label "anticipation" given the low power and endogenous timing. Any residual positives for FIT families should therefore be read as *suggestive* only; headline magnitudes are not decision-grade. Substantively, the exercise implies that deployment responses usually materialise in *policy bundles*; isolating "single-lever" histories strips away the very sequencing that delivers effects. We treat this design strictly as a stress test of direction, not a basis for policy claims: it cautions against over-interpreting isolated-event estimates and reinforces that our main quantitative conclusions must rest on the broader, better-powered specifications (all-events, first-events, and dose–response), with explicit reporting of cohort counts at each horizon.

B.7.5 Event study with cost of technology as a covariate

Table B.24: solar, With lgCost as covariate

Policy	$\ell = -5$	$\ell = -4$	$\ell = -3$	$\ell = -2$	$\ell = 0$	$\ell = 1$	$\ell = 2$	$\ell = 3$	$\ell = 4$	$\ell = 5$
FIT SOL PR	-0.06** -0.01*	-0.08** 0.05*	-0.15* -0.03*	-0.04* -0.05*	0.13* 0.18*	0.06* 0.07*	0.13* 0.13*	0.50 0.20	0.10* -0.03*	0.05* -0.02*
FIT SOL DUR	-0.10* -0.01**	-0.02* 0.02**	-0.14** -0.15**	-0.21* -0.17**	0.19 0.21	0.24 0.23	0.23** 0.18**	0.80* 0.28	0.16* -0.03*	0.15 0.11*
AUCTION SOL PR	0.20 0.26	0.51 0.36	-0.12 0.06	-0.11* 0.04	0.17* 0.08*	0.07* 0.24	0.06* 0.14	0.12 -0.07*	0.05 -0.09*	0.09 -0.08*
AUCTION SOL DUR	0.08* 0.02*	0.51 0.41	0.04 0.21	-0.06* 0.07	0.07* 0.08*	0.03* 0.10*	-0.05* 0.08	-0.02 -0.00	-0.11* -0.08	0.03* -0.05*
RECS	0.13 -0.12*	0.04 0.04	-0.08 -0.13	-0.06 -0.01	-0.22 -0.17*	-0.27* -0.22*	-0.03* 0.00**	-0.15* -0.22*	-0.20* -0.17*	-0.13* -0.07*
RENEWABLE EXP	-0.21* -0.10*	-0.14* -0.12*	0.19 0.09	0.06* 0.07*	0.15* 0.24*	-0.03 0.11*	0.01 0.07*	0.02 0.03*	-0.01 0.02*	-0.00* 0.00*
RDD RES	0.47* 0.25*	0.01* 0.06*	0.02* 0.05*	0.06* 0.09*	-0.00* -0.00*	0.11* 0.04*	-0.12* -0.07*	-0.09* -0.15*	-0.04* -0.15*	-0.06** -0.05**
FFS PRODUCER	0.34 0.32	-0.03 -0.04	0.23 0.17	0.30 0.22*	0.08 0.62	0.16 0.18	0.12 -0.10*	-0.30 -0.06*	-0.22 -0.00*	0.22* 0.04*
ETS E PR	-0.08* -0.01*	-0.13* -0.06*	-0.01* 0.07*	0.03** 0.05*	-0.10* -0.05*	-0.00* 0.05*	-0.02* 0.01*	-0.03* 0.01*	-0.04* -0.11*	0.04* 0.04*
ETS E GHG	0.17 0.16	-0.01 -0.07	0.09 0.08	0.22 0.13	-0.17* -0.04	-0.20 -0.02*	-0.10 -0.09*	-0.09 0.03*	0.08* -0.11*	0.11 0.00*
CARBONTAX E PR	-0.28* -0.33*	-0.25* -0.32*	-0.30 -0.24**	0.14 -0.09*	0.25 0.41	0.00* -0.07*	-0.01* 0.08	-0.12* 0.09	-0.05 0.05	-0.01* -0.08
FFS E	-0.23* -0.28*	-0.16* -0.08*	1.28* 1.36*	-0.08* -0.12*	1.29* 1.27*	-0.31* -0.06*	-0.42 -0.22*	-0.03* 0.10*	-0.28* -0.10*	0.14 -0.13*
EXCISETAX E NATGAS	0.13 0.05*	0.10 -0.11	-0.19 -0.16	-0.41 -0.43	-0.64 -0.53	0.25 0.08	0.25 -0.01	0.07 -0.11*	0.27 0.15*	... 0.07*
EXCISETAX E COAL	0.13 0.04*	0.10 -0.11	-0.19 -0.16	-0.41 -0.43	-0.64 -0.53	0.25 0.08	0.25 -0.01	0.07 -0.11*	0.27 0.15*	... 0.07*
PHOUT COAL DATE	0.19 0.11	0.18 0.12*	-0.15 -0.08*	-0.04* 0.06*	-0.05* 0.04*	-0.02* 0.08	0.08* 0.09*	0.01* 0.06*	0.08* 0.09*	0.21 0.17*
PHOUT COAL STAT	0.32 0.12	0.04 -0.02	-0.04 -0.08	0.01* -0.07*	0.00 -0.12*	0.14 -0.09	0.07 -0.01	0.00* -0.01	0.10* 0.06	0.17 0.11
BAN COAL DATE	0.52 0.26	0.43 -0.03	0.12 0.07	0.17* 0.05	-0.06 -0.07	0.24 -0.09	0.17 0.13	0.24 0.13	0.32 0.24	0.33 0.39
BAN COAL STAT	0.52 0.26	0.43 -0.03	0.12 0.07	0.17* 0.05	-0.06 -0.07	0.24 -0.09	0.17 0.13	0.24 0.13	0.32 0.24	0.33 0.39

Notes: Event-time coefficients (Sun–Abraham IW). Coefficients are relative to the year before treatment ($\ell = -1$ omitted).
Stars: * p<0.10, ** p<0.05, *** p<0.01. 95% confidence intervals.
Each cell shows the robustness specification (top) and the baseline (bottom, grey).

Table B.25: wind, With lgCost as covariate

Policy	$\ell=-5$	$\ell=-4$	$\ell=-3$	$\ell=-2$	$\ell=0$	$\ell=1$	$\ell=2$	$\ell=3$	$\ell=4$	$\ell=5$
FIT WIND PR	0.01** -0.00**	-0.06** -0.03**	0.02** 0.06**	-0.00** -0.04**	0.07** 0.05**	-0.05** 0.03**	-0.01** 0.07**	0.05** 0.04**	-0.01** -0.01**	0.02** 0.00**
FIT WIND DUR	0.02** -0.06**	0.09* -0.06**	-0.02** -0.07**	0.07** -0.01**	0.11** 0.04**	-0.04** 0.06*	0.02** 0.12*	-0.01** 0.00**	-0.02** -0.02**	-0.03** -0.03**
AUCTION WIND PR	-0.02* 0.04	0.02* 0.06	-0.08* 0.02*	0.12 0.21	-0.05** -0.01**	0.01* 0.05*	0.06 0.12	-0.09* 0.02	-0.12* -0.04*	-0.05* -0.01*
AUCTION WIND DUR	-0.07* -0.05*	0.04* 0.07*	-0.06* -0.01*	0.14 0.20	-0.05** -0.05*	-0.11** -0.09*	-0.04* 0.01	-0.11* -0.05*	-0.12* -0.10*	-0.05** -0.08*
RECS	0.02* 0.05*	-0.05* -0.01*	-0.12* -0.09**	-0.00* -0.07**	-0.03* 0.04*	-0.06** -0.10**	-0.01* -0.02**	-0.09** -0.08**	0.05** -0.05*	0.07** -0.01**
RENEWABLE EXP	0.10* 0.06*	-0.01** 0.01**	0.02** -0.01**	0.05** 0.02**	0.15* 0.13*	0.06** 0.03**	0.05** 0.01**	0.02** -0.01**	-0.00** -0.03**	-0.01** -0.04**
RDD RES	-0.05** 0.06**	-0.04** 0.03**	-0.10** -0.08**	-0.03** 0.01**	-0.08** -0.09**	-0.07** -0.01**	-0.11** -0.08**	0.02** -0.03**	-0.09** -0.08**	-0.05** -0.06**
FFS PRODUCER	0.34** 0.14	-0.03* -0.03**	0.07** 0.00**	0.02** -0.06**	0.45** 0.46**	0.12** 0.14**	0.19** 0.14**	0.03** 0.03**	0.07** 0.08**	0.01** 0.04**
ETS E PR	-0.11* -0.06**	-0.03* -0.01**	-0.04* -0.00**	-0.19** -0.01**	-0.15* -0.05**	-0.11* -0.03*	-0.08* -0.05**	-0.07** -0.06**	-0.18* -0.08*	-0.04** 0.01**
ETS E GHG	0.02 0.08*	-0.16* -0.07*	-0.05* 0.05**	-0.08** 0.06**	-0.11* -0.04*	-0.18* -0.06*	-0.13* -0.00*	-0.05* 0.01*	-0.02* -0.00*	-0.08** -0.02**
CARBONTAX E PR	0.43** 0.49**	-0.02** -0.00*	-0.04** 0.09**	0.13 0.04	0.11* 0.07*	-0.01* -0.04**	0.01* -0.05**	-0.03** -0.01**	-0.02** -0.01**	-0.01* -0.07**
FFS E	-0.02** 0.05**	0.00** 0.03**	0.01* 0.07*	0.08* 0.19*	0.06* 0.11*	0.19* 0.22*	0.04** 0.13**	0.18* 0.21*	0.20* 0.18*	0.14** 0.13**
EXCISETAX E NATGAS	-0.22* -0.16*	0.09* 0.14**	0.04* 0.05*	0.01* 0.04*	0.12** 0.13**	0.09** 0.06*	-0.13 -0.05*	0.01** 0.07**	-0.01* 0.00**	0.01** 0.02**
EXCISETAX E COAL	-0.22* -0.16*	0.09* 0.14**	0.04* 0.05*	0.01* 0.04*	0.12** 0.13**	0.09** 0.06*	-0.13 -0.05*	0.01** 0.07**	-0.01* 0.00**	0.01** 0.02**
PHOUT COAL DATE	0.02* -0.04*	-0.01* -0.04*	0.02* 0.01*	0.08** 0.03*	0.01** -0.04**	0.05** 0.00**	-0.09* -0.06*	0.05* 0.01*	0.06* 0.04*	0.00** 0.03**
PHOUT COAL STAT	-0.02* -0.05*	-0.15 -0.14	-0.04 -0.05	0.06* 0.00**	0.03** -0.05**	0.01* -0.07*	-0.10 -0.16	-0.09 -0.16	-0.00* -0.08*	0.02* -0.04*
BAN COAL DATE	0.08* 0.05*	-0.15 -0.09	0.08* 0.10*	0.19* 0.12**	0.07* 0.04*	-0.08 -0.10	-0.04* -0.07*	-0.10 -0.10	0.18* 0.09*	0.27 0.13*
BAN COAL STAT	0.08* 0.05*	-0.15 -0.09	0.08* 0.10*	0.19* 0.12**	0.07* 0.04*	-0.08 -0.10	-0.04* -0.07*	-0.10 -0.10	0.18* 0.09*	0.27 0.13*

Notes: Event-time coefficients (Sun–Abraham IW). Coefficients are relative to the year before treatment ($\ell = -1$ omitted).
Stars: * p<0.10, ** p<0.05, *** p<0.01. 95% confidence intervals.
Each cell shows the robustness specification (top) and the baseline (bottom, grey).

We augment the Sun–Abraham specification with contemporaneous country–technology cost, $\log \text{Cost}_{it}$, to partial out *local* price swings that are not fully captured by time fixed effects (e.g., exchange–rate pass-through, BOS[23] frictions, procurement premia).

Cost endogeneity. When $\log \text{Cost}_{it}$ (i.e. unit CAPEX cost) is included, we address endogeneity with a control-function correction. We estimate a first stage using *lagged* exogenous drivers,

$$\log \text{Cost}_{it} = \alpha + \boldsymbol{\pi}^{\top} \mathbf{Z}_{i,t-1} + u_{it}, \tag{8}$$

and include the residual $\widehat{u}_{it}$ alongside $\log \text{Cost}_{it}$ in Equation 17–Equation 18. This absorbs the component of costs correlated with unobservables that also affect adoption growth.

Identification caveat (bad-control risk). Technology cost is plausibly *endogenous* to diffusion and policy via learning-by-doing and supply-chain deepening. Conditioning on $\log \text{Cost}_{it}$ can therefore block a genuine policy pathway and bias $\hat{\beta}_{\ell}$ toward zero (post-treatment/mediator control) and, with measurement

[23] Balance-of-system.

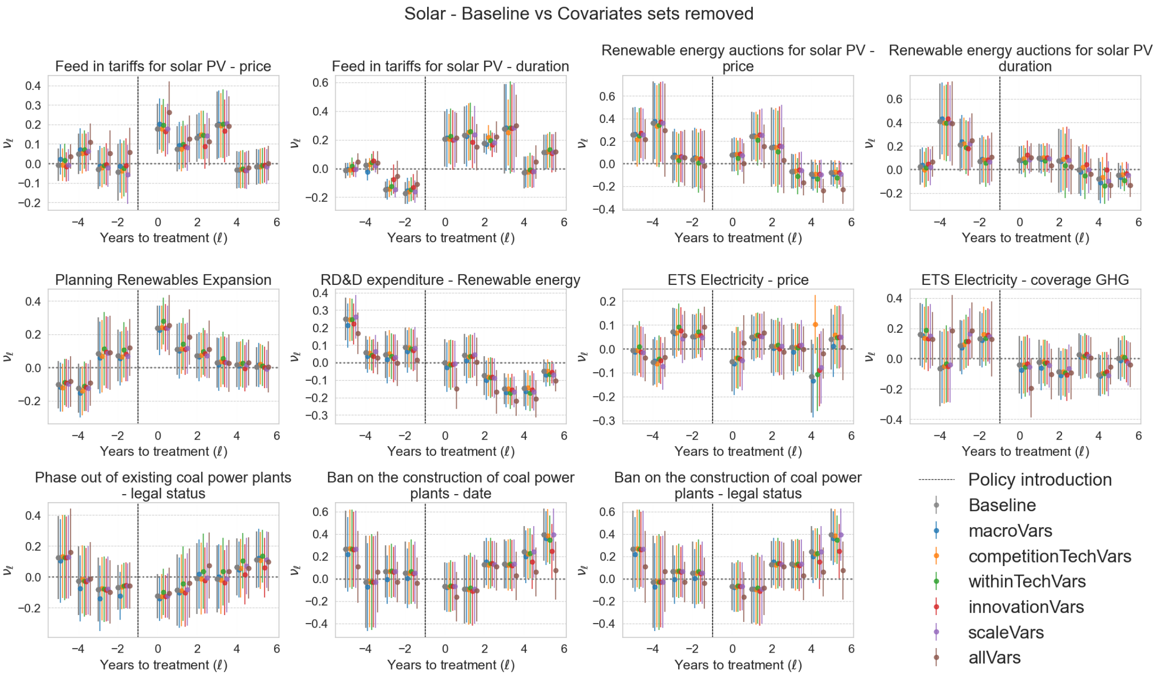
error in costs, induce further attenuation.[24] We therefore treat these estimates as *lower-bound, policy-direct* effects rather than total impacts.

What changes when costs are held fixed. Table B.24 and Table B.25 report the side-by-side coefficients (top cell = with $\log \text{Cost}$; bottom = baseline without it). (i) For the solar FIT families, post-event coefficients remain positive at short horizons but are generally attenuated relative to baseline, consistent with part of the policy effect operating through price reductions or cheaper project delivery. (ii) Auction-related estimates are mixed and less stable-some horizons attenuate or flip in sign-again consistent with costs mediating procurement effects and with thinner treated cohorts. (iii) Several non-deployment instruments (e.g., ETS, carbon taxes) show additional dampening once costs are conditioned on, which is coherent with an indirect policy $\to$ cost $\to$ deployment channel being partialled out. (iv) wind displays the same qualitative pattern: signs largely align with the baseline at near-term lags, but magnitudes shrink once $\log \text{Cost}$ is included, with wider intervals for auction terms.

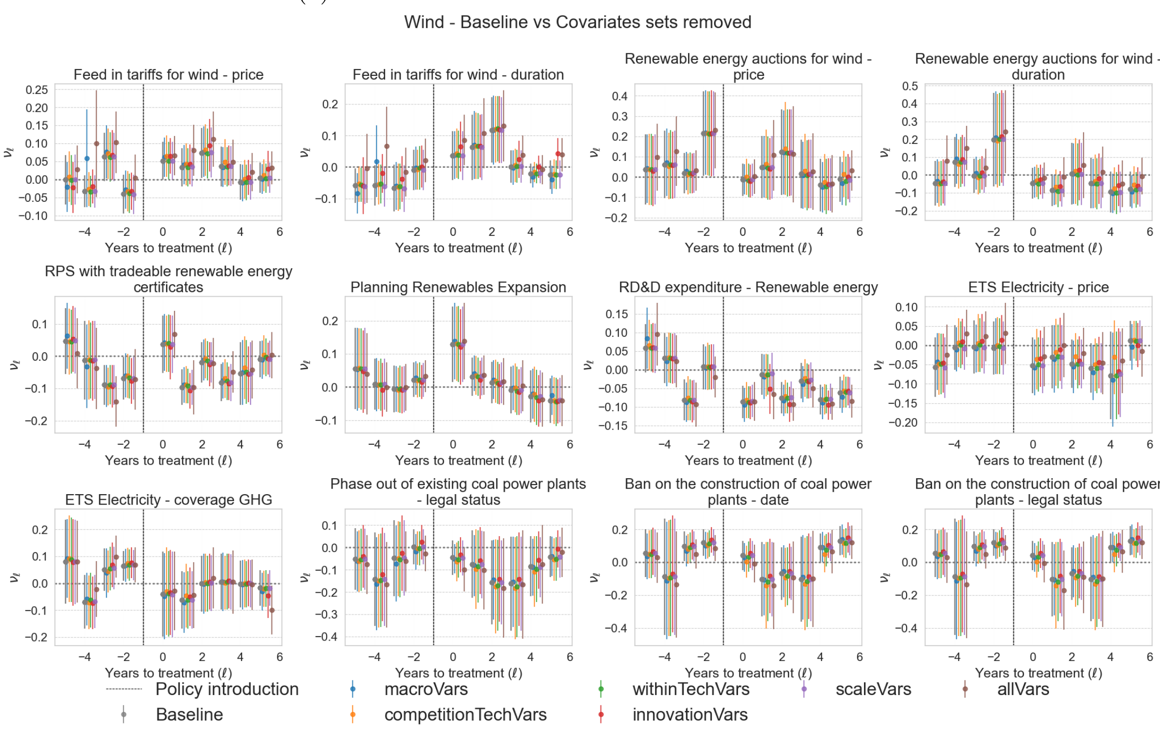
Takeaway. Including $\log \text{Cost}$ does *not* overturn the qualitative timing patterns, but it systematically trims magnitudes and increases noise. Given the mediator/simultaneity concern, we do not regard these as preferred estimates for policy evaluation. Instead, they serve as a stress test: effects that survive conditioning on cost are conservative, direct policy impacts; differences relative to the baseline quantify the likely cost-mediated share. Policy conclusions in the paper therefore rely on the baseline (no-cost) specification, with the cost-controlled results interpreted as a lower bound.

[24] See *learning-by-doing* in Arrow (1962) and the *bad controls* discussion in Angrist and Pischke (2009). Time fixed effects already absorb global cost trends; the added $\log \text{Cost}_{it}$ mainly captures *within-year, cross-country* cost deviations. If those deviations are themselves moved by policy or diffusion, conditioning on them mechanically trims policy effects.

B.7.6 Macro covariates



(a) solar - Baseline vs. covariate blocks removed



(b) wind - Baseline vs. covariate blocks removed

Figure B.16: Blockwise sensitivity of event–study paths ν_ℓ to covariate inclusion. Dots denote mean estimates; bands are 90% CIs; the vertical dashed line marks policy onset. Legend: grey = Baseline; blue = excluding Macro (BidYield, FDI/GDP, GFCF); green = excluding Within–tech (Capacity share, % of 2023 electricity capacity); purple = excluding Scale (logGDPpc, logPopulation); red = excluding Innovation (R&D/GDP); orange = excluding Competition (OilRents/GDP); brown = *noCovariates* (no controls).

To probe whether estimated policy effects on diffusion speeds are picking up correlated macro–financial or structural factors, we augment the event–study with a parsimonious set of time-varying covariates, grouped as follows: *(i) Macro* (BidYield, FDI/GDP, Gross Fixed Capital Formation), capturing financing conditions, openness, and investment cycles; *(ii) Competition technology* (OilRents/GDP), proxying returns to incumbent fossil supply that can dampen renewable uptake; *(iii) Within-technology state (Capacity share only; % of 2023 electricity capacity)*, summarising diffusion stage/headroom; *(iv) Innovation* (R&D/GDP), reflecting knowledge intensity; and *(v) Scale* (log GDP per capita, log Population), capturing market size, purchasing power, and administrative capacity.

Our baseline includes all five blocks, with the **within-technology block restricted to capacity share only**. We deliberately **exclude contemporaneous cost measures** (e.g., log installed cost) from the baseline because policy can shift costs via induced deployment and learning, making them post–treatment ("bad–control") variables; we report cost–augmented results only as robustness. Including capacity share in the baseline serves to absorb systematic stage-of-diffusion differences (the logistic gradient depends on position along the S–curve), improving cross-country comparability in $k_{it} = \Delta \log S_{it}$. Using the share relative to *2023 total electricity capacity* fixes the denominator over time, mitigating simultaneity with policy shocks to system size.

Design of the sensitivity analysis. We implement a blockwise "leave–one–out" exercise: starting from the baseline, we drop one block at a time and re-estimate the Sun–Abraham cohort–relative–time path $\{\nu_\ell\}_{\ell=-L}^{+K}$. We also report a *no–covariates* specification (retaining only fixed effects and event–time terms). Figure B.16 overlays these paths (grey = baseline; blue = w/o Macro; green = w/o Within–tech [Capacity share]; red = w/o Innovation; orange = w/o Competition; purple = w/o Scale; brown = noCovariates). For parsimony in the main text we focus on FIT-PRICE and FIT-DURATION for solar and wind, plus PLANNING policies where applicable; additional policies show similar patterns. (Underlying event–time coefficient tables are available on request.)

Findings. Across solar and wind, the event–time profiles are stable to the removal of any single block. Dropping Macro, Competition, Innovation, or Scale yields paths that are statistically indistinguishable from the baseline at the 90% level for most ℓ, with only modest changes in precision. The largest (still limited) movements arise when excluding the Within–technology block: post–treatment coefficients are somewhat *larger* when capacity share is *not* partialled out, consistent with this variable absorbing part of the policy–induced acceleration. The *no–covariates* specification produces slightly noisier but similarly shaped profiles and does not overturn inference on the timing or sign of policy impacts. These patterns hold for FIT-PRICE and FIT-DURATION in both technologies; where PLANNING measures are present, their paths are likewise robust in shape and magnitude.

Interpretation and specification choice. The blockwise robustness supports the identifying assumption of parallel trends *conditional* on broad macro–structural factors and diffusion stage. We retain capacity share in the baseline to control for position on the S–curve and mitigate cross–country heterogeneity in headroom, while excluding contemporaneous cost variables to avoid post–treatment bias. Cost–augmented specifications are reported as robustness and exhibit the expected attenuation of post–policy effects. The stability of paths when dropping any single exogenous block suggests our conclusions are not driven by incidental correlation with financing cycles, fossil rents, generic innovation intensity, or market scale.

B.7.7 Event study with net energy imports as a covariate

As an additional robustness check, the baseline event-study is augmented with a measure of country-year net energy imports. The motivation is that dependence on imported energy may affect both the likelihood of adopting renewable-support policies and the speed of subsequent renewable deployment. Countries that are more exposed to external energy supply may have stronger incentives to adopt support policies, while also exhibiting systematically different underlying adoption dynamics. Omitting this factor could therefore bias the estimated policy effects.

Specification. Let $\text{NetEnergyImports}_{it}$ denote the country-year measure of net energy imports. The event-study specification is augmented as

$$y_{it} = \sum_{\ell \neq -1} \nu_\ell \, g_{i\ell}(t) + \rho \, \text{NetEnergyImports}_{i,t-1} + X_{it}' \delta + \alpha_i + \gamma_t + \varepsilon_{it}, \tag{9}$$

where $g_{i\ell}(t)$ denotes the Sun–Abraham event-time terms, X_{it} contains the remaining baseline covariates and policy controls, and α_i and γ_t are country and year fixed effects. The net-energy-imports variable is thus introduced as an additional lagged country-year covariate, while all other aspects of the specification, including the event window, treatment definition, and sample restrictions, are left unchanged.

This exercise is best interpreted as an omitted-variable robustness check. It asks whether the estimated policy effects are sensitive to conditioning on cross-country differences in energy dependence that may shape both policy incentives and renewable adoption trajectories.

Table B.26: Solar, With NetEnergyImports as covariate

Policy	$\ell = -5$	$\ell = -4$	$\ell = -3$	$\ell = -2$	$\ell = 0$	$\ell = 1$	$\ell = 2$	$\ell = 3$	$\ell = 4$	$\ell = 5$
FIT SOL PR	-0.01* -0.01*	0.05* 0.05*	-0.04* -0.03*	-0.05* -0.05*	0.17* 0.18*	0.07* 0.07*	0.13* 0.13*	0.19 0.20	-0.03* -0.03*	-0.02* -0.02*
FIT SOL DUR	-0.00** -0.01**	0.03** 0.02**	-0.14** -0.15**	-0.16** -0.17**	0.20 0.21	0.23 0.23	0.17** 0.18**	0.27 0.28	-0.03* -0.03*	0.10* 0.11*
AUCTION SOL PR	0.25 0.26	0.35 0.36	0.05 0.06	0.03 0.04	0.07* 0.08*	0.24 0.24	0.14 0.14	-0.08* -0.07*	-0.10* -0.09*	-0.09* -0.08*
AUCTION SOL DUR	0.02* 0.02*	0.40 0.41	0.20 0.21	0.07 0.07	0.08* 0.08*	0.09* 0.10*	0.07 0.08	-0.01 -0.00	-0.09 -0.08	-0.05* -0.05*
RECS	-0.12* -0.12*	0.05 0.04	-0.13 -0.13	-0.02 -0.01	-0.17* -0.17*	-0.22* -0.22*	0.00** 0.00**	-0.22** -0.22*	-0.17* -0.17*	-0.08* -0.07*
RENEWABLE EXP	-0.10* -0.10*	-0.12* -0.12*	0.09 0.09	0.07* 0.07*	0.24* 0.24*	0.11* 0.11*	0.07* 0.07*	0.03* 0.03*	0.01* 0.02*	-0.00* 0.00*
RDD RES	0.27* 0.25*	0.07* 0.06*	0.05* 0.05*	0.10* 0.09*	0.00* -0.00*	0.05* 0.04*	-0.07* -0.07*	-0.15* -0.15*	-0.15* -0.15*	-0.05** -0.05**
FFS PRODUCER	0.31 0.32	-0.05 -0.04	0.15 0.17	0.21* 0.22*	0.62 0.62	0.17 0.18	-0.11* -0.10*	-0.07* -0.06*	-0.01* -0.00*	0.03* 0.04*
ETS E PR	-0.00* -0.01*	-0.05* -0.06*	0.07* 0.07*	0.05* 0.05*	-0.05* -0.05*	0.05* 0.05*	0.01* 0.01*	0.01* 0.01*	-0.12* -0.11*	0.04* 0.04*
ETS E GHG	0.16 0.16	-0.06 -0.07	0.09 0.08	0.14 0.13	-0.04 -0.04	-0.02* -0.02*	-0.08* -0.09*	0.02* 0.03*	-0.11* -0.11*	-0.00* 0.00*
CARBONTAX E PR	-0.33* -0.33*	-0.33* -0.32*	-0.24** -0.24**	-0.10* -0.09*	0.40 0.41	-0.07* -0.07*	0.07 0.08	0.08 0.09	0.04 0.05	-0.09 -0.08
FFS E	-0.27* -0.28*	-0.08* -0.08*	1.36* 1.36*	-0.12* -0.12*	1.27* 1.27*	-0.07* -0.06*	-0.22* -0.22*	0.09* 0.10*	-0.10* -0.10*	-0.13* -0.13*
EXCISETAX E NATGAS	0.05* 0.05*	-0.11 -0.11	-0.16 -0.16	-0.44 -0.43	-0.53 -0.53	0.08 0.08	-0.01 -0.01	-0.12* -0.11*	0.14* 0.15*	0.06* 0.07*
EXCISETAX E COAL	0.05* 0.04*	-0.11 -0.11	-0.16 -0.16	-0.44 -0.43	-0.53 -0.53	0.08 0.08	-0.01 -0.01	-0.12* -0.11*	0.14* 0.15*	0.06* 0.07*
PHOUT COAL DATE	0.10 0.11	0.11* 0.12*	-0.09* -0.08*	0.05* 0.06*	0.02* 0.04*	0.06* 0.08	0.06* 0.09*	0.03* 0.06*	0.07* 0.09*	0.15* 0.17*
PHOUT COAL STAT	0.11 0.12	-0.04 -0.02	-0.10 -0.08	-0.08* -0.07*	-0.15* -0.12*	-0.12 -0.09	-0.05 -0.01	-0.05 -0.01	0.03 0.06	0.08 0.11
BAN COAL DATE	0.25 0.26	-0.05 -0.03	0.04 0.07	0.03 0.05	-0.10 -0.07	-0.13 -0.09	0.09 0.13	0.09 0.13	0.20 0.24	0.36 0.39
BAN COAL STAT	0.25 0.26	-0.05 -0.03	0.04 0.07	0.03 0.05	-0.10 -0.07	-0.13 -0.09	0.09 0.13	0.09 0.13	0.20 0.24	0.36 0.39

Notes: Event-time coefficients (Sun–Abraham IW). Coefficients are relative to the year before treatment ($\ell = -1$ omitted).
Stars: * p<0.10, ** p<0.05, *** p<0.01. 95% confidence intervals.
Each cell shows the robustness specification (top) and the baseline (bottom, grey).

Table B.27: Wind, With NetEnergyImports as covariate

Policy	$\ell=-5$	$\ell=-4$	$\ell=-3$	$\ell=-2$	$\ell=0$	$\ell=1$	$\ell=2$	$\ell=3$	$\ell=4$	$\ell=5$
FIT WIND PR	-0.01** -0.00**	-0.04** -0.03**	0.06** 0.06**	-0.04** -0.04**	0.05** 0.05**	0.03** 0.03**	0.07** 0.07**	0.04** 0.04**	-0.01** -0.01**	0.00** 0.00**
FIT WIND DUR	-0.06** -0.06**	-0.06** -0.06**	-0.06** -0.07**	-0.02** -0.01**	0.04** 0.04**	0.07* 0.06*	0.12* 0.12*	0.00** 0.00**	-0.02** -0.02**	-0.03** -0.03**
AUCTION WIND PR	0.04 0.04	0.06 0.06	0.02* 0.02*	0.21 0.21	-0.01** -0.01**	0.05* 0.05*	0.12 0.12	0.01 0.02	-0.04* -0.04*	-0.02* -0.01*
AUCTION WIND DUR	-0.05* -0.05*	0.07* 0.07*	-0.01* -0.01*	0.20 0.20	-0.05* -0.05*	-0.09* -0.09*	0.01 0.01	-0.05* -0.05*	-0.10* -0.10*	-0.08* -0.08*
RECS	0.03* 0.05*	-0.03* -0.01*	-0.09** -0.09**	-0.07** -0.07**	0.04* 0.04*	-0.10** -0.10**	-0.02** -0.02**	-0.08** -0.08**	-0.05* -0.05*	-0.01** -0.01**
RENEWABLE EXP	0.06* 0.06*	0.01** 0.01**	-0.01** -0.01**	0.02** 0.02**	0.13* 0.13*	0.03** 0.03**	0.01** 0.01**	-0.01** -0.01**	-0.03** -0.03**	-0.04** -0.04**
RDD RES	0.06** 0.06**	0.03** 0.03**	-0.08** -0.08**	0.01** 0.01**	-0.08** -0.09**	-0.01** -0.01**	-0.07** -0.08**	-0.03** -0.03**	-0.08** -0.08**	-0.06** -0.06**
FFS PRODUCER	0.14 0.14	-0.03** -0.03**	-0.01** 0.00**	-0.06** -0.06**	0.46** 0.46**	0.13** 0.14**	0.14** 0.14**	0.02** 0.03**	0.08** 0.08**	0.04** 0.04**
ETS E PR	-0.05** -0.06**	0.00** -0.01**	0.00** -0.00**	-0.00** -0.01**	-0.05** -0.05**	-0.03** -0.03*	-0.05** -0.05**	-0.06** -0.06**	-0.08* -0.08*	0.01** 0.01**
ETS E GHG	0.09* 0.08*	-0.06* -0.07*	0.06** 0.05**	0.07** 0.06**	-0.03* -0.04*	-0.06* -0.06*	0.00* -0.00*	0.01* 0.01*	0.00* -0.00*	-0.02** -0.02**
CARBONTAX E PR	0.49** 0.49**	-0.00** -0.00*	0.09** 0.09**	0.04 0.04	0.08* 0.07*	-0.04** -0.04**	-0.05** -0.05**	-0.01** -0.01**	-0.01** -0.01**	-0.07** -0.07**
FFS E	0.05** 0.05**	0.03** 0.03**	0.07* 0.07*	0.20* 0.19*	0.11* 0.11*	0.23* 0.22*	0.14** 0.13**	0.21* 0.21*	0.18* 0.18*	0.13** 0.13**
EXCISETAX E NATGAS	-0.15* -0.16*	0.14** 0.14**	0.05* 0.05*	0.05* 0.04*	0.14** 0.13**	0.06* 0.06*	-0.05* -0.05*	0.07** 0.07**	0.00** 0.00**	0.02** 0.02**
EXCISETAX E COAL	-0.15* -0.16*	0.14** 0.14**	0.05* 0.05*	0.05* 0.04*	0.14** 0.13**	0.06* 0.06*	-0.05* -0.05*	0.07** 0.07**	0.00** 0.00**	0.02** 0.02**
PHOUT COAL DATE	-0.04* -0.04*	-0.03* -0.04*	0.01* 0.01*	0.04* 0.03*	-0.03** -0.04**	0.01** 0.00**	-0.05* -0.06*	0.01* 0.01*	0.04* 0.04*	0.03** 0.03**
PHOUT COAL STAT	-0.05* -0.05*	-0.14 -0.14	-0.05 -0.05	0.00** 0.00**	-0.05** -0.05**	-0.08* -0.07*	-0.16 -0.16	-0.16 -0.16	-0.08* -0.08*	-0.04* -0.04*
BAN COAL DATE	0.06* 0.05*	-0.09 -0.09	0.10* 0.10*	0.12** 0.12**	0.04* 0.04*	-0.11 -0.10	-0.07* -0.07*	-0.10 -0.10	0.09** 0.09*	0.14* 0.13*
BAN COAL STAT	0.06* 0.05*	-0.09 -0.09	0.10* 0.10*	0.12** 0.12**	0.04* 0.04*	-0.11 -0.10	-0.07* -0.07*	-0.10 -0.10	0.09** 0.09*	0.14* 0.13*

Notes: Event-time coefficients (Sun–Abraham IW). Coefficients are relative to the year before treatment ($\ell=-1$ omitted).
Stars: * p<0.10, ** p<0.05, *** p<0.01. 95% confidence intervals.
Each cell shows the robustness specification (top) and the baseline (bottom, grey).

What changes when net energy imports are held constant. Table B.26 and Table B.27 report the side-by-side coefficients, where the top entry in each cell corresponds to the specification including NetEnergyImports and the bottom entry reports the baseline estimate.

For Solar, Table B.26 shows that the estimated event-time paths are almost unchanged. FIT price and duration reforms remain positive at short horizons, with only trivial attenuation relative to baseline. For example, the on-impact coefficient for FIT SOL PR moves from 0.18 in the baseline to 0.17 with net energy imports included, while FIT SOL DUR shifts from 0.21 to 0.20. Auction effects remain modest and mixed at longer horizons, RECs remain weak or negative post-treatment, and the qualitative ordering across policy families is preserved. There are no new sign reversals at policy onset and no meaningful change in lead behavior.

For Wind, the same conclusion emerges from Table B.27. The positive near-term responses for FIT families remain essentially identical to the baseline, auction effects remain limited, and estimates for RECs, ETS, carbon pricing, fossil-fuel support, and coal-related measures continue to track the baseline closely. In most cases the coefficients are numerically identical up to rounding, indicating that conditioning on net energy import dependence does not materially alter the estimated dynamic responses.

Takeaway. Including net energy imports as an additional covariate does not materially change the estimated policy paths for either Solar or Wind. The close correspondence between the top and bottom entries in Table B.26 and Table B.27 suggests that the baseline findings are not being driven by omitted differences in countries' exposure to imported energy. The main conclusions therefore appear robust to conditioning on this additional dimension of macro-energy structure.

B.7.8 Event study with federal-system interactions

As a robustness check, we allow the event-time associations between national renewable-support reforms and deployment growth to differ between federal and unitary political systems. This exercise addresses the possibility that national reforms have different meanings across institutional settings because implementation authority, policy coordination, and unobserved subnational intervention may differ. It should be interpreted as a heterogeneity analysis rather than as an estimate of the causal effect of federalism. We flag as federal: Germany, Canada, Australia, Switzerland, Belgium, Austria, India, Mexico, Argentina; Russia. Note that the United States and Brazil are not covered by the CAPMF.

Federal-interaction specification. Let F_i equal one when country i has a federal constitutional system and zero otherwise. We interact F_i with all cohort-by-event-time terms entering the Sun–Abraham design. Schematically, the resulting specification is

$$y_{ist} = \sum_{\ell \neq -1} \nu_\ell g_{is\ell}(t) + \sum_{\ell \neq -1} \theta_\ell \left[g_{is\ell}(t) \times F_i \right] + X_{it}' \delta + \alpha_i + \gamma_t + \lambda_s + \varepsilon_{ist}, \tag{10}$$

where s indexes event stacks, $g_{is\ell}(t)$ represents the cohort-by-event-time terms underlying the interaction-weighted estimator, and λ_s denotes event-stack fixed effects. The specification retains the baseline covariates, policy controls, sample restrictions, event selection rules, and estimation window. The reported coefficients are obtained by applying the same Sun–Abraham aggregation weights as in the baseline analysis.

Because F_i is time invariant, its main effect is absorbed by the country fixed effects. The aggregated coefficient ν_ℓ is the event-time path for unitary countries, while $\nu_\ell + \theta_\ell$ is the corresponding path for federal countries. The interaction θ_ℓ measures the federal–unitary difference at event time ℓ. These group-specific paths may nevertheless differ because of institutional structure, adopter composition, reform timing, policy intensity, or unobserved subnational policies.

Table B.28: Solar, Federal-system interaction: federal vs. unitary event-time paths

Policy	$\ell = -5$	$\ell = -4$	$\ell = -3$	$\ell = -2$	$\ell = 0$	$\ell = 1$	$\ell = 2$	$\ell = 3$	$\ell = 4$	$\ell = 5$
FIT SOL PR	-0.14*	-0.07	-0.16**	-0.10	0.18***	0.00	0.05	0.21*	0.01	-0.10
	0.03	0.11*	0.03	0.00	0.18*	0.12	0.17**	0.20	-0.05	0.03
FIT SOL DUR	-0.00	0.01	0.01	-0.05	-0.08	0.12*	0.22***	0.26***	-0.05	0.27***
	-0.02	0.03	-0.18***	-0.21***	0.28**	0.27*	0.17***	0.27	-0.02	0.07
AUCTION SOL PR	0.27***	-0.02	0.04	-0.30***	-0.12	0.55***	0.33	-0.12	-0.14	-0.07
	0.23*	0.59*	0.06	0.31	0.22*	0.06	-0.03	-0.05	-0.06	-0.08
AUCTION SOL DUR	-0.05	-0.01	0.05	-0.27**	-0.10	0.13	0.28	-0.17*	-0.17**	-0.02
	0.12	0.67**	0.30*	0.28	0.17*	0.09	-0.03	0.04	-0.06	-0.07
RECS	-0.26***	-0.10	0.05	0.41***	-0.17**	-0.29***	0.20***	-0.41***	-0.34***	-0.11**
	-0.09	0.08	-0.18**	-0.19***	-0.17***	-0.19***	-0.08	-0.14**	-0.10	-0.06
RENEWABLE EXP	0.02	-0.11	0.01	-0.02	0.12*	0.18**	0.19***	0.12*	0.09*	0.03
	-0.15*	-0.14	0.12	0.09	0.26***	0.07	0.02	-0.01	-0.00	-0.01
RDD RES	0.31***	0.15***	-0.06	0.16**	0.07	0.05	0.02	-0.09	-0.07	0.03
	0.22**	0.05	0.08	0.07	-0.02	0.04	-0.10	-0.17***	-0.17**	-0.07
FFS PRODUCER	-0.09	-0.15*	0.51***	0.41***	0.11	0.04	0.03	-0.08	0.08	0.05
	0.46***	-0.04	-0.07	0.13	0.74***	0.18***	-0.19***	-0.09	-0.07	-0.01
ETS E PR	0.03	-0.10	0.13**	0.19***	-0.03	-0.02	-0.02	-0.01	-0.07	-0.02
	-0.02	-0.05	0.06	0.02	-0.06	0.06	0.02	0.01	-0.12	0.06
ETS E GHG	0.09	0.11	0.20**	0.29***	-0.09	-0.09	-0.06	0.02	-0.00	-0.05
	0.18	-0.14	0.05	0.09	-0.03	-0.00	-0.09	0.02	-0.13	0.02
CARBONTAX E PR	-0.26***	-0.23***	-0.16**	0.10	0.05	-0.21***	0.02	-0.04	0.00	0.01
	-0.35***	-0.36***	-0.26***	-0.21***	0.66***	0.02	0.11**	0.18***	0.07	-0.17***
FFS E	–	–	–	–	–	–	–	–	–	–
	–	–	–	–	–	–	–	–	–	–
EXCISETAX E NATGAS	–	–	–	–	–	–	–	–	–	–
	–	–	–	–	–	–	–	–	–	–
EXCISETAX E COAL	–	–	–	–	–	–	–	–	–	–
	–	–	–	–	–	–	–	–	–	–
PHOUT COAL DATE	0.11	-0.10	-0.24	-0.23	-0.09	-0.13	-0.08	-0.01	0.10	0.19
	0.04	0.23**	-0.00	0.20*	0.11	0.19	0.17**	0.11	0.12	0.17
PHOUT COAL STAT	0.01	-0.23	-0.10	-0.19	-0.17	-0.22	-0.31	-0.15	-0.02	0.02
	0.16	0.06	-0.06	-0.02	-0.10	-0.03	0.13	0.09	0.14	0.17
BAN COAL DATE	-0.06	-0.18	0.01	-0.02	-0.05	-0.07	0.03	0.09	0.17	0.35**
	0.37	0.02	0.08	0.07	-0.07	-0.10	0.16	0.15	0.26*	0.41**
BAN COAL STAT	-0.06	-0.18	0.01	-0.02	-0.05	-0.07	0.03	0.09	0.17	0.35**
	0.37	0.02	0.08	0.07	-0.07	-0.10	0.16	0.15	0.26*	0.41**

Notes: Sun–Abraham interaction-weighted event study with federal-system interactions, as described in eq. (10). Each cell reports the federal marginal path $\nu_\ell + \theta_\ell$ on top and the unitary marginal path ν_ℓ below in grey. Stars test each marginal path against zero; they do not test equality between federal and unitary countries. The average federal–unitary interactions and their tests are reported in Table B.30. Standard errors are clustered by country. Stars: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. "–" indicates that no federal country adopted the policy, so the federal path and federal–unitary interaction are not identified; the table suppresses the unitary-only estimate in these rows.

Table B.29: Wind, Federal-system interaction: federal vs. unitary event-time paths

Policy	$\ell = -5$	$\ell = -4$	$\ell = -3$	$\ell = -2$	$\ell = 0$	$\ell = 1$	$\ell = 2$	$\ell = 3$	$\ell = 4$	$\ell = 5$
FIT WIND PR	0.23***	0.11***	0.07*	-0.05**	0.11***	-0.00	-0.03	-0.05**	-0.06***	-0.06
	-0.04	-0.06**	0.06	-0.04	0.04	0.04	0.10*	0.05	0.00	0.02
FIT WIND DUR	0.04*	-0.03	0.01	-0.01	0.20***	0.04	0.08***	-0.00	-0.04	-0.03
	-0.07**	-0.06	-0.08*	-0.01	0.00	0.06	0.12	0.00	-0.02	-0.02
AUCTION WIND PR	0.19	0.31*	0.09	-0.01	0.01	0.16	0.26	0.14	-0.01	-0.02
	-0.06	-0.11	-0.02	0.36*	-0.03	-0.01	0.04	-0.06	-0.06	-0.01
AUCTION WIND DUR	0.08	0.30*	0.10	0.06	-0.02	-0.04	0.16	0.08	-0.05	0.03
	-0.11*	-0.05	-0.06	0.28	-0.08	-0.13**	-0.10	-0.14**	-0.14***	-0.22***
RECS	0.06	-0.11*	-0.13***	-0.07**	0.03	-0.10***	-0.02	-0.17***	-0.18***	-0.10***
	0.04	0.04	-0.08**	-0.07	0.04	-0.09**	-0.01	-0.04	0.00	0.04
RENEWABLE EXP	0.06	0.11**	0.07	0.02	-0.01	-0.07	-0.04	-0.09	-0.10	-0.06
	0.05	-0.02	-0.03	0.01	0.16*	0.05	0.03	0.01	-0.01	-0.03
RDD RES	0.20***	0.06	-0.15*	0.05	-0.14***	-0.08***	-0.02	0.07**	-0.04	-0.00
	0.02	0.02	-0.07*	0.00	-0.07***	0.00	-0.09***	-0.05**	-0.09***	-0.08***
FFS PRODUCER	0.47***	-0.15**	-0.12***	-0.15***	-0.23***	-0.15*	-0.10*	-0.35***	-0.20***	-0.25***
	-0.02	0.04	0.07	-0.01	0.74***	0.25***	0.23***	0.17***	0.19***	0.16***
ETS E PR	-0.05	-0.02	-0.03	0.00	-0.10**	-0.10*	-0.11**	-0.09**	-0.07	-0.02
	-0.06	-0.01	0.00	-0.01	-0.04	-0.02	-0.04	-0.06	-0.08	0.02
ETS E GHG	0.01	-0.02	0.01	0.09	-0.04	-0.09	-0.05	-0.02	0.00	-0.01
	0.11	-0.08	0.06	0.05	-0.04	-0.05	0.01	0.01	-0.00	-0.01
CARBONTAX E PR	0.52***	-0.03	-0.03	0.31***	-0.01	-0.05*	-0.06*	-0.13***	-0.10**	-0.01
	0.49***	-0.02	0.15***	-0.19***	0.11***	-0.06**	-0.07**	0.05*	0.03	-0.13***
FFS E	–	–	–	–	–	–	–	–	–	–
	–	–	–	–	–	–	–	–	–	–
EXCISETAX E NATGAS	–	–	–	–	–	–	–	–	–	–
	–	–	–	–	–	–	–	–	–	–
EXCISETAX E COAL	–	–	–	–	–	–	–	–	–	–
	–	–	–	–	–	–	–	–	–	–
PHOUT COAL DATE	-0.06	0.01	0.02	0.02	0.01	0.03	-0.03	0.02	-0.08	0.00
	-0.03	-0.05	0.01	0.04	-0.05	-0.01	-0.07	0.00	0.07	0.04
PHOUT COAL STAT	0.08	-0.14*	-0.20	-0.06	-0.17**	-0.11	-0.19	-0.21*	-0.25*	-0.16**
	-0.11	-0.15	0.01	0.03	0.00	-0.06	-0.15	-0.15	-0.03	0.00
BAN COAL DATE	0.12	-0.18	0.02	0.05	-0.05	-0.19	-0.09	-0.24	-0.04	-0.01
	0.03	-0.06	0.12**	0.14***	0.07	-0.08	-0.06	-0.06	0.13**	0.19***
BAN COAL STAT	0.12	-0.18	0.02	0.05	-0.05	-0.19	-0.09	-0.24	-0.04	-0.01
	0.03	-0.06	0.12**	0.14***	0.07	-0.08	-0.06	-0.06	0.13**	0.19***

Notes: Sun–Abraham interaction-weighted event study with federal-system interactions, as described in eq. (10). Each cell reports the federal marginal path $\nu_\ell + \theta_\ell$ on top and the unitary marginal path ν_ℓ below in grey. Stars test each marginal path against zero; they do not test equality between federal and unitary countries. The average federal–unitary interactions and their tests are reported in Table B.30. Standard errors are clustered by country. Stars: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. "–" indicates that no federal country adopted the policy, so the federal path and federal–unitary interaction are not identified; the table suppresses the unitary-only estimate in these rows.

Table B.30: Average federal-system interaction $\bar{\theta}$ by policy family

Tech.	Policy	Fed. adopters	$\bar{\theta}_{\text{post}}$ (s.e.)	$\bar{\theta}_{\text{pre}}$ (s.e.)
Solar	FIT SOL PR	7/32	-0.050 (0.059)	-0.161** (0.063)
Solar	FIT SOL DUR	5/28	-0.050 (0.066)	0.082* (0.049)
Solar	AUCTION SOL PR	7/28	0.061 (0.122)	-0.301*** (0.105)
Solar	AUCTION SOL DUR	7/29	-0.031 (0.125)	-0.412*** (0.120)
Solar	RECS	3/13	-0.065 (0.062)	0.120 (0.089)
Solar	RENEWABLE EXP	8/30	0.064 (0.060)	-0.002 (0.083)
Solar	RDD RES	6/28	0.082 (0.062)	0.037 (0.067)
Solar	FFS PRODUCER	5/17	-0.058 (0.074)	0.047 (0.091)
Solar	ETS E PR	6/35	-0.022 (0.055)	0.062 (0.042)
Solar	ETS E GHG	7/36	-0.011 (0.053)	0.125 (0.104)
Solar	CARBONTAX E PR	2/9	-0.175*** (0.045)	0.158*** (0.042)
Solar	FFS E	0/7	not identified	
Solar	EXCISETAX E NATGAS	0/1	not identified	
Solar	EXCISETAX E COAL	0/3	not identified	
Solar	PHOUT COAL DATE	2/20	-0.150 (0.119)	-0.229 (0.177)
Solar	PHOUT COAL STAT	5/36	-0.209** (0.094)	-0.164 (0.129)
Solar	BAN COAL DATE	6/32	-0.048 (0.121)	-0.195 (0.164)
Solar	BAN COAL STAT	6/32	-0.048 (0.121)	-0.195 (0.164)
Wind	FIT WIND PR	5/32	-0.059** (0.029)	0.110*** (0.027)
Wind	FIT WIND DUR	4/26	0.017 (0.036)	0.058* (0.032)
Wind	AUCTION WIND PR	5/19	0.113 (0.135)	0.103 (0.110)
Wind	AUCTION WIND DUR	5/16	0.162 (0.112)	0.121 (0.107)
Wind	RECS	3/13	-0.081** (0.031)	-0.044 (0.026)
Wind	RENEWABLE EXP	8/30	-0.098** (0.048)	0.063* (0.035)
Wind	RDD RES	6/28	0.027 (0.021)	0.044 (0.046)
Wind	FFS PRODUCER	5/17	-0.501*** (0.061)	-0.007 (0.037)
Wind	ETS E PR	6/35	-0.047** (0.021)	-0.004 (0.023)
Wind	ETS E GHG	7/36	-0.022 (0.022)	-0.013 (0.030)
Wind	CARBONTAX E PR	2/9	-0.051 (0.037)	0.082** (0.033)
Wind	FFS E	0/7	not identified	
Wind	EXCISETAX E NATGAS	0/1	not identified	
Wind	EXCISETAX E COAL	0/3	not identified	
Wind	PHOUT COAL DATE	2/20	-0.003 (0.053)	0.002 (0.075)
Wind	PHOUT COAL STAT	5/36	-0.120** (0.056)	-0.022 (0.047)
Wind	BAN COAL DATE	6/32	-0.136** (0.061)	-0.056 (0.069)
Wind	BAN COAL STAT	6/32	-0.136** (0.061)	-0.056 (0.069)

Notes: $\bar{\theta}_{\text{post}}$ is the equal-weight average of the federal–unitary interactions over $\ell \in \{0, \dots, 5\}$, while $\bar{\theta}_{\text{pre}}$ averages them over $\ell \in \{-5, \dots, -2\}$. Each average is tested as a single linear restriction using country-clustered standard errors. A significant post-event average indicates rejection of a zero average federal–unitary difference over the reported horizon; it does not establish that the complete dynamic paths differ at every horizon. A significant pre-event average indicates that the two groups were already following different trajectories before treatment and weakens a causal interpretation of the post-event difference. "Fed. adopters" reports the number of federal adopting countries relative to all adopting countries. Stars report conventional, unadjusted p-values: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Federal–unitary heterogeneity. Table B.28 and Table B.29 report the estimated event-time paths separately for federal and unitary countries. The interaction is estimable for fifteen of the eighteen policy families. It is not identified for FFS E, EXCISETAX E NATGAS, and EXCISETAX E COAL, because these policies have no federal adopters in the estimation sample.

We summarize the comparison using the average post-event interaction $\bar{\theta}_{\text{post}}$ in Table B.30. Ten of the thirty estimable policy–technology cases reject a zero average interaction at the conventional ten-percent level. The largest estimated differences occur for fossil-fuel producer support in the wind sample and electricity carbon pricing in the solar sample. Significant average differences also appear for wind FIT prices, renewable certificates, renewable expenditure, ETS prices, and some coal-exit measures.

These results require a cautious interpretation. First, statistical significance of a federal or unitary marginal path does not itself imply that the two paths differ; that comparison is provided by the interaction test. Second, failure to reject $\bar{\theta}_{\text{post}} = 0$ establishes only that the average post-event difference is imprecisely estimated. It does not demonstrate equality of the complete dynamic paths, since differences at individual horizons can offset one another in the average.

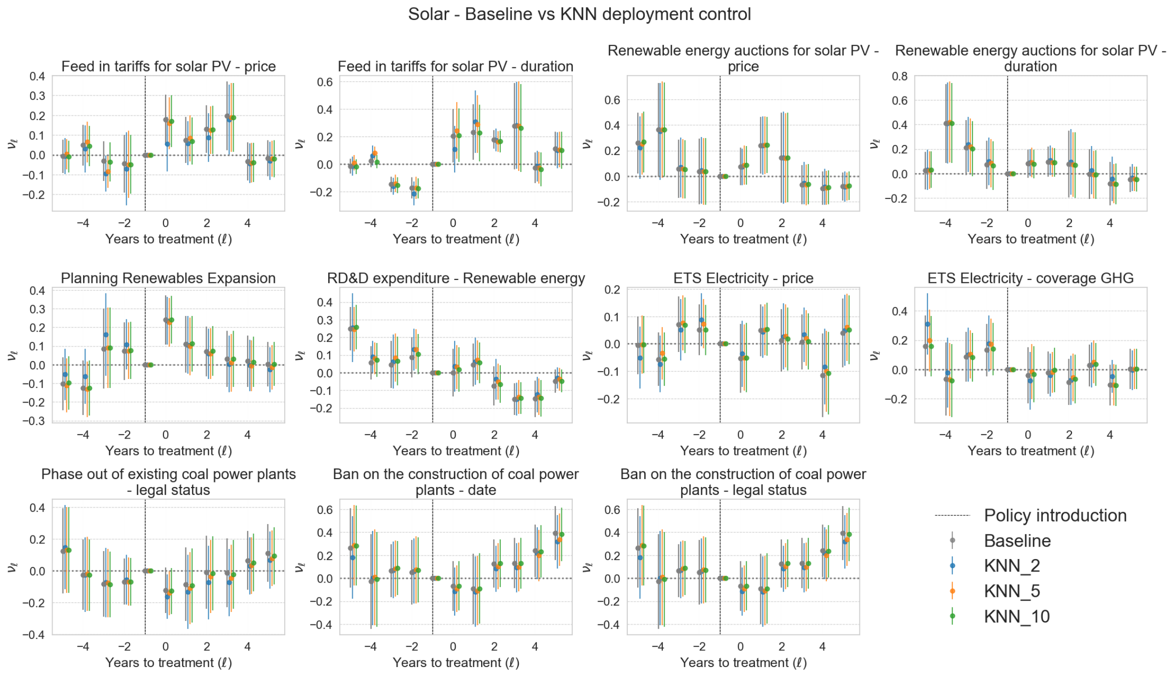
The pre-event interactions provide an additional qualification. Solar carbon-pricing reforms, wind FIT-price reforms, and wind renewable-expenditure reforms exhibit significant average interactions both before and after treatment. Their post-event differences may therefore reflect persistent differences in group-specific trends rather than a change associated with treatment. Solar auction reforms also display pronounced differential pre-event movements without a significant average post-event interaction.

Takeaway. The exercise provides descriptive evidence that national policy reforms are associated with different event-time paths in federal and unitary systems for a subset of policy–technology cases. It does not, however, identify the causal effect of federal institutions. Federal status may be correlated with adopter composition, reform timing, policy design and intensity, or unobserved subnational intervention.

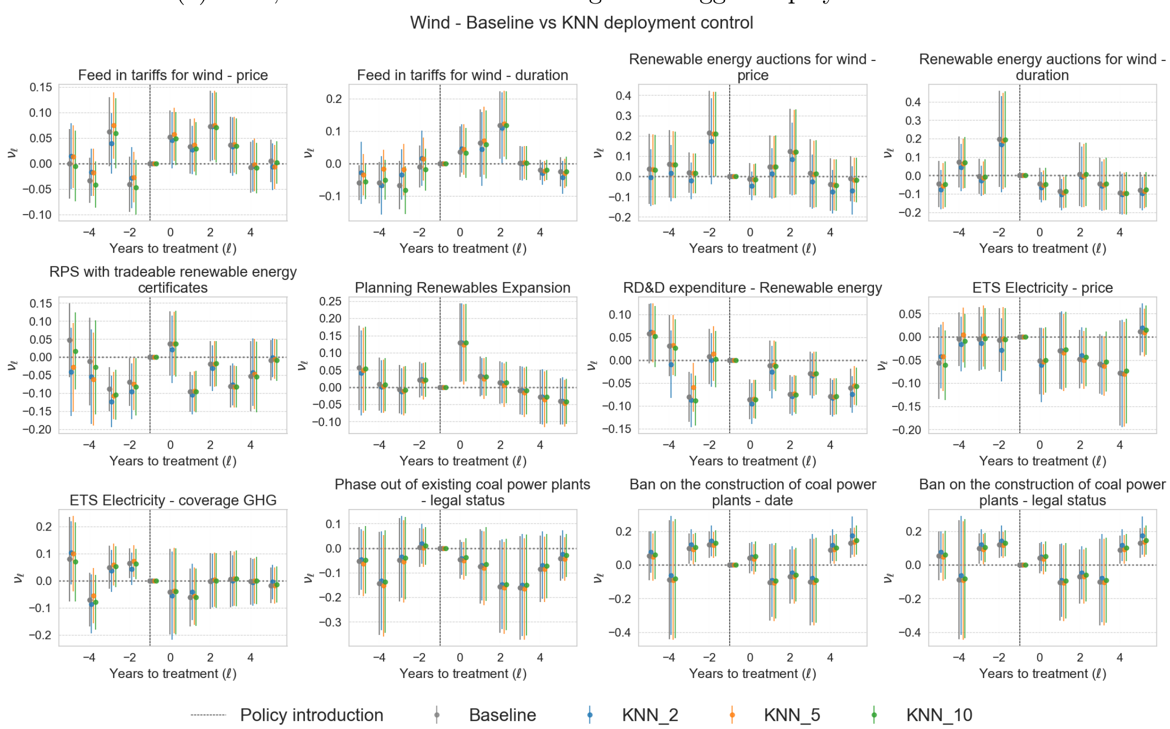
For most policy–technology cases, we do not reject a zero average post-event interaction. This suggests that the pooled estimates are not generally driven by a systematic federal–unitary difference, although it does not prove equality of the underlying dynamic paths. Where significant differences arise, they are policy specific rather than uniformly attenuated in federal systems.

The evidence is also limited by the small number of federal adopters, ranging from two to eight across the estimable policies, and by the number of comparisons conducted. The reported significance levels should consequently be viewed as conventional, unadjusted evidence. Overall, the results identify settings in which pooling federal and unitary countries may conceal meaningful heterogeneity, while offering more limited support for interpreting that heterogeneity as an institutional treatment effect.

B.7.9 Event study with K-nearest-neighbour lagged deployment control



(a) solar, Baseline vs. K-nearest-neighbour lagged deployment control



(b) wind, Baseline vs. K-nearest-neighbour lagged deployment control

Figure B.17: Sensitivity of event-study paths ν_ℓ to the inclusion of a K-nearest-neighbour lagged deployment control. For each country-year, the control is defined as the average lagged deployment of the focal technology among the K nearest neighbours, estimated separately for $K \in \{2, 5, 10\}$. Dots denote point estimates and whiskers denote 90% confidence intervals. The vertical dashed line marks policy onset. Grey = baseline; blue = KNN_2; orange = KNN_5; green = KNN_10.

As an additional robustness check, the event-study specification is augmented with a K-nearest-neighbour (KNN) lagged deployment control. The purpose is to assess whether the estimated policy effects are sensitive to cross-country diffusion patterns, such as imitation, demonstration, or other spillovers from deployment in similar countries, that are not explicitly captured by the baseline country-level covariates and policy controls.

Construction of the KNN control. For each country i, year t, technology T, and neighbourhood size K, let $\mathcal{N}_K(i,t)$ denote the set of the K nearest neighbours under the chosen distance metric. The neighbour-deployment control is then defined as

$$\text{KNNDeploy}_{it}^{(K)} = \frac{1}{K} \sum_{j \in \mathcal{N}_K(i,t)} \text{Deploy}_{j,t-1}^{(T)}, \tag{11}$$

that is, the average lagged deployment of the focal technology among the K nearest neighbours of country i. This variable is constructed separately for $K = 2$, $K = 5$, and $K = 10$, and each version is added one at a time to the baseline regression dataset. The KNN variable is included as a lagged covariate and is not interpreted causally; rather, it serves as a reduced-form control for cross-country diffusion dynamics.

The augmented specification is therefore

$$y_{it} = \sum_{\ell \neq -1} \nu_\ell \, g_{i\ell}(t) + \lambda_K \, \text{KNNDeploy}_{it}^{(K)} + X_{it}'\delta + \alpha_i + \gamma_t + \varepsilon_{it}, \tag{12}$$

where $g_{i\ell}(t)$ denotes the Sun–Abraham event-time terms, X_{it} contains the remaining baseline controls, and α_i and γ_t are country and year fixed effects. All other features of the design, including policy-event definitions, sample restrictions, covariates, and estimation window, remain unchanged.

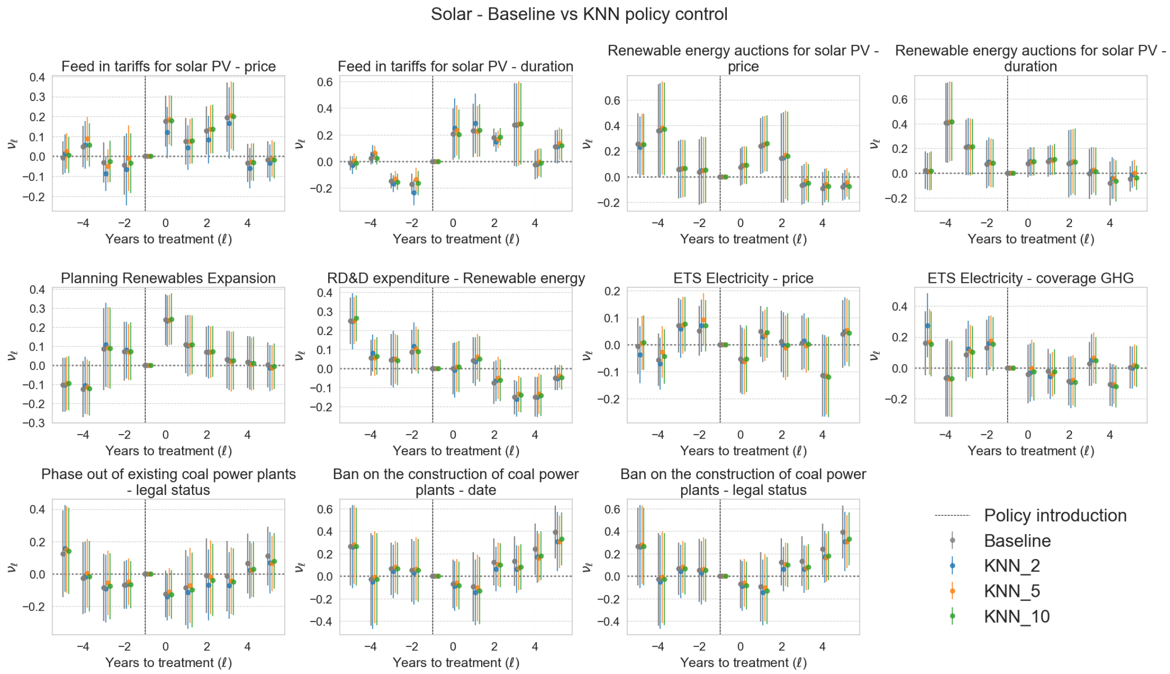
Findings. Figure B.17 shows that the estimated event-time paths are generally robust to the inclusion of lagged neighbour deployment. For solar, the paths under KNN_5 and KNN_10 almost always track the baseline closely. KNN_2 produces somewhat larger deviations for a small number of instruments, most notably the short-run response of FIT price and, to a lesser extent, some coal-policy measures. Even in these cases, however, the broader dynamic profile remains intact: positive near-term responses for subsidy-style instruments remain positive, and policies that are weak, delayed, or mixed in the baseline remain so once neighbour deployment is controlled for. The auction, planning, renewable-expansion, R&D, and ETS profiles are particularly stable across the three KNN variants.

For wind, the overlap is even tighter. Across feed-in tariffs, auctions, RECs, planning, R&D, ETS, and coal-related measures, the KNN-adjusted paths are nearly indistinguishable from the baseline for all three values of K. Some modest movements appear for the most local specification, KNN_2, particularly in a few pre-treatment leads and for certain auction or REC coefficients, but these shifts do not alter the sign, timing, or substantive interpretation of the post-treatment responses. The overall ranking of policy families remains unchanged.

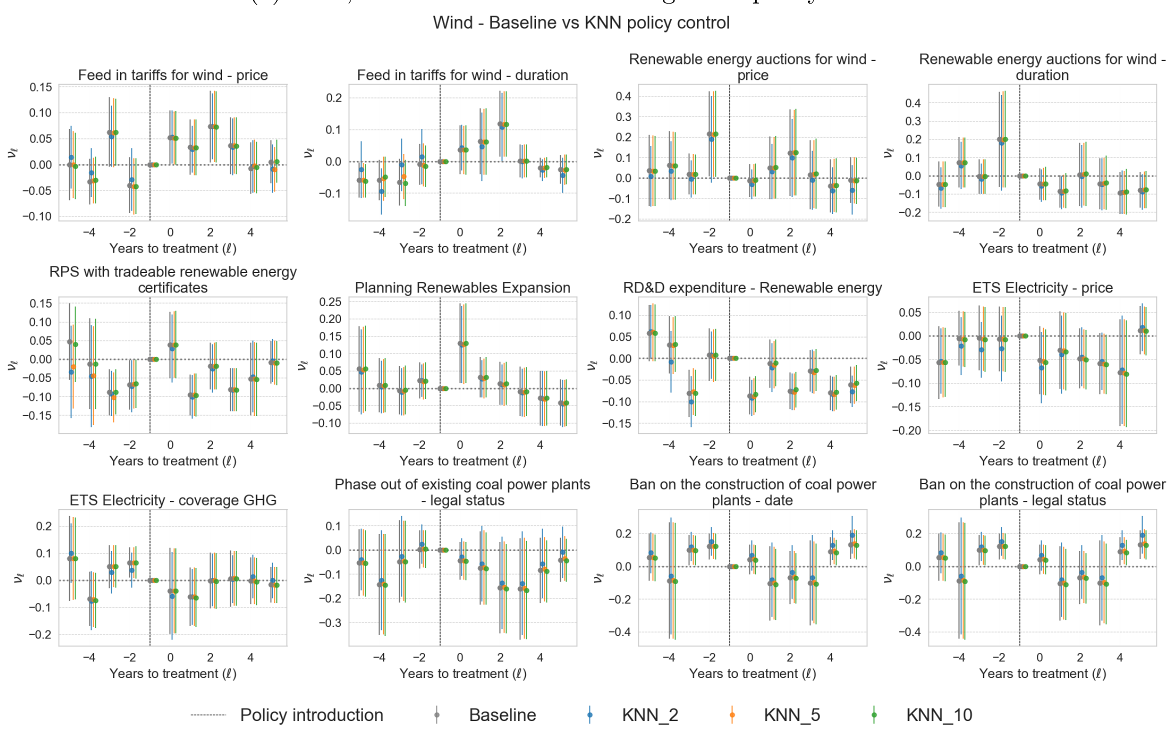
Interpretation. This robustness check suggests that the main results are not artifacts of omitted cross-country deployment spillovers. Conditioning on lagged deployment in similar countries does not materially change the estimated policy dynamics, especially once the neighbourhood is defined with $K = 5$ or $K = 10$. The slightly greater sensitivity of a few solar coefficients under KNN_2 is consistent with the most local neighbour control absorbing some highly localized diffusion dynamics, but not to an extent that overturns the substantive conclusions.

Takeaway. Including a K-nearest-neighbour lagged deployment control leaves the main event-study patterns largely unchanged for both solar and wind. The results therefore appear robust to conditioning on cross-country diffusion patterns, indicating that the baseline policy effects are not primarily driven by omitted neighbour-deployment spillovers.

B.7.10 Event study with K-nearest-neighbour policy control



(a) solar, Baseline vs. K-nearest-neighbour policy control



(b) wind, Baseline vs. K-nearest-neighbour policy control

Figure B.18: Sensitivity of event-study paths ν_ℓ to the inclusion of a K-nearest-neighbour policy control. For each country-year, the control is defined as the average policy stance for the focal policy among the K nearest neighbours, estimated separately for $K \in \{2, 5, 10\}$. Dots denote point estimates and whiskers denote 90% confidence intervals. The vertical dashed line marks policy onset. Grey = baseline; blue = KNN_2; orange = KNN_5; green = KNN_10.

As an additional robustness check, the event-study specification is augmented with a K-nearest-neighbour (KNN) policy control. The purpose is to assess whether the estimated policy effects are sensitive to cross-country policy diffusion patterns, such as imitation, coordination, or clustering of similar policy reforms across comparable countries, that are not explicitly captured by the baseline country-level covariates and policy controls.

Construction of the KNN control. For each country i, year t, focal policy family m, and neighbourhood size K, let $\mathcal{N}_K(i,t)$ denote the set of the K nearest neighbours under the chosen distance metric. The neighbour-policy control is then defined as

$$\text{KNNPolicy}_{it}^{(K,m)} = \frac{1}{K} \sum_{j \in \mathcal{N}_K(i,t)} P_{jt}^{(m)}, \tag{13}$$

that is, the average policy stance for the focal policy family among the K nearest neighbours of country i. This variable is constructed separately for $K = 2$, $K = 5$, and $K = 10$, and each version is added one at a time to the baseline regression dataset. The KNN policy variable is included as an additional control and is not interpreted causally; rather, it serves as a reduced-form control for cross-country policy diffusion dynamics.

The augmented specification is therefore

$$y_{it} = \sum_{\ell \neq -1} \nu_\ell \, g_{i\ell}(t) + \lambda_K \, \text{KNNPolicy}_{it}^{(K,m)} + X_{it}'\delta + \alpha_i + \gamma_t + \varepsilon_{it}, \tag{14}$$

where $g_{i\ell}(t)$ denotes the Sun–Abraham event-time terms, X_{it} contains the remaining baseline controls, and α_i and γ_t are country and year fixed effects. All other features of the design, including policy-event definitions, sample restrictions, covariates, and estimation window, remain unchanged.

Findings. Figure B.18 shows that the estimated event-time paths are generally robust to the inclusion of neighbour-policy controls. For solar, the paths under KNN_5 and KNN_10 again track the baseline closely for most instruments and horizons. KNN_2 generates somewhat larger movements for a small number of policies, particularly for the short-run responses of FIT price and duration, and for a few tax and coal-policy measures. Even there, the broader dynamic profile remains intact: subsidy-style instruments continue to show positive near-term responses, while policies that are weak, delayed, or mixed in the baseline remain so once neighbour-policy spillovers are controlled for. Auction, planning, renewable-expansion, R&D, and ETS profiles are especially stable across the three KNN variants.

For wind, the overlap is again very tight. Across feed-in tariffs, auctions, RECs, planning, R&D, ETS, and coal-related measures, the KNN-policy-adjusted paths are nearly indistinguishable from the baseline for all three values of K. Some modest movements appear for the most local specification, KNN_2, particularly for a few pre-treatment leads and selected auction, fossil-fuel, or excise-tax coefficients, but these do not alter the sign, timing, or substantive interpretation of the post-treatment responses. The overall ranking of policy families remains unchanged.

Interpretation. This robustness check suggests that the main results are not artifacts of omitted cross-country policy spillovers or coordinated policy adoption among similar countries. Conditioning on the policy stance of neighbouring countries does not materially change the estimated policy dynamics, especially once the neighbourhood is defined with $K = 5$ or $K = 10$. The slightly greater sensitivity of a few solar coefficients under KNN_2 is consistent with the most local neighbour control absorbing some highly localized policy clustering, but not to an extent that overturns the substantive conclusions.

Takeaway. Including a K-nearest-neighbour policy control leaves the main event-study patterns largely unchanged for both solar and wind. The results therefore appear robust to conditioning on cross-country policy diffusion patterns, indicating that the baseline policy effects are not primarily driven by omitted neighbour-policy spillovers.

B.8 Alternative event definition

This appendix probes whether our main results depend on how we *define* policy "events." In the baseline, we mark *Significant–Gradual–2y* (`SG`) activations: a two-point or larger rise in policy stringency cumulated over two years ($\tilde{\eta} \geq 2,\ \tilde{\tau} = 2$). Holding the estimator and controls fixed (Sun–Abraham imputation-weighted event study with country and year fixed effects, identical trimming and cohort windows, $\ell \in [-5, 5]$ with $\ell = -1$ omitted, country-clustered SEs, and the same risk sets of not-yet-treated and never-treated units), we re-map cohorts in four ways:

1. **Significant–Abrupt–1y (`SA`).** Tightens the horizon to one year ($\tilde{\tau} = 1$) while retaining the $\tilde{\eta} \geq 2$ threshold. This isolates sharp jumps and tests whether baseline patterns are an artifact of the two-year accumulation.

2. **Dose–response weighting.** We weight the event-time dummies by the realised event size η_e, yielding ν_ℓ interpretable *per one-point* change on the stringency scale (linear dose–response test).

3. **Any-change–1y (`AC`).** Lowers the threshold to any non-zero one-year change ($\tilde{\eta} > 0,\ \tilde{\tau} = 1$), trading higher sample size for lower signal-to-noise and potentially more anticipatory "churn."

4. **First significant event.** Keeps only the *first* positive `SG` activation per country (by L4 family or L3 category) to strip out within-country ratchets and foreground the initial rollout.

Across subsections, we emphasise the eight core policy families discussed in the main text (FIT price, FIT duration, auctions price, auctions duration, planning, public R&D, coal ban *date*, coal ban *status*); other families are reported for completeness where informative. Tables place the alternative estimate on the *top* line of each cell and reproduce the baseline `SG` estimate on the *bottom* line (grey) for side-by-side comparison; figures overlay paths with identical axes and confidence bands.

Diagnostics and interpretation. For each specification we report joint-Wald tests on pre-treatment leads ($\ell \in \{-5, -4, -3, -2\}$). Where pre-trends are rejected, we treat the dynamic profile as *timing evidence only* and avoid level claims. Two recurring trade-offs guide interpretation: (i) `SA` and "first-event" designs sharpen timing but reduce power, making pre-trend tests more brittle; (ii) `AC` increases coverage but can introduce benign anticipation and measurement noise. Within this framework, robustness is judged by stability of signs, peak timing (typically $\ell \in \{0, +1, +2\}$ for deployment levers), and policy ordering across definitions.

B.8.1 Significant-Abrupt-1yr events

Analysis limited to sharp events only

Table B.31: Solar, sharp significant policy events

Policy	$\ell = -5$	$\ell = -4$	$\ell = -3$	$\ell = -2$	$\ell = 0$	$\ell = 1$	$\ell = 2$	$\ell = 3$	$\ell = 4$	$\ell = 5$
FIT SOL PR	-0.03**	0.05*	-0.02*	0.02*	0.22*	0.09*	0.09*	0.17*	-0.06*	-0.03*
	-0.01*	0.05*	-0.03*	-0.05*	0.18*	0.07*	0.13*	0.20	-0.03*	-0.02*
FIT SOL DUR	0.00**	0.02**	-0.15**	-0.16**	0.23	0.23	0.17**	0.26	-0.01*	0.10*
	-0.01**	0.02**	-0.15**	-0.17**	0.21	0.23	0.18**	0.28	-0.03*	0.11*
AUCTION SOL PR	0.14	0.28	0.04	0.02	0.04*	0.06*	0.04	-0.11*	-0.10*	-0.09*
	0.26	0.36	0.06	0.04	0.08*	0.24	0.14	-0.07*	-0.09*	-0.08*
AUCTION SOL DUR	0.02*	0.20	0.09	-0.00	0.01*	0.01*	-0.00	-0.06*	-0.10*	-0.11*
	0.02*	0.41	0.21	0.07	0.08*	0.10*	0.08	-0.00	-0.08	-0.05*
RECS	-0.12*	0.04	-0.13	-0.01	-0.19*	-0.23*	-0.00**	-0.23*	-0.18*	-0.07*
	-0.12*	0.04	-0.13	-0.01	-0.17*	-0.22*	0.00**	-0.22*	-0.17*	-0.07*
RENEWABLE EXP	-0.10*	-0.11*	0.09	0.03*	0.20*	0.10*	0.06*	0.01*	0.02*	0.00*
	-0.10*	-0.12*	0.09	0.07*	0.24*	0.11*	0.07*	0.03*	0.02*	0.00*
RDD RES	0.11**	0.01**	-0.04*	0.03*	0.05*	0.10*	-0.00*	-0.12**	-0.11*	-0.07**
	0.25*	0.06*	0.05*	0.09*	-0.00*	0.04*	-0.07*	-0.15*	-0.15*	-0.05**
FFS PRODUCER	0.19	0.01*	0.07*	0.15*	0.30	0.06*	-0.05*	0.00*	0.03*	-0.00*
	0.32	-0.04	0.17	0.22*	0.62	0.18	-0.10*	-0.06*	-0.00*	0.04*
ETS E PR	-0.01*	-0.05*	-0.00*	0.02*	0.04*	0.02*	0.01*	-0.03*	0.06*	-0.01*
	-0.01*	-0.06*	0.07*	0.05*	-0.05*	0.05*	0.01*	0.01*	-0.11*	0.04*
ETS E GHG	0.12	0.02	0.13	0.03	-0.09	-0.06*	-0.04*	0.03*	-0.12*	-0.04*
	0.16	-0.07	0.08	0.13	-0.04	-0.02*	-0.09*	0.03*	-0.11*	0.00*
CARBONTAX E PR	-0.33*	-0.34*	-0.26*	-0.07*	0.42	-0.08*	0.07	0.07	0.05	-0.07*
	-0.33*	-0.32*	-0.24**	-0.09*	0.41	-0.07*	0.08	0.09	0.05	-0.08
FFS E	-0.13*	0.02*	0.53	0.07	0.35**	-0.03*	0.02*	0.09*	0.14**	0.01**
	-0.28*	-0.08*	1.36*	-0.12*	1.27*	-0.06*	-0.22*	0.10*	-0.10*	-0.13*
EXCISETAX E NATGAS	0.05*	-0.11	-0.16	-0.43	-0.53	0.08	-0.01	-0.11*	0.15*	0.07*
	0.05*	-0.11	-0.16	-0.43	-0.53	0.08	-0.01	-0.11*	0.15*	0.07*
EXCISETAX E COAL	0.05*	-0.11	-0.16	-0.43	-0.53	0.08	-0.01	-0.11*	0.15*	0.07*
	0.04*	-0.11	-0.16	-0.43	-0.53	0.08	-0.01	-0.11*	0.15*	0.07*
PHOUT COAL DATE	0.12	0.15	-0.05*	0.07*	0.06*	0.10	0.10*	0.09*	0.12*	0.19
	0.11	0.12*	-0.08*	0.06*	0.04*	0.08	0.09*	0.06*	0.09*	0.17*
PHOUT COAL STAT	0.19	0.02	-0.10	-0.06*	-0.13*	-0.08	0.00	0.02	0.10	0.14
	0.12	-0.02	-0.08	-0.07*	-0.12*	-0.09	-0.01	-0.01	0.06	0.11
BAN COAL DATE	0.26	-0.03	0.07	0.05	-0.07	-0.09	0.13	0.13	0.24	0.39
	0.26	-0.03	0.07	0.05	-0.07	-0.09	0.13	0.13	0.24	0.39
BAN COAL STAT	0.26	-0.03	0.07	0.05	-0.07	-0.09	0.13	0.13	0.24	0.39
	0.26	-0.03	0.07	0.05	-0.07	-0.09	0.13	0.13	0.24	0.39

Notes: Event-time coefficients (Sun–Abraham IW). Coefficients are relative to the year before treatment ($\ell = -1$ omitted).
Stars: * p<0.10, ** p<0.05, *** p<0.01. 95% confidence intervals.
Each cell shows the robustness specification (top) and the baseline (bottom, grey).

Table B.32: Wind, Sharp significant policy events

Policy	$\ell=-5$	$\ell=-4$	$\ell=-3$	$\ell=-2$	$\ell=0$	$\ell=1$	$\ell=2$	$\ell=3$	$\ell=4$	$\ell=5$
FIT WIND PR	0.01** -0.00**	-0.02** -0.03**	0.06** 0.06**	-0.04** -0.04**	0.05** 0.05**	0.03** 0.03**	0.07** 0.07**	0.03** 0.04**	0.01** -0.01**	0.00** 0.00**
FIT WIND DUR	-0.06** -0.06**	-0.06** -0.06**	-0.07** -0.07**	-0.02** -0.01**	0.04** 0.04**	0.06* 0.06*	0.12* 0.12*	0.01** 0.00**	-0.02** -0.02**	-0.02** -0.03**
AUCTION WIND PR	0.00* 0.04	0.08* 0.06	-0.01* 0.02*	0.22 0.21	-0.02** -0.01**	-0.03* 0.05*	0.08 0.12	-0.03* 0.02	-0.08* -0.04*	-0.04* -0.01*
AUCTION WIND DUR	-0.05* -0.05*	0.07* 0.07*	-0.00* -0.01*	0.20 0.20	-0.05* -0.05*	-0.08* -0.09*	0.01 0.01	-0.04* -0.05*	-0.09* -0.10*	-0.08* -0.08*
RECS	0.05* 0.05*	-0.01* -0.01*	-0.09** -0.09**	-0.07** -0.07**	0.03* 0.04*	-0.10** -0.10**	-0.02** -0.02**	-0.08** -0.08**	-0.06* -0.05*	-0.01** -0.01**
RENEWABLE EXP	0.04* 0.06*	0.00** 0.01**	-0.01** -0.01**	0.01** 0.02**	0.11* 0.13*	0.02** 0.03**	0.02** 0.01**	-0.01** -0.01**	-0.02** -0.03**	-0.04** -0.04**
RDD RES	0.02** 0.06**	0.07** 0.03**	-0.07** -0.08**	0.06** 0.01**	-0.08** -0.09**	-0.01** -0.01**	-0.08** -0.08**	-0.04** -0.03**	-0.04** -0.08**	-0.05** -0.06**
FFS PRODUCER	0.11** 0.14	-0.00** -0.03**	-0.00** 0.00**	-0.00** -0.06**	0.20 0.46**	0.04* 0.14**	0.07* 0.14**	-0.01* 0.03**	0.03* 0.08**	-0.06** 0.04**
ETS E PR	-0.05** -0.06**	-0.01** -0.01**	0.03** -0.00**	-0.04** -0.01**	-0.05** -0.05**	-0.07* -0.03*	-0.03** -0.05**	-0.07** -0.06**	-0.04* -0.08*	-0.02** 0.01**
ETS E GHG	0.07* 0.08*	-0.06* -0.07*	0.08** 0.05**	0.07** 0.06**	-0.04* -0.04*	-0.06* -0.06*	0.00* -0.00*	0.00* 0.01*	-0.00* -0.00*	-0.02** -0.02**
CARBONTAX E PR	0.49** 0.49**	-0.00* -0.00*	0.09** 0.09**	0.05 0.04	0.08* 0.07*	-0.03** -0.04**	-0.05** -0.05**	-0.01** -0.01**	-0.01** -0.01**	-0.08** -0.07**
FFS E	-0.02** 0.05**	-0.01** 0.03**	-0.05** 0.07*	-0.00** 0.19*	-0.02** 0.11*	-0.03** 0.22*	-0.06** 0.13**	-0.05** 0.21*	-0.04** 0.18*	-0.03** 0.13**
EXCISETAX E NATGAS	-0.16* -0.16*	0.14** 0.14**	0.05* 0.05*	0.04* 0.04*	0.13** 0.13**	0.06* 0.06*	-0.05* -0.05*	0.07** 0.07**	0.00** 0.00**	0.02** 0.02**
EXCISETAX E COAL	-0.16* -0.16*	0.14** 0.14**	0.05* 0.05*	0.04* 0.04*	0.13** 0.13**	0.06* 0.06*	-0.05* -0.05*	0.07** 0.07**	0.00** 0.00**	0.02** 0.02**
PHOUT COAL DATE	-0.04* -0.04*	-0.04* -0.04*	0.01* 0.01*	0.03* 0.03*	-0.04** -0.04**	0.00** 0.00**	-0.06* -0.06*	0.01* 0.01*	0.04* 0.04*	0.02** 0.03**
PHOUT COAL STAT	-0.06* -0.05*	-0.15 -0.14	-0.06 -0.05	0.00** 0.00**	-0.04** -0.05**	-0.09* -0.07*	-0.17 -0.16	-0.17 -0.16	-0.09* -0.08*	-0.05* -0.04*
BAN COAL DATE	0.05* 0.05*	-0.09 -0.09	0.10* 0.10*	0.12** 0.12**	0.04* 0.04*	-0.10 -0.10	-0.07* -0.07*	-0.10 -0.10	0.09* 0.09*	0.13* 0.13*
BAN COAL STAT	0.05* 0.05*	-0.09 -0.09	0.10* 0.10*	0.12** 0.12**	0.04* 0.04*	-0.10 -0.10	-0.07* -0.07*	-0.10 -0.10	0.09* 0.09*	0.13* 0.13*

Notes: Event-time coefficients (Sun–Abraham IW). Coefficients are relative to the year before treatment ($\ell = -1$ omitted).
Stars: * p<0.10, ** p<0.05, *** p<0.01. 95% confidence intervals.
Each cell shows the robustness specification (top) and the baseline (bottom, grey).

We tighten the event definition to isolate abrupt, substantial policy moves by restricting to *Significant–Abrupt–1y* events (`SA`: $\tilde{\eta} = 2$, $\tilde{\tau} = 1$; cf. Methods). All modelling choices are held fixed (same Sun–Abraham imputation–weighted estimator, controls, fixed effects, trimming, and cohort window). This robustness asks whether our baseline results hinge on using a two–year evaluation horizon. In the tables below, the top entry in each cell reports the `SA` estimate and the bottom (grey) reproduces the baseline `SG` (*Significant–Gradual–2y*) estimate for reference; coefficients are relative to $\ell = -1$.

Solar. Table B.31 shows that the core patterns persist under `SA`. Feed–in Tariff (Price) remains clearly positive around treatment, with slightly more front-loaded responses at $\ell = 0$–$+1$ relative to the `SG` baseline. FIT (Duration) similarly preserves its post-treatment rise, alongside the same pre-treatment dips at $\ell = -3, -2$, consistent with either anticipation or sample composition rather than an artifact of the two–year window. Auction effects stay modest and somewhat transient (small positives near $\ell = 0$ with attenuation by $\ell = +3$), while RECs continue to be weak or negative post-treatment. Planning for renewables expansion retains small, steady positives. ETS (Electricity) remains mixed and close to zero in economic terms. Families with few treated countries, notably Carbon Tax (Electricity), show the same fragility as in the baseline (visible negative leads and wide intervals) without flipping the post-treatment interpretation.

Wind. As in solar, Table B.32 confirms stable signs and timing. FIT policies preserve small, consistent positives through $\ell = +1$–$+2$; auctions remain moderate and short-lived; RECs are again weak/negative in the post period; planning retains small positives; ETS coefficients are modestly negative or near zero; and small-N families exhibit wide intervals but no systematic sign reversals relative to the `SG` baseline.

Takeaway. Limiting attention to sharp one–year policy jumps leaves our qualitative conclusions intact and, if anything, makes some responses appear slightly more immediate (front-loaded) while preserving magnitudes at the headline lags. The baseline results are therefore not an artifact of the two–year event construction; effects attributed to FIT, auctions, planning, and RECs/ETS behave similarly under `SA`. Any local sensitivities align with small effective sample sizes rather than the choice of event horizon.

B.8.2 Dose-response: weighting by event size

Table B.33: Solar, regression weighted by event size (Dosal response)

Policy	$\ell = -5$	$\ell = -4$	$\ell = -3$	$\ell = -2$	$\ell = 0$	$\ell = 1$	$\ell = 2$	$\ell = 3$	$\ell = 4$	$\ell = 5$
FIT SOL PR	0.01** -0.01*	0.00** 0.05*	-0.02** -0.03*	-0.03** -0.05*	0.03** 0.18*	0.01** 0.07*	0.03** 0.13*	0.04** 0.20	-0.00** -0.03*	-0.01** -0.02*
FIT SOL DUR	0.01*** -0.01**	0.02*** 0.02**	-0.02*** -0.15**	-0.03** -0.17**	0.04** 0.21	0.05** 0.23	0.04** 0.18**	0.06** 0.28	-0.00** -0.03*	0.03** 0.11*
AUCTION SOL PR	0.03** 0.26	0.02** 0.36	-0.01** 0.06	-0.01** 0.04	0.01** 0.08*	0.02** 0.24	-0.01** 0.14	-0.03** -0.07*	-0.03** -0.09*	-0.02** -0.08*
AUCTION SOL DUR	0.00** 0.02*	0.10* 0.41	0.06** 0.21	0.01** 0.07	0.01** 0.08*	0.02** 0.10*	0.01** 0.08	-0.00* -0.00	-0.06** -0.08	-0.02** -0.05*
RECS	-0.03** -0.12*	0.01** 0.04	-0.05** -0.13	-0.01** -0.01	-0.05** -0.17*	-0.07** -0.22*	-0.00** 0.00**	-0.07** -0.22*	-0.05** -0.17*	-0.01** -0.07*
RENEWABLE EXP	-0.03** -0.10*	-0.03** -0.12*	0.05** 0.09	0.04** 0.07*	0.07** 0.24*	0.01** 0.11*	0.01** 0.07*	-0.00** 0.03*	0.00** 0.02*	0.00** 0.00*
RDD RES	0.09** 0.25*	0.02** 0.06*	0.03** 0.05*	0.02** 0.09*	-0.01** -0.00*	-0.01** 0.04*	-0.02** -0.07*	-0.03** -0.15*	-0.04** -0.15*	-0.01** -0.05**
FFS PRODUCER	0.17** 0.32	0.00** -0.04	0.06** 0.17	0.07** 0.22*	0.12** 0.62	0.04** 0.18	-0.01** -0.10*	-0.01** -0.06*	0.01** -0.00*	0.01** 0.04*
ETS E PR	-0.00** -0.01*	-0.02** -0.06*	-0.00** 0.07*	0.01** 0.05*	-0.01** -0.05*	0.01** 0.05*	-0.00** 0.01*	-0.01** 0.01*	-0.05** -0.11*	0.01** 0.04*
ETS E GHG	0.03** 0.16	-0.01** -0.07	0.02** 0.08	0.02** 0.13	0.02** -0.04	-0.00** -0.02*	-0.02** -0.09*	-0.00** 0.03*	-0.03** -0.11*	-0.01** 0.00*
CARBONTAX E PR	-0.08** -0.33*	-0.10** -0.32*	-0.07** -0.24**	-0.02** -0.09*	0.11** 0.41	-0.02** -0.07*	0.01** 0.08	0.01** 0.09	-0.00** 0.05	-0.04** -0.08
FFS E	-0.14** -0.28*	-0.04** -0.08*	0.68** 1.36*	-0.06** -0.12*	0.66** 1.27*	-0.03** -0.06*	-0.08** -0.22*	0.02** 0.10*	-0.02** -0.10*	-0.03** -0.13*
EXCISETAX E NATGAS	0.02** 0.05*	-0.04** -0.11	-0.05** -0.16	-0.14** -0.43	-0.18** -0.53	0.03** 0.08	-0.00** -0.01	-0.04** -0.11*	0.05** 0.15*	0.02** 0.07*
EXCISETAX E COAL	0.02** 0.04*	-0.05* -0.11	-0.08* -0.16	-0.22* -0.43	-0.26* -0.53	0.04* 0.08	-0.00* -0.01	-0.06** -0.11*	0.08** 0.15*	0.04** 0.07*
PHOUT COAL DATE	0.03** 0.11	0.04** 0.12*	-0.02** -0.08*	0.01** 0.06*	0.01** 0.04*	0.04** 0.08	0.02** 0.09*	0.02** 0.06*	0.02** 0.09*	0.04** 0.17*
PHOUT COAL STAT	0.04* 0.12	0.01** -0.02	-0.01** -0.08	-0.01** -0.07*	-0.02** -0.12*	-0.01** -0.09	0.01** -0.01	0.00** -0.01	0.01** 0.06	0.02** 0.11
BAN COAL DATE	0.03** 0.26	-0.00** -0.03	0.01** 0.07	0.00** 0.05	-0.01** -0.07	-0.01** -0.09	0.01** 0.13	0.01** 0.13	0.03** 0.24	0.04** 0.39
BAN COAL STAT	0.05** 0.26	-0.00** -0.03	0.03** 0.07	0.02** 0.05	-0.01** -0.07	-0.01** -0.09	0.02** 0.13	0.02** 0.13	0.05** 0.24	0.08** 0.39

Notes: Event-time coefficients (Sun–Abraham IW). Coefficients are relative to the year before treatment ($\ell = -1$ omitted).
Stars: * p<0.10, ** p<0.05, *** p<0.01. 95% confidence intervals.
Each cell shows the robustness specification (top) and the baseline (bottom, grey).

Table B.34: Wind, regression weighted by event size (Dosal response)

Policy	$\ell = -5$	$\ell = -4$	$\ell = -3$	$\ell = -2$	$\ell = 0$	$\ell = 1$	$\ell = 2$	$\ell = 3$	$\ell = 4$	$\ell = 5$
FIT WIND PR	-0.00** -0.00**	-0.02*** -0.03**	0.00*** 0.06**	-0.02** -0.04**	0.01*** 0.05**	0.00*** 0.03**	0.01*** 0.07**	0.01*** 0.04**	-0.00*** -0.01**	-0.00*** 0.00**
FIT WIND DUR	-0.01*** -0.06**	-0.01*** -0.06**	-0.01*** -0.07**	-0.00*** -0.01**	0.01*** 0.04**	0.01*** 0.06*	0.02** 0.12*	0.00*** 0.00**	-0.01*** -0.02**	-0.00*** -0.03**
AUCTION WIND PR	0.01** 0.04	0.01** 0.06	0.00*** 0.02*	0.03** 0.21	-0.00*** -0.01**	0.01** 0.05*	0.02** 0.12	0.00** 0.02	-0.01** -0.04*	-0.00** -0.01*
AUCTION WIND DUR	-0.02** -0.05*	0.02** 0.07*	-0.01** -0.01*	0.06* 0.20	-0.02** -0.05*	-0.03** -0.09*	-0.00** 0.01	-0.03** -0.05*	-0.05** -0.10*	-0.04** -0.08*
RECS	0.02** 0.05*	-0.01** -0.01*	-0.03** -0.09**	-0.03*** -0.07**	-0.00** 0.04*	-0.03*** -0.10**	-0.01** -0.02**	-0.02** -0.08**	-0.01** -0.05*	0.00*** -0.01**
RENEWABLE EXP	0.01** 0.06*	-0.01** 0.01**	-0.01** -0.01**	-0.00** 0.02**	0.03** 0.13*	0.00*** 0.03**	-0.00*** 0.01**	-0.00*** -0.01**	-0.00*** -0.03**	-0.01*** -0.04**
RDD RES	0.02** 0.06**	0.02** 0.03**	-0.03*** -0.08**	-0.01** 0.01**	-0.02*** -0.09**	0.01*** -0.01**	-0.01*** -0.08**	0.01** -0.03**	-0.02*** -0.08**	-0.02*** -0.06**
FFS PRODUCER	0.04** 0.14	0.00*** -0.03**	0.00*** 0.00**	-0.01*** -0.06**	0.08*** 0.46**	0.03*** 0.14**	0.03*** 0.14**	0.00*** 0.03**	0.01*** 0.08**	0.01*** 0.04**
ETS E PR	-0.02** -0.06**	-0.01*** -0.01**	-0.00*** -0.00**	-0.00*** -0.01**	-0.01** -0.05**	-0.00*** -0.03*	-0.01** -0.05**	-0.02** -0.06**	-0.03** -0.08*	0.00** 0.01**
ETS E GHG	0.01** 0.08*	-0.01*** -0.07*	0.01*** 0.05**	0.01*** 0.06**	-0.01** -0.04*	-0.01** -0.06*	-0.00** -0.00*	0.00** 0.01*	-0.00** -0.00*	-0.01*** -0.02**
CARBONTAX E PR	0.24** 0.49**	0.01*** -0.00*	0.06*** 0.09**	-0.03** 0.04	0.03*** 0.07*	-0.01*** -0.04**	-0.01*** -0.05**	0.02*** -0.01**	0.01*** -0.01**	-0.03** -0.07**
FFS E	0.02** 0.05**	0.01** 0.03**	0.03** 0.07*	0.10** 0.19*	0.04** 0.11*	0.07** 0.22*	0.04** 0.13**	0.04** 0.21*	0.04** 0.18*	0.03*** 0.13**
EXCISETAX E NATGAS	-0.05** -0.16*	0.05** 0.14**	0.02** 0.05*	0.01** 0.04*	0.04** 0.13**	0.02** 0.06*	-0.02** -0.05*	0.02** 0.07**	0.00** 0.00**	0.01** 0.02**
EXCISETAX E COAL	-0.08** -0.16*	0.07** 0.14**	0.02** 0.05*	0.02** 0.04*	0.07** 0.13**	0.03** 0.06*	-0.03** -0.05*	0.04** 0.07**	0.00** 0.00**	0.01** 0.02**
PHOUT COAL DATE	-0.02** -0.04*	-0.01** -0.04*	-0.00** 0.01*	-0.00** 0.03*	-0.02** -0.04**	-0.00** 0.00**	-0.02** -0.06*	-0.01** 0.01*	0.00** 0.04*	0.00** 0.03**
PHOUT COAL STAT	0.00** -0.05*	-0.04** -0.14	-0.00** -0.05	0.00** 0.00**	-0.00** -0.05**	-0.01** -0.07*	-0.02** -0.16	-0.02** -0.16	-0.01** -0.08*	-0.00*** -0.04*
BAN COAL DATE	0.00*** 0.05*	-0.01** -0.09	0.01*** 0.10*	0.01*** 0.12**	0.00*** 0.04*	-0.01** -0.10	-0.01** -0.07*	-0.01** -0.10	0.01*** 0.09*	0.01*** 0.13*
BAN COAL STAT	0.01** 0.05*	-0.02** -0.09	0.02** 0.10*	0.02*** 0.12**	0.01** 0.04*	-0.02** -0.10	-0.01** -0.07*	-0.02** -0.10	0.02** 0.09*	0.03** 0.13*

Notes: Event-time coefficients (Sun–Abraham IW). Coefficients are relative to the year before treatment ($\ell = -1$ omitted).
Stars: * p<0.10, ** p<0.05, *** p<0.01. 95% confidence intervals.
Each cell shows the robustness specification (top) and the baseline (bottom, grey).

We test for dose response by allowing event-time effects to scale with the *size* of the policy change. Let $g(e)$ denote country e's event (cohort) year and $\eta_e \equiv P_e(g) - P_e(g - \tilde{\tau})$ the (signed) change in stringency over the event window (Methods). Define the lead/lag indicators $D_{e,t}^{(\ell)} = \mathbf{1}\{t - e = \ell\}$ and their event-size–weighted counterparts $W_{e,t}^{(\ell)} = \eta_c D_{e,t}^{(\ell)}$. We then estimate

$$y_{c,t} \;=\; \alpha_e + \gamma_t + X'_{e,t}\beta + \sum_{\ell \neq -1} \nu_\ell \, W_{e,t}^{(\ell)} + \varepsilon_{e,t}. \tag{15}$$

Because $W_{e,t}^{(\ell)}$ embeds both the magnitude and sign of the event, ν_ℓ is a *per–one-point* response on the $[0, 10]$ stringency scale: it is the change in $y_{e,t}$ at relative time ℓ associated with a one-point increase in policy stringency at the event. For an event of realised size η_e, the implied effect at horizon ℓ is simply $\eta_e \cdot \nu_\ell$.

The dose-weighted regression uses the same sample, trimming, fixed effects (α_e, γ_t), controls $X_{e,t}$, and omitted base period $(\ell = -1)$ as the baseline. Coefficients are obtained via the Sun–Abraham imputation-weighted setup on the same trimmed stack; standard errors are clustered at the country level. Tables report the dose-weighted ν_ℓ (top entry in each cell) alongside the unweighted "active" baseline estimate (bottom, grey) for direct comparison.

Findings. The tables for solar and wind (Table B.33–Table B.34) show clear dose–response: (i) FEED-IN-TARIFFS (price and duration) retain positive and statistically non-zero ν_ℓ around $\ell \in \{0, +1, +2\}$, now interpretable per point of stringency; multiplying by two gives the implied effect of a "significant" two-point step. (ii) Auctions remain modest and short-lived; (iii) RECS are weak/negative post-treatment; (iv) planning exhibits small positives; (v) ETS coefficients hover near zero to slightly negative. Relative to the unweighted baseline, magnitudes are mechanically smaller (per-unit scaling) but significance patterns and the policy ordering are unchanged. Some pre-period blips appear in a few families, consistent with the inclusion of incremental tuning events even when $\eta_c > 0$.

Takeaway. Scaling event-time indicators by η_c leaves our qualitative conclusions intact and strengthens interpretation: estimated effects for FITs and auctions scale approximately linearly with the size of the policy move at short horizons, while RECS/ETS do not display positive dose–response. The baseline results are therefore not artifacts of treating all events equally; larger policy steps are associated with proportionally larger responses where effects are present.

B.8.3 Relaxing the threshold for event significance

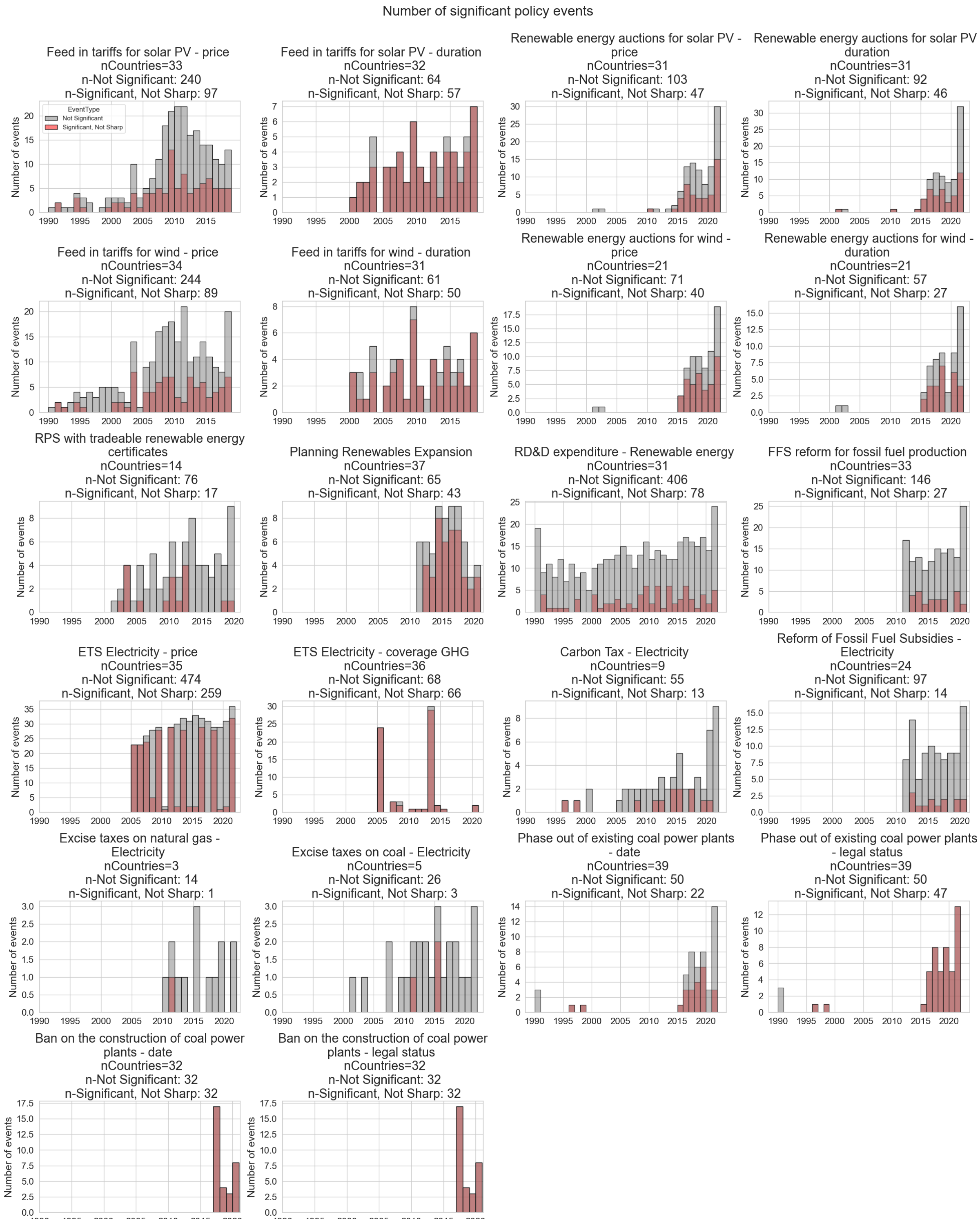


Figure B.19: Plot showing the number of events over time for each policy. Red are significant-not sharp (baseline) events, and Grey are Any-Change events. Each plot reports the statistics for the number of countries represented, and the total number of events of each type

Table B.35: solar, Any-change policy events

Policy	$\ell=-5$	$\ell=-4$	$\ell=-3$	$\ell=-2$	$\ell=0$	$\ell=1$	$\ell=2$	$\ell=3$	$\ell=4$	$\ell=5$
FIT SOL PR	0.07*	0.13*	0.02**	0.07*	0.14*	0.09*	0.06*	0.01*	-0.09*	-0.06**
	-0.01*	0.05*	-0.03*	-0.05*	0.18*	0.07*	0.13*	0.20	-0.03*	-0.02*
FIT SOL DUR	0.02**	0.05**	-0.11**	-0.15**	0.29	0.20	0.16**	0.25	-0.05*	0.10*
	-0.01**	0.02**	-0.15**	-0.17**	0.21	0.23	0.18**	0.28	-0.03*	0.11*
AUCTION SOL PR	-0.08*	0.27	0.03	0.03	0.02*	0.02*	0.00	-0.03*	-0.05*	-0.06*
	0.26	0.36	0.06	0.04	0.08*	0.24	0.14	-0.07*	-0.09*	-0.08*
AUCTION SOL DUR	0.02*	0.20	0.09	0.01	0.02*	0.02*	0.02	-0.03*	-0.07*	-0.05*
	0.02*	0.41	0.21	0.07	0.08*	0.10*	0.08	-0.00	-0.08	-0.05*
RECS	-0.11*	-0.02*	-0.09**	0.03*	-0.02*	0.03*	0.01**	-0.08**	0.00*	-0.03**
	-0.12*	0.04	-0.13	-0.01	-0.17*	-0.22*	0.00**	-0.22*	-0.17*	-0.07*
RENEWABLE EXP	-0.15*	-0.16*	-0.04*	-0.05*	0.18*	0.20*	0.13*	0.12*	0.08*	0.05*
	-0.10*	-0.12*	0.09	0.07*	0.24*	0.11*	0.07*	0.03*	0.02*	0.00*
RDD RES	-0.00**	-0.02**	0.01**	0.04**	0.03**	0.01**	-0.03**	-0.04**	-0.02**	-0.02**
	0.25*	0.06*	0.05*	0.09*	-0.00*	0.04*	-0.07*	-0.15*	-0.15*	-0.05**
FFS PRODUCER	0.17*	0.06*	0.04*	-0.11*	0.13*	-0.07*	-0.11*	-0.06*	-0.02*	-0.01*
	0.32	-0.04	0.17	0.22*	0.62	0.18	-0.10*	-0.06*	-0.00*	0.04*
ETS E PR	0.02**	-0.04*	-0.05**	0.02**	-0.06**	-0.04*	-0.05*	-0.07*	-0.05*	0.02**
	-0.01*	-0.06*	0.07*	0.05*	-0.05*	0.05*	0.01*	0.01*	-0.11*	0.04*
ETS E GHG	0.03	-0.07	0.06*	-0.01	-0.09	-0.08*	-0.05*	0.02*	-0.11*	-0.02*
	0.16	-0.07	0.08	0.13	-0.04	-0.02*	-0.09*	0.03*	-0.11*	0.00*
CARBONTAX E PR	-0.17**	-0.15**	-0.15**	-0.00*	0.30	0.01**	0.10*	0.11*	0.14*	-0.16*
	-0.33*	-0.32*	-0.24**	-0.09*	0.41	-0.07*	0.08	0.09	0.05	-0.08
FFS E	-0.12*	0.12	0.07	-0.08*	-0.00	-0.02*	0.04*	0.03*	-0.00*	-0.01*
	-0.28*	-0.08*	1.36*	-0.12*	1.27*	-0.06*	-0.22*	0.10*	-0.10*	-0.13*
EXCISETAX E NATGAS	-0.28*	0.06*	-0.14*	0.06	-0.40*	-0.53	-0.64*	-0.38**	-0.12*	-0.24*
	0.05*	-0.11	-0.16	-0.43	-0.53	0.08	-0.01	-0.11*	0.15*	0.07*
EXCISETAX E COAL	0.22**	0.02	0.02	-0.08	0.01	0.03*	-0.03*	-0.00*	-0.07**	-0.05*
	0.04*	-0.11	-0.16	-0.43	-0.53	0.08	-0.01	-0.11*	0.15*	0.07*
PHOUT COAL DATE	0.19	0.05	-0.04	0.02*	-0.07	-0.01	0.07	0.09	0.16	0.16
	0.11	0.12*	-0.08*	0.06*	0.04*	0.08	0.09*	0.06*	0.09*	0.17*
PHOUT COAL STAT	0.19	0.02	-0.10	-0.06*	-0.13*	-0.08	0.00	0.02	0.10	0.14
	0.12	-0.02	-0.08	-0.07*	-0.12*	-0.09	-0.01	-0.01	0.06	0.11
BAN COAL DATE	0.26	-0.03	0.07	0.06	-0.07	-0.09	0.13	0.13	0.25	0.40
	0.26	-0.03	0.07	0.05	-0.07	-0.09	0.13	0.13	0.24	0.39
BAN COAL STAT	0.26	-0.03	0.07	0.06	-0.07	-0.09	0.13	0.13	0.25	0.40
	0.26	-0.03	0.07	0.05	-0.07	-0.09	0.13	0.13	0.24	0.39

Notes: Event-time coefficients (Sun–Abraham IW). Coefficients are relative to the year before treatment ($\ell=-1$ omitted).
Stars: * p<0.10, ** p<0.05, *** p<0.01. 95% confidence intervals.
Each cell shows the robustness specification (top) and the baseline (bottom, grey).

Table B.36: Wind, any-change policy events

Policy	$\ell=-5$	$\ell=-4$	$\ell=-3$	$\ell=-2$	$\ell=0$	$\ell=1$	$\ell=2$	$\ell=3$	$\ell=4$	$\ell=5$
FIT WIND PR	0.01** -0.00**	0.02** -0.03**	0.03** 0.06**	0.00** -0.04**	0.03** 0.05**	0.02** 0.03**	0.01** 0.07**	0.01** 0.04**	0.02** -0.01**	0.00** 0.00**
FIT WIND DUR	-0.10** -0.06**	-0.05** -0.06**	-0.06** -0.07**	-0.04** -0.01**	0.07** 0.04**	0.09* 0.06*	0.22* 0.12*	0.05** 0.00**	0.03** -0.02**	0.00** -0.03**
AUCTION WIND PR	0.01* 0.04	0.07* 0.06	0.02* 0.02*	0.20 0.21	0.02** -0.01**	0.01** 0.05*	0.07* 0.12	0.04* 0.02	-0.03* -0.04*	-0.01* -0.01*
AUCTION WIND DUR	0.00* -0.05*	0.08* 0.07*	0.05* -0.01*	0.20 0.20	0.02** -0.05*	0.00** -0.09*	0.07* 0.01	0.05* -0.05*	-0.02* -0.10*	0.01* -0.08*
RECS	-0.03** 0.05*	0.01** -0.01*	-0.00** -0.09**	0.00** -0.07**	0.02** 0.04*	-0.02** -0.10**	-0.00** -0.02**	-0.00** -0.08**	0.02** -0.05*	0.01** -0.01**
RENEWABLE EXP	0.03* 0.06*	0.01** 0.01**	-0.01** -0.01**	0.00** 0.02**	0.07* 0.13*	0.03** 0.03**	0.03** 0.01**	0.00** -0.01**	-0.01** -0.03**	-0.03** -0.04**
RDD RES	0.01** 0.06**	0.03** 0.03**	-0.01** -0.08**	0.05** 0.01**	-0.02** -0.09**	0.00** -0.01**	-0.02** -0.08**	-0.02** -0.03**	-0.03** -0.08**	-0.04** -0.06**
FFS PRODUCER	0.07** 0.14	-0.00** -0.03**	0.05* 0.00**	-0.03** -0.06**	0.04* 0.46**	-0.01** 0.14**	-0.02** 0.14**	-0.03** 0.03**	-0.02** 0.08**	-0.05** 0.04**
ETS E PR	-0.04** -0.06**	-0.01** -0.01**	0.03** -0.00**	-0.04** -0.01**	-0.03** -0.05**	-0.03** -0.03*	-0.02** -0.05**	-0.06** -0.06**	-0.04** -0.08*	-0.01** 0.01**
ETS E GHG	0.06* 0.08*	-0.08* -0.07*	0.07** 0.05**	0.05** 0.06**	-0.04* -0.04*	-0.07* -0.06*	0.01* -0.00*	-0.00* 0.01*	-0.01** -0.00*	-0.02** -0.02**
CARBONTAX E PR	0.24* 0.49**	0.14** -0.00*	0.04** 0.09**	0.05* 0.04	0.01** 0.07*	-0.02** -0.04**	-0.06** -0.05**	-0.02** -0.01**	-0.07** -0.01**	-0.05** -0.07**
FFS E	0.01** 0.05**	0.08* 0.03**	0.01** 0.07*	0.01** 0.19*	-0.02** 0.11*	-0.05** 0.22*	-0.02** 0.13**	-0.03** 0.21*	-0.03** 0.18*	-0.05** 0.13**
EXCISETAX E NATGAS	-0.05* -0.16*	0.11** 0.14**	0.06** 0.05*	0.08** 0.04*	0.10** 0.13**	0.07** 0.06*	-0.01* -0.05*	0.07** 0.07**	0.08** 0.00**	0.02** 0.02**
EXCISETAX E COAL	-0.06** -0.16*	-0.06** 0.14**	-0.08** 0.05*	-0.07** 0.04*	-0.03** 0.13**	-0.08** 0.06*	-0.09** -0.05*	-0.04** 0.07**	-0.01** 0.00**	-0.04** 0.02**
PHOUT COAL DATE	-0.02* -0.04*	-0.10 -0.04*	0.01 0.01*	0.06** 0.03*	-0.03** -0.04**	-0.06* 0.00**	-0.14 -0.06*	-0.13 0.01*	-0.03* 0.04*	-0.02* 0.03**
PHOUT COAL STAT	-0.06* -0.05*	-0.15 -0.14	-0.06 -0.05	0.00** 0.00**	-0.04** -0.05**	-0.08* -0.07*	-0.17 -0.16	-0.17 -0.16	-0.09* -0.08*	-0.05* -0.04*
BAN COAL DATE	0.05* 0.05*	-0.09 -0.09	0.10* 0.10*	0.12** 0.12**	0.04* 0.04*	-0.10 -0.10	-0.07* -0.07*	-0.10 -0.10	0.09* 0.09*	0.13* 0.13*
BAN COAL STAT	0.05* 0.05*	-0.09 -0.09	0.10* 0.10*	0.12** 0.12**	0.04* 0.04*	-0.10 -0.10	-0.07* -0.07*	-0.10 -0.10	0.09* 0.09*	0.13* 0.13*

Notes: Event-time coefficients (Sun–Abraham IW). Coefficients are relative to the year before treatment ($\ell = -1$ omitted).
Stars: * p<0.10, ** p<0.05, *** p<0.01. 95% confidence intervals.
Each cell shows the robustness specification (top) and the baseline (bottom, grey).

We probe sensitivity to the event-definition threshold by relaxing the minimum stringency change from $\tilde{\eta}=2$ to *any* non-zero change and keeping a one-year horizon, i.e. `AnyChange-1y` (`AC`: $(\tilde{\eta}, \tilde{\tau}) = (0, 1)$; cf. Methods). All other modelling choices are held fixed (Sun–Abraham imputation–weighted estimator, controls, fixed effects, cohort window). Conceptually, `AC` admits incremental policy re-tunings (e.g., small FIT tariff updates, certificate band tweaks, ETS cap recalibrations) that the baseline `SG` filter treats as noise. The trade-off is standard: higher sample size versus lower signal-to-noise.

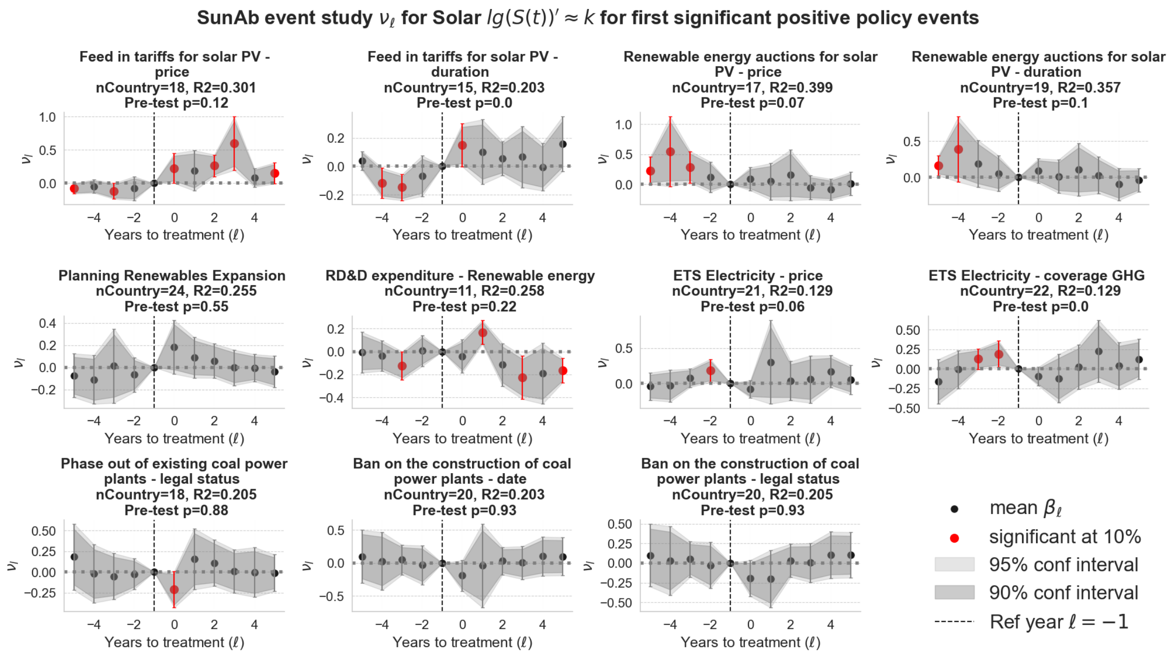
Figure B.19 shows that the `AC` construction markedly increases event counts and broadens country coverage for high-frequency policy families (FITs, RECs, ETS), while one-off commitments (coal bans/phaseouts) change little. We then re-estimate event studies under `AC` and juxtapose them with the baseline `SG` coefficients (top entry in each cell is `AC`; bottom, grey, is `SG`).

Solar. In Table B.35, FIT (Price) remains positive around treatment but attenuates relative to `SG` (e.g., $\ell = 0, 1, 2$ shrink slightly) and displays additional positive leads ($\ell \in \{-5, -4, -2\}$), consistent with either anticipatory adjustment or added measurement noise from minor policy edits. FIT (Duration) preserves its post-treatment rise with similar magnitudes. Auctions stay modest and short-lived; RECs remain weak/negative post-treatment, with a few small pre-period wiggles under `AC`. Planning retains small, steady positives. Small-N families (e.g., Carbon Tax-Electricity) continue to be fragile but do not flip qualitative sign patterns.

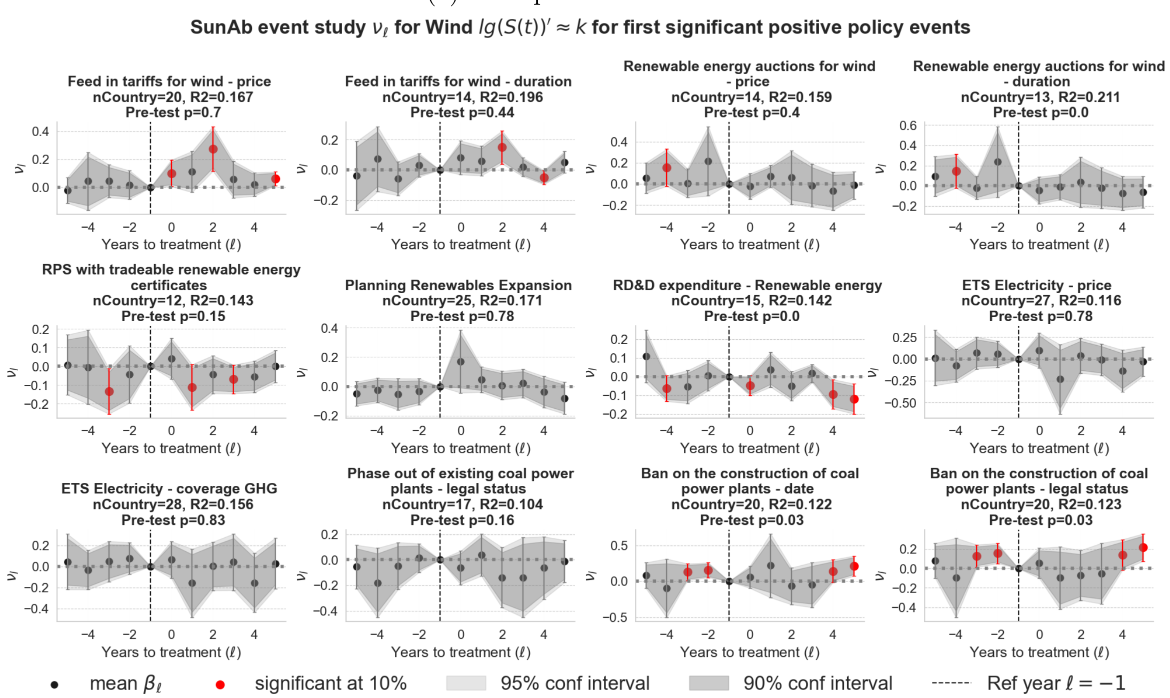
Wind. Table B.36 shows the same qualitative ranking: FIT families post small positives near $\ell = 0$–$+2$; auctions are moderate and transient; RECs are weak/negative post; planning yields small positives; ETS coefficients are near zero to mildly negative in places. As with solar, `AC` introduces extra lead activity (e.g., small non-zero coefficients at $\ell < 0$), suggesting more pre-trend "churn" when minor edits are counted as events.

Takeaway. Allowing any non-zero one-year change increases sample size and leaves the *qualitative* policy ordering intact (FITs/auctions positive around treatment; RECs weak/negative; planning small positives; ETS near zero). The main differences are slightly attenuated post-treatment magnitudes and more frequent non-zero leads, consistent with added noise from incremental policy adjustments. This supports using the ≥ 2 filter in the main analysis as a prudent signal-extraction rule, with `AC` serving as a robustness check that does not overturn our core conclusions.

B.8.4 First significant events

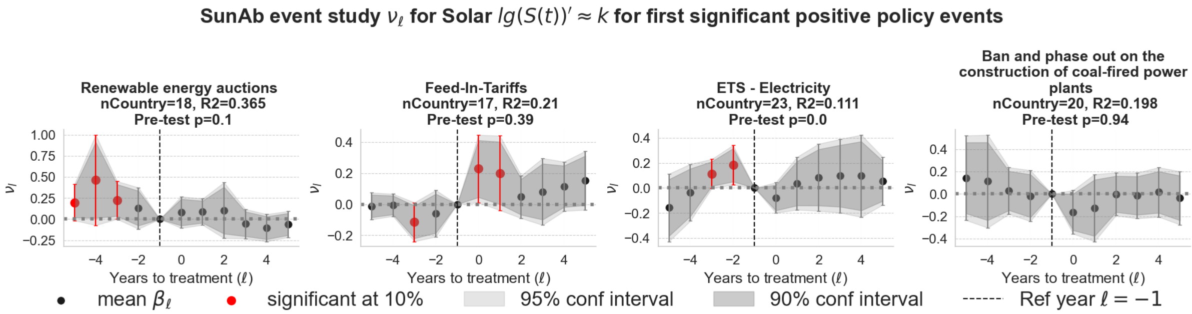


(a) First positive L4 - solar

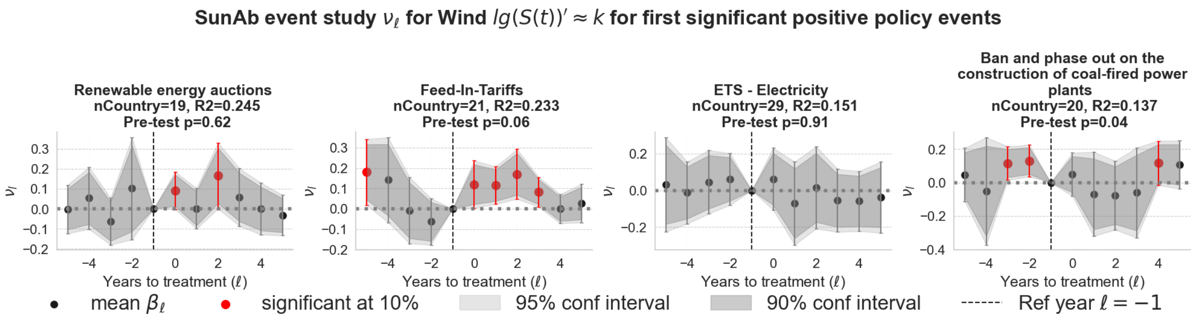


(b) First positive L4 - wind

Figure B.20: First significant positive L4 policy events - solar and wind. Each panel isolates the *first* detected positive L4 activation within each country and plots the event-study path ν_ℓ by years relative to treatment ℓ. Restricting to first events may change magnitudes and precision relative to the full sample because it de-emphasises repeated-treatment dynamics and places more weight on initial rollouts; nonetheless, similarity in sign and timing between the solar and wind panels strengthens confidence in the baseline timing of the policy response.



(a) First positive L3 - solar



(b) First positive L3 - wind

Figure B.21: First-positive treatment event-study (L3) - solar and wind. Each panel plots the event-study path ν_ℓ for the *first positive* L3 policy event (first treated instance within the L3 policy category, per country) by years relative to treatment ℓ. This appendix plot isolates the initial policy response by focusing on the first positive L3 event within each country: it is therefore a robustness check that highlights early/introductory effects (as opposed to later or repeated treatments). Restricting to first events guards against stacking from repeated activations and clarifies lead–lag structure, but it typically reduces sample size and therefore statistical power - compare effect magnitudes and precision to the all-events plots (e.g. Figure B.1) to judge robustness.

We re-estimate the Sun–Abraham event study using only the *first* detected `Significant-Gradual-2y` *positive* activation per country (within L4 families and, separately, L3 categories). This isolates the initial rollout and removes within-country policy ratchets that, in practice, account for a non-trivial share of deployment dynamics. As expected, restricting to first events lowers power and modestly widens confidence intervals; nevertheless, the timing and signs for the eight core policy families discussed in the main text are broadly consistent with the all-events baseline.

Pre-trend diagnostics. For each first-event specification we test the joint null of no pre-treatment dynamics, $H_0 : \{\nu_\ell = 0\}_{\ell \in \{-5,-4,-3,-2\}}$, using a Wald test with country- and year-fixed effects and the same covariates as in the baseline.[25] Two caveats apply. First, the first-event restriction reduces treated cohorts and weakens power, so occasional significant leads should be interpreted cautiously. Second, policies that are explicitly *announced* before they bite (plans, coal phase-out legislation, carbon pricing) can generate benign anticipation patterns without violating parallel trends in a substantive sense.

At a high level: (i) FIT–PR and FIT–DUR show small, often negative leads-consistent with "dry pipeline" periods before the inaugural tariff-so the joint-Wald is *rarely* rejected. (ii) Planning also clears

[25] Panel headers in the appendix figures report the joint-Wald p-values; tables show individual lead coefficients.

pre-trend checks in most instances, with occasional mild negative $\ell = -2$ coefficients. (iii) RDD more often exhibits weak negative leads (plausible reallocations while programs are set up). (iv) BAN COAL (DATE/STATUS) and pricing instruments sometimes display anticipation at $\ell = -1$ or $\ell = -2$; where the joint-Wald flags pre-trends we retain the series for transparency but downweight level interpretations and emphasize timing.

Feed-in-Tariffs price level. For **solar**, first-event estimates retain a clear positive at impact that *builds* over the first two post years, with larger medium-run responses by $\ell \in \{+2, +3\}$ than in the all-events sample.[26] Leads at $\ell \leq -2$ are mildly negative; the joint-Wald is typically not rejected, supporting parallel trends. For **wind**, FIT–PR shows a positive step at $\ell = 0$ and strengthens by $\ell = +2$ with little evidence of economically large anticipatory spikes; pre-trend tests generally pass.

Feed-in-Tariffs duration/tenor. Holding the guaranteed tenor constant in the first activation delivers smaller on-impact effects than FIT–PR but still exhibits *accumulating* positives by $\ell = +1$ to $+2$ in both **solar** and **wind**. Solar shows negative leads at $\ell = -3, -2$, consistent with delayed pipeline formation; the joint-Wald seldom rejects. Wind displays the same qualitative shape with slightly earlier lift and similarly clean pre-trend diagnostics.

Planning renewable expansion. Plan-type measures (e.g. binding capacity targets, multi-year expansion schedules) show *immediate but moderate* positives at $\ell = 0$ for both **solar** and **wind**, persisting into $\ell = +1, +2$. First-event restriction attenuates magnitudes relative to all-events-unsurprising because much of the effect arrives via subsequent instruments (auctions, FIT ratchets, grid upgrades). Leads are generally small; where a weak $\ell = -2$ dip appears, the joint-Wald typically remains above conventional cutoffs, indicating no systematic pre-trend.

Public R&D for renewables. First-event RDD effects are near zero at impact and often turn *weakly negative* at medium lags in **solar** and **wind**, consistent with long-fuse knowledge channels that need deployment complements to translate into capacity. Relative to all-events, first-events make these small negative leads more visible; on occasion the joint-Wald flags them. We therefore treat RDD first-event magnitudes as *timing suggestive* and lean on all-events for levels.

Coal ban *dates*. For **solar**, the *first* ban-date activation yields coefficients that are statistically indistinguishable from zero at and after impact, with some small negatives at $\ell = 0$ and mixed signs thereafter. We therefore do not claim a near-term causal effect for solar from the inaugural ban-date alone. For **wind**, the shape may look encouraging at first glance, but the *leads* display systematic movement and the joint-Wald pre-trend test often rejects at conventional cutoffs. Given these anticipatory dynamics, we treat the wind first-event ban-date profile as *non-causal* in isolation.

Coal ban *status/legislation*. A similar picture holds for the first codified status change. For **solar**, first-event effects are small and not statistically different from zero in the post window. For **wind**, some

[26] See solar L4 table: $\nu_0 > 0$ with further increases at $\ell = +1, +2$; the robustness row often shows even larger $\ell = +3$ values than the baseline (grey).

specifications show small positives, but these coexist with meaningful pre-trend action (frequently failing the joint-Wald), which prevents a clean causal interpretation. In practice, status changes tend to formalize prior plans and announcements and arrive alongside other market reforms; the first-event lens therefore strips away the subsequent "enabling" ratchets that typically deliver measurable deployment.

Why we keep coal bans in the main. First-event evidence tells us that a lone, inaugural ban (date or status) does not, by itself, generate immediate and cleanly identifiable capacity responses-especially once we enforce strict pre-trend diagnostics. We nonetheless retain coal-ban families in the main figures and discussion for three reasons that are relevant to policy inference but consistent with the caveats above:

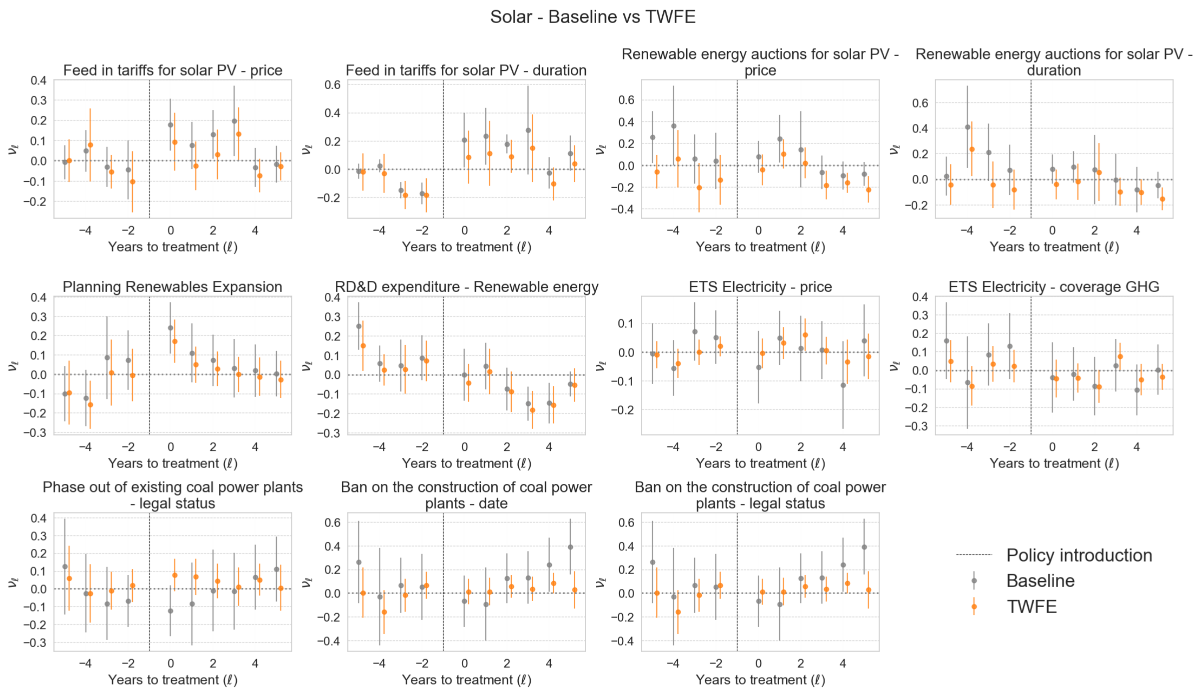
1. *Policy salience and sequencing.* Coal bans are high-salience anchors in a policy *sequence*. In our all-events specifications (which capture subsequent ratchets-auctions, FIT adjustments, grid commitments), the medium-run patterns are more informative and pre-trends are cleaner. The first-event filter removes exactly those follow-ons that operationalize the ban's intent.

2. *Power and composition.* First-event restrictions shrink treated cohorts and tilt the sample toward early adopters with idiosyncratic baselines. This reduces statistical power and makes pre-trend tests more brittle; null post-effects for solar and pre-trends for wind are therefore informative about *timing*, not proof of no effect in policy sequences.

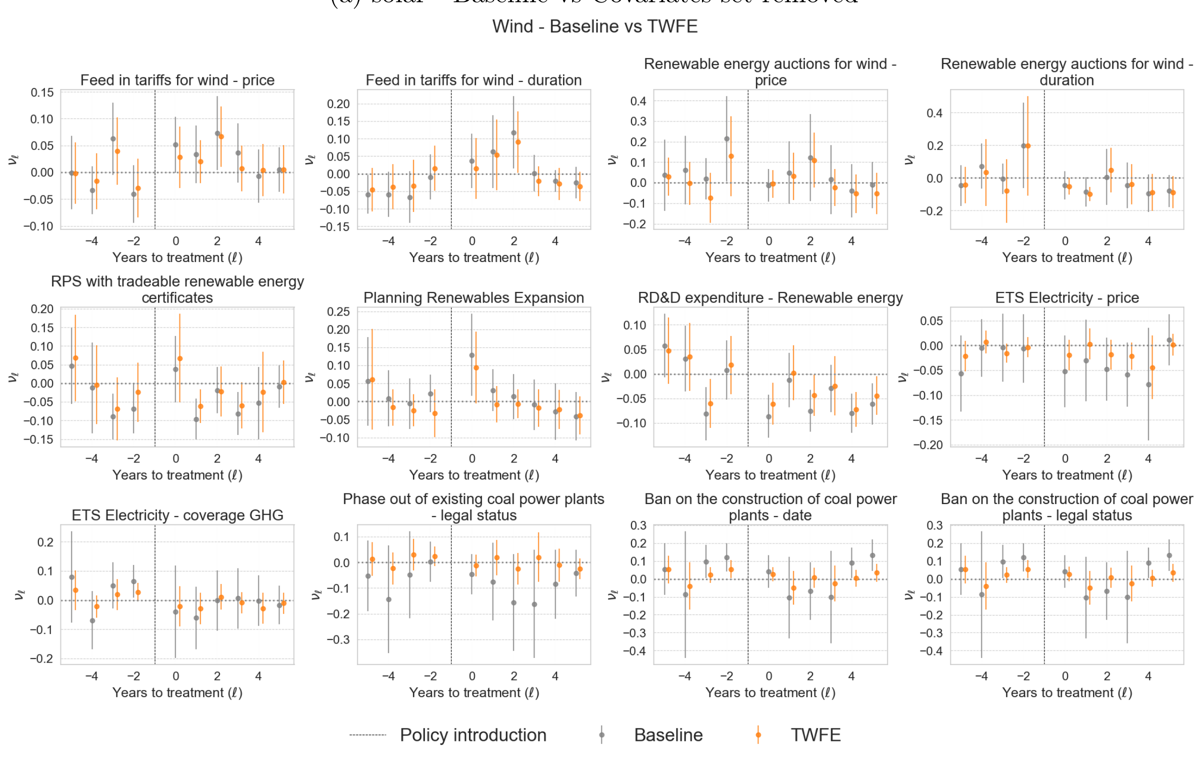
3. *Policy bundles, not silos.* Dose-response and L3 planning results indicate that expectations matter once paired with procurement and grid policy. We therefore present coal-ban families as components of a bundle rather than standalone "workhorse" levers and avoid level claims that hinge on first-event estimates.

Takeaway. Under the first-event design, *coal ban dates and status* do not deliver robust causal impacts: solar shows no significant post-treatment response; wind exhibits pre-trends that preclude causal interpretation. These results are fully compatible with our main narrative once we recognize that bans function as *commitment devices* whose effects materialize through subsequent procurement and system-integration ratchets. Accordingly, in the main we emphasize FIT price and duration as early, reliably causal movers; planning as an immediately supportive but enabling policy; R&D as indirect and lagged; and coal-ban measures as expectation-setting elements whose deployment impact is realized in policy bundles rather than at the first announcement or first legal codification.

B.9 Alternative estimator: TWFE vs. Sun-Abraham (SUNAB)



(a) solar - Baseline vs Covariates set removed



(b) wind - Baseline vs Covariates set removed

Figure B.22: Comparing the baseline Sun Abraham specification to Two-Way Fixed Effect (TWFE) estimator approach. Each panel plots the event-study path ν_ℓ by years relative to treatment (ℓ). Dots show the mean ν_ℓ with associated error bands showing the 90% confidence intervals, and the dashed vertical line marks the policy introduction. Grey = Baseline, Orange = test (full-window restriction)

We benchmark the baseline Sun-Abraham imputation–weighted event study against a two–way fixed–effects (TWFE) estimator that uses the never–treated countries as controls and the same trimmed event window and fixed effects.[27]

High-level comparison. Across the eight core policy families discussed in the main text (FIT price, FIT duration, auctions price, auctions duration, planning/expansion, public R&D, coal ban dates, coal ban status), SUNAB and TWFE broadly agree on *timing* of effects, while TWFE tends to yield slightly *attenuated magnitudes* and somewhat *larger lead movements*. This shows up most clearly in FIT results, where TWFE often has more negative leads at $\ell \in \{-3, -2\}$ and smaller on-impact/post coefficients than SUNAB (Figure B.22). For policies with long realization lags (auctions, planning), both estimators deliver shallow near-term responses; for expectation-type policies (coal bans, pricing), both show noisier leads, with SUNAB typically flagging pre-trend concerns more clearly.

Core families. **FIT price (PR).** Timing aligns: a step at $\ell = 0$ and strengthening through $\ell = +2$ in both solar and wind. TWFE coefficients are generally smaller than SUNAB at impact and medium lags, and leads are slightly more negative in TWFE, consistent with residual contamination from staggered adoption.

FIT duration (DUR). Both estimators show a slower build (smaller $\ell = 0$) and accumulation by $\ell \in \{+1, +2\}$. As with PR, TWFE magnitudes are somewhat attenuated, with mildly stronger negative leads.

Planning / expansion targets. SUNAB and TWFE indicate modest, persistent post-treatment gains. TWFE occasionally shows small negative leads that SUNAB does not, suggesting some anticipatory or compositional tilt among never–treated controls; the substantive conclusion (plans help but require follow-on procurement/grid levers) is unchanged.

Public R&D (RDD). Both estimators are near zero at impact with weakly negative medium-run coefficients and some lead motion; neither approach supports a direct, immediate deployment channel. SUNAB tends to surface pre-trend uncertainty more clearly; we therefore interpret R&D dynamics primarily as *indirect* and lagged.

Coal bans (date/status). Neither estimator yields a clean causal pattern once pre-trends are considered. In solar, post coefficients are generally small and not robustly different from zero; in wind, TWFE sometimes shows positive post values, but both estimators display meaningful lead action. We therefore do *not* claim causal first-order deployment effects for isolated ban announcements or status changes and keep these families in the main discussion as *expectations/commitment* instruments whose deployment impact materializes in bundles with procurement and grid policy.

Pre-trends and inference. Joint-Wald tests on SUNAB leads typically pass for FIT families and planning, while TWFE more often shows borderline lead movement (e.g., slightly more negative $\ell = -2, -3$). Where either estimator fails pre-trends (notably coal bans and some pricing instances), we treat dynamic profiles

[27]Both estimators omit $\ell = -1$, include country and year fixed effects, and use the same covariate set unless stated. Standard errors are country–clustered; figures show 90% confidence bands. SUNAB is robust to treatment-effect heterogeneity across cohorts/timing; conventional TWFE can place implicit negative weights under staggered adoption when effects are heterogeneous. Restricting controls to never–treated mitigates, but does not completely eliminate, composition issues if the never–treated set is small or systematically different.

as *timing evidence only* and avoid level claims. In such cases we defer magnitudes to specifications that condition on policy bundles or to all-events designs with sharper identification.

Takeaway. SUNAB and TWFE tell the same timing story for the workhorse deployment levers (FITs, planning) with TWFE typically smaller in magnitude and slightly noisier in the leads. For policies prone to anticipation or bundling (coal bans, pricing), both estimators exhibit lead motion; SUNAB's pre-trend diagnostics make those caveats more transparent. We therefore retain SUNAB as the *baseline* and present TWFE with never–treated controls as a *robustness* that corroborates the main dynamics while reminding readers of the limits of TWFE under staggered adoption with heterogeneous effects.

C Event study and Magnitude of effects details

C.1 Event Study setup details

We estimate dynamic policy effects using interaction–weighted event studies (Sun and Abraham 2021) on stacked event windows. For each cohort g and relative time ℓ, we obtain $\tau_{g,\ell}$ comparing treated units only to never- or not-yet-treated units within the stack (time×stack fixed effects ensure clean risk sets). We then aggregate post-estimation via researcher-chosen weights w_g to $\nu_\ell = \sum_g w_g \tau_{g,\ell}$. This estimand is event-conditional and history-dependent.

Cohort-by-relative-time specification. For each treated cohort e and relative time ℓ, define

$$D_{it}^{(e,\ell)} = \mathbf{1}\{E_i = e\} \cdot \mathbf{1}\{t - e = \ell\}, \tag{16}$$

with $\ell \in \{-K, \dots, -2, 0, \dots, M\}$ and $\ell_0 = -1$ *omitted* as the baseline.[28] We consider two variants:

(i) Active (unweighted):

$$k_{it} = \sum_e \sum_{\ell \neq \ell_0} \beta_{e\ell} D_{it}^{(e,\ell)} + \boldsymbol{\delta}^\top \mathbf{X}_{i,t-1} + \mu_i + \gamma_t + \varepsilon_{it}. \tag{17}$$

(ii) Interaction-weighted by event size: let η_i be the focal event *size* (the stringency change measured at $t = E_i$). Then

$$k_{it} = \sum_e \sum_{\ell \neq \ell_0} \beta_{e\ell}^{\eta} \big(\eta_i D_{it}^{(e,\ell)}\big) + \boldsymbol{\delta}^\top \mathbf{X}_{i,t-1} + \mu_i + \gamma_t + \varepsilon_{it}. \tag{18}$$

Here μ_i and γ_t are country and year fixed effects; $\mathbf{X}_{i,t-1}$ collects pre-determined controls (Section 4.5). We trim to a common window $[-K, M]$, retain never-treated units when available, and if none exist, trim the post-period to precede the last cohort as in Sun and Abraham (2021).

Aggregation across cohorts. Equation 17–Equation 18 present the cohort-by-relative-time specification: $\widehat{\beta}_{e\ell}$ (or $\widehat{\beta}_{e\ell}^{\eta}$) are cohort-specific event-time coefficients estimated by fixed effects regression. For exposition and plotting we aggregate these cohort estimates into an interaction-weighted cohort average. For each ℓ we report interaction-weighted averages

$$\nu_\ell = \sum_e \omega_e \widehat{\beta}_{e\ell}, \qquad \sum_e \omega_e = 1, \tag{19}$$

with weights ω_e proportional to cohort sizes (i.e. $\omega_e = n_e / \sum_e n_e$). In the weighted case Equation 18, $\widehat{\beta}_{e\ell}^{\eta}$ aggregate analogously and are interpretable as effects per unit of event size. Standard errors for ν_ℓ use the beta method based on the regression variance-covariance matrix

$$Var(\nu_\ell) = \omega^\top Var(\widehat{\beta}_{\cdot\ell})\, \omega, \qquad \mathrm{se}(\nu_\ell) = \sqrt{\omega^\top \widehat{V}_{\beta_{\cdot\ell}}\, \omega}. \tag{20}$$

Multiple events. If countries experience multiple focal events, we form pseudo-units $i \times$ #event so that each unit contributes at most one cohort; country and year FEs are preserved, and inference is clustered by

[28] Omitting $\ell_0 = -1$ makes $t = E_i - 1$ the reference; coefficients are deviations from that period.

country.

Interpretation and identification. Pre-event coefficients ($\ell < 0$) test for differential pre-trends relative to $\ell_0 = -1$. Post-event coefficients ($\ell \geq 0$) quantify deviations in diffusion speed k_{it} relative to the pre-event baseline. Identification relies on cohort-specific parallel trends conditional on μ_i, γ_t, and $\mathbf{X}_{i,t-1}$ within the trimmed window.

Transformations and timing. Let $\mathcal{A} = \{\log(\text{GDP per capita}), \log(\text{Population})\}$ denote activity variables, and let $\mathcal{F}$ collect slower-moving structural/finance variables: gross fixed capital formation (%GDP), inward FDI (%GDP), oil rents (%GDP), R&D (%GDP), and a financing proxy (sovereign 10y yield) when available. We transform within country as

$$A_{it}^{\Delta} = A_{it} - A_{i,t-1} \quad \forall A \in \mathcal{A}, \qquad \mathbf{X}_{i,t-1} \supset \big\{A_{i,t-1}^{\Delta} : A \in \mathcal{A}\big\} \cup \big\{F_{i,t-1} : F \in \mathcal{F}\big\}. \tag{21}$$

Thus, activity enters in first differences (lagged one year), aligning growth-on-growth with k_{it}, while frictions/structural shares enter as lagged levels to reduce simultaneity concerns [29]. All continuous controls are standardised (mean 0, variance 1) after the transformations in Equation 21.

C.2 Magnitude of effects and selected metric details

Our aim is to translate the event–study coefficients into an *absolute* change in the speed at which cumulative deployment grows. Because early-stage adoption of wind and solar is well approximated by the linearised logistic expression Equation 2, the gradient of the first derivative $d \log S_{i,t}/dt$ is an approximation of the **diffusion speed** k. A policy that alters this derivative therefore shifts the future trajectory of cumulative deployment.

Step 1: Baseline (no–policy) growth rate. For each country i we define a *constant* baseline slope equal to the average pre–treatment slope

$$k_{0,i} = \frac{1}{N} \sum_{j=1}^{N} \big[\log S_{i,T_i-j} - \log S_{i,T_i-j-1}\big], \tag{22}$$

where T_i is the first operative policy year and N is the lead window length, set to $N = 4$. This $k_{0,i}$ serves as the no–policy counterfactual trend.

Step 2: Mapping aggregated event–study estimates to slope shifts with directional gating and three coefficient modes. Let ν_ℓ and $\text{se}(\nu_\ell)$ denote the interaction-weighted cohort-aggregate estimate and its standard error for relative year ℓ. Let $s_i \in \{+1, -1\}$ encode the event's sign ("positive" $= +1$, "negative" $= -1$) and let η_i be the event size. We use one-sided, *directional* gates and define

$$g_+(\nu, \text{se}; z) = \max\{0, \nu - z\text{se}\}, \qquad g_-(\nu, \text{se}; z) = \max\{0, -(\nu + z\text{se})\}, \tag{23}$$

[29]To ensure comparability across instruments and to avoid oversaturation, we *do not* interact treatments with penetration stage or system-stress proxies; any moderation beyond fixed effects is left to these controls and to cohort aggregation.

where $z \geq 0$ controls bound tightness (e.g. z=1 for 1σ, z=1.645 for one-sided 90%, z=1.96 for one-sided 95%). For mode $m \in \{\text{conservative}, \text{point}, \text{optimistic}\}$ define the usable (nonnegative) aggregated magnitude $\tilde{\nu}_{i,\ell}^{(m)} \geq 0$ by

$$\tilde{\nu}_{i,\ell}^{\text{conservative}} = \begin{cases} g_+(\nu_\ell, \text{se}(\nu_\ell); z), & s_i = +1, \\ g_-(\nu_\ell, \text{se}(\nu_\ell); z), & s_i = -1, \end{cases} \tag{24}$$

$$\tilde{\nu}_{i,\ell}^{\text{point}} = \begin{cases} \nu_\ell \cdot \mathbf{1}\{g_+(\nu_\ell, \text{se}(\nu_\ell); z) > 0\}, & s_i = +1, \\ (-\nu_\ell) \cdot \mathbf{1}\{g_-(\nu_\ell, \text{se}(\nu_\ell); z) > 0\}, & s_i = -1, \end{cases} \tag{25}$$

$$\tilde{\nu}_{i,\ell}^{\text{optimistic}} = \begin{cases} \max\{0, \nu_\ell + z\text{se}(\nu_\ell)\}, & s_i = +1, \\ \max\{0, -(\nu_\ell - z\text{se}(\nu_\ell))\}, & s_i = -1. \end{cases} \tag{26}$$

The slope shock depends on the *Weighted (dosal-response) event specification:* ν_ℓ^η denotes the aggregate effect *per unit of event size.* The slope shock for unit i is

$$\Delta k_{i,\ell}^{(m)} \ = \ s_i \ \eta_i \ \tilde{\nu}_{i,\ell}^{\eta,(m)}. \tag{27}$$

The conservative mode uses a one-sided lower bound in the event's direction (shrinks uncertain aggregates toward zero); the point mode keeps the central aggregated estimate but only when the directional bound excludes zero; the optimistic mode uses the opposite one-sided bound as an upper envelope.

Step 3: Constructing the policy–on slope path. With $\tau = t - T_i$ the relative time,

$$k_{i,t}^{\text{pol},(m)} = \begin{cases} k_{0,i}, & \ell < 0, \\ k_{0,i} + \Delta k_{i,t}^{(m)}, & 0 \leq \ell \leq M, \\ k_{0,i}, & \ell > M. \end{cases} \tag{28}$$

Step 4: Integrating slopes into counterfactual paths (discrete time). We work in annual discrete time and ensure continuity by anchoring at the last pre–policy observation:

$$\widehat{\log S}_{i,T_i-1}^{\text{pol},(m)} = \widehat{\log S}_{i,T_i-1}^{\text{no}} = \log S_{i,T_i-1},$$

and iterate

$$\widehat{\log S}_{i,t}^{\text{pol},(m)} = \widehat{\log S}_{i,t-1}^{\text{pol},(m)} + k_{i,t}^{\text{pol},(m)} \tag{29}$$

$$\widehat{\log S}_{i,t}^{\text{no}} = \widehat{\log S}_{i,t-1}^{\text{no}} + k_{0,i}. \tag{30}$$

Exponentiating yields levels, $\widehat{S}_{i,t}^{\text{pol},(m)} = \exp\bigl(\widehat{\log S}_{i,t}^{\text{pol},(m)}\bigr)$ and $\widehat{S}_{i,t}^{\text{no}} = \exp\bigl(\widehat{\log S}_{i,t}^{\text{no}}\bigr)$. For presentation we may rebase the simulated paths by a constant so that $\widehat{\log S}_{i,t^\star}^{\text{pol},(m)}$ matches the observed $\log S_{i,t^\star}$ at a chosen year $t^\star$ (e.g. 2022), without affecting differences.

We take the **point** mode as the primary estimate for diffusion boosts (time profiles, totals, areas between curves), and show **conservative–optimistic** bands as sensitivity envelopes.

Step 5. Effect size metric To estimate the magnitude of the treatment effect we use the geometric mean capacity ratio

$$\mathcal{G}_i(C) = \exp\Big(\frac{1}{N_C}\sum_{t\le C}\ln\frac{\widehat{S}^{\text{pol}}_{i,t}}{\widehat{S}^{\text{no}}_{i,t}}\Big) \tag{31}$$

where N_C is the number of included years. For interpretation relative to current scale, we also express $\Delta S_i(C)$ as a percentage of the observed capacity in a reference year (e.g. 2023). $\mathcal{G}$ derived directly from the log-difference formulation (Equation 22–Equation 30) and has three advantages: (i) it is mathematically robust, being additive in the log domain and multiplicative in levels; (ii) it is invariant to the absolute scale of installed capacity, allowing comparisons across technologies and countries; and (iii) it has a clear interpretation as the average multiplicative deviation from the no-policy baseline over the analysis horizon. While $\mathcal{G}$ is the most interpretable and scale-independent metric for cross-case comparisons, we also report complementary measures - such as absolute change in installed capacity (MW) and percentage-point change in penetration - to provide additional policy-relevant context in select cases.

C.2.1 Potential Effect–size metrics.

The instantaneous treatment effect on speed in year t is

$$\boxed{\mathrm{TE}^{(k)}_{i,t} = k^{\text{pol}}_{i,t} - k_{0,i} = \Delta k_{i,t}.} \tag{32}$$

There are several potential level-based metrics at a cut–off year C that can be used to estimate the effect of diffusion:

$$\text{Area (MW·years)}\mathcal{A}_i(C) = \sum_{t\le C-1}\frac{\big(\widehat{S}^{\text{pol}}_{i,t+1} - \widehat{S}^{\text{no}}_{i,t+1}\big) + \big(\widehat{S}^{\text{pol}}_{i,t} - \widehat{S}^{\text{no}}_{i,t}\big)}{2}, \tag{33}$$

$$\text{End–year delta (MW)}\Delta S_i(C) = \widehat{S}^{\text{pol}}_{i,C} - \widehat{S}^{\text{no}}_{i,C}, \tag{34}$$

$$\text{Geometric mean ratio } \mathcal{G}_i(C) = \exp\Big(\frac{1}{N_C}\sum_{t\le C}\ln\frac{\widehat{S}^{\text{pol}}_{i,t}}{\widehat{S}^{\text{no}}_{i,t}}\Big), \tag{35}$$

$$\overline{\Delta\log S}_i(C) = \frac{1}{N_C}\sum_{t\le C}\Big(\widehat{\log S}^{\text{pol}}_{i,t} - \widehat{\log S}^{\text{no}}_{i,t}\Big), \tag{36}$$

Table C.1: Comparison of alternative treatment effect metrics.

Metric	Advantages	Limitations
Geometric-mean capacity ratio $\mathcal{G}$	• Additive in the log domain, multiplicative in levels. • Invariant to scale (works across countries/technologies). • Robust to outliers compared to arithmetic mean. • Interpretable as "average multiplicative deviation" from baseline.	• Less intuitive than absolute changes for some audiences. • Does not directly relate to physical capacity additions - primarily relative.
Absolute change in capacity (MW)	• Directly relates to physical capacity installed. • Intuitive for policy and industry stakeholders.	• Not scale-invariant: large economies dominate. • Harder to compare across countries or technologies.
Percentage-point change in penetration	• Relative measure, more comparable than absolute MW. • Works even when capacity levels differ substantially.	• Sensitive to denominator definition (total capacity). • Less stable when baseline penetration is very low.
Arithmetic mean capacity ratio	• Simple to compute. • Can be interpreted as average relative effect.	• More sensitive to outliers than $\mathcal{G}$. • Does not preserve additivity in the log domain.
Time saved to reach level (years)	• Expresses effects as an acceleration in time, easy to interpret. • Naturally comparable across countries and technologies. • Uses the full dynamic path rather than a single-year snapshot.	• Requires interpolation (and sometimes extrapolation) of the no-policy path. • Undefined if the no-policy path never reaches the policy level. • Can be sensitive to tail behaviour and model misspecification.

C.2.2 Time saved metric

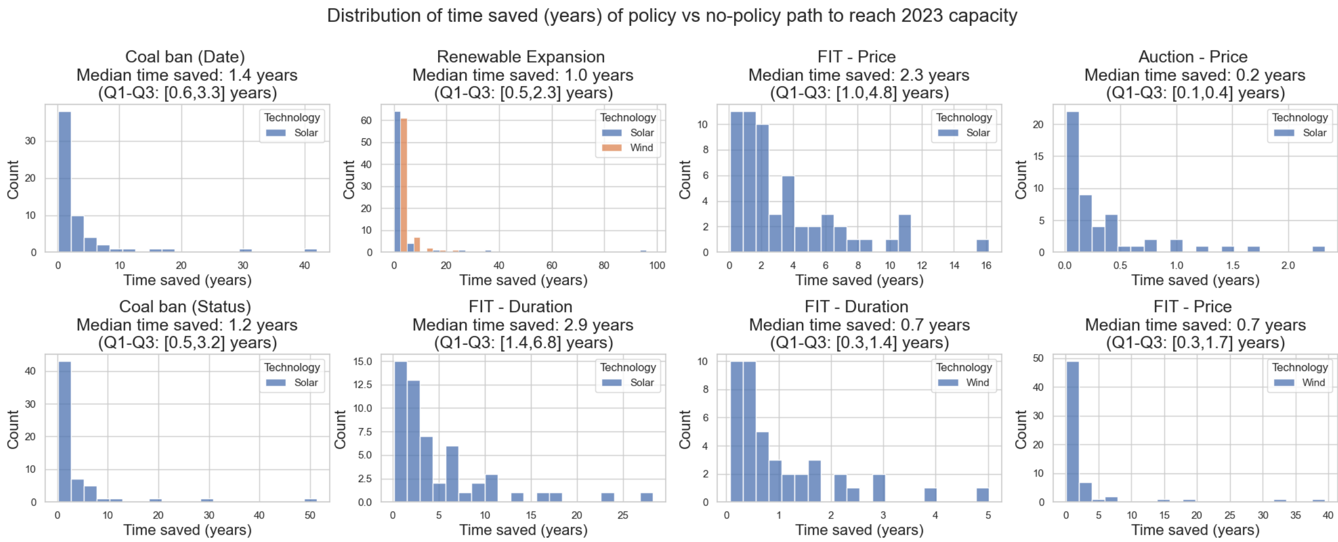


Figure C.1: Distribution of "time saved" from each policy family, defined as the number of years by which the no-policy counterfactual would need to be brought forward to match the policy path's 2023 capacity level. Each panel reports the median and interquartile range across country–technology observations. Positive values indicate that the policy path reaches the 2023 level earlier than the no-policy path, reflecting an acceleration of diffusion.

The "time-saved" metric reframes policy effects as an acceleration rather than a difference in levels. Instead of asking how much extra capacity the policy path yields in a given year, it asks how many years earlier the no-policy trajectory would have had to reach the policy path's 2023 level. This converts treatment effects into a temporal shift, making results comparable across countries and technologies with very different scales. Figure C.1 shows the distribution of these time gains for each policy family. Most policies produce modest but positive accelerations, with medians typically between one and three years and long right tails in cases where the counterfactual lags far behind. This perspective complements level-based metrics by highlighting how policies compress the diffusion timeline rather than only increasing capacity.

C.3 Log-difference metric and geometric-mean capacity ratio

We define the per-policy log effect for policy p in country i at time t, filled with zeros outside the relevant policy window, as

$$\Delta\ell_i^{(p)}(t) = \log \hat{S}_{i,t}^{\text{pol},(p)} - \log \hat{S}_{i,t}^{\text{no}}, \tag{37}$$

where $\hat{S}_{i,t}^{\text{pol},(p)}$ is the simulated adoption trajectory with the policy in place and $\hat{S}_{i,t}^{\text{no}}$ is the counterfactual adoption trajectory without the policy.

The total log effect across all policies p is then

$$\Delta\ell_i^{\text{total}}(t) = \sum_p \Delta\ell_i^{(p)}(t). \tag{38}$$

For a fixed horizon C, the average log effect is

$$\overline{\Delta \log S_i^{\text{total}}}(C) = \frac{1}{N_C} \sum_{t \leq C} \Delta \ell_i^{\text{total}}(t) = \sum_p \overline{\Delta \log S_i^{(p)}}(C), \tag{39}$$

where N_C is the number of observations up to horizon C. This formulation is *additive* in the log domain under the same horizon and baseline, even when policy windows overlap.

Connection to the geometric-mean capacity ratio. If we define the per-period capacity ratio for policy p as

$$r_i^{(p)}(t) = \frac{\hat{S}_{i,t}^{\text{pol},(p)}}{\hat{S}_{i,t}^{\text{no}}}, \tag{40}$$

then its geometric mean up to C is

$$G_i^{(p)}(C) = \exp\left(\frac{1}{N_C} \sum_{t \leq C} \log r_i^{(p)}(t) \right) = \exp\Big(\overline{\Delta \log S_i^{(p)}}(C) \Big). \tag{41}$$

The total geometric-mean capacity ratio implied by multiple policies is therefore

$$G_i^{\text{total}}(C) = \exp\Big(\overline{\Delta \log S_i^{\text{total}}}(C) \Big) = \prod_p G_i^{(p)}(C), \tag{42}$$

which follows directly from the additivity of the log-difference metric.

C.4 Timing, capacity at first policy, and $\mathcal{G}$ - additional details

The log–log specification in Section 5.7 summarises the relationship between boost and timing. Pooling solar and wind gives a negative elasticity (estimated β near -0.06), indicating diminishing returns to late intervention: a ten-fold increase in capacity at the time of first policy is associated with an order-unity reduction in expected boost on the order of 10–15%. Disaggregation shows the effect is concentrated in solar and in FIT-type instruments; wind and many non-FIT instruments show much smaller or no clear timing gradient.

Caveats: (i) selection - countries that act earlier differ in capacity and institutions; (ii) sample size - disaggregation reduces precision; (iii) measure construction - because $\mathcal{G}$ uses positive events, failed late attempts are less visible. Treat these associations as informative summaries rather than causal laws.

The gradient is estimated with

$$\log_{10} \mathcal{G}_i = \alpha + \beta \log_{10}(\text{Cap. at first policy})_i + \varepsilon_i,$$

so β is an elasticity: a $10\times$ larger capacity at the time of the first policy is associated with a multiplicative change in the expected boost of 10^{β}. Pooling solar and wind across all policy instruments yields a clear negative elasticity ($\hat{\beta} = -0.059,\ R^2 = 0.288,\ p = 1.38 \times 10^{-15},\ N = 190$). Interpreted elastically, a $10\times$ later policy (in capacity terms) is associated with a $\approx 13\%$ smaller boost $\big(10^{-0.059} \approx 0.87\big)$; a $100\times$ later policy corresponds to $\approx 24\%$ smaller boosts $\big(10^{-0.118} \approx 0.76\big)$.

Disaggregating by instrument suggests that the pooled timing effect is largely carried by FIT-type policies. Keeping only FIT price/duration events gives ($\hat{\beta} = -0.057,\ R^2 = 0.187, p = 3.9 \times 10^{-5}, N = 84$)-a similar elasticity to the pooled case $\left(10^{-0.057} \approx 0.88\right)$. By contrast, keeping only non-FIT instruments produces a small effect ($\hat{\beta} = -0.0118,\ R^2 = 0.057, p = 0.014, N = 106$), about a $2-3\%$ reduction in $\mathcal{G}$ for a $10\times$ later policy $\left(10^{-0.0118} \approx 0.97\right)$.

Technology-specific patterns are asymmetric. For solar, the pooled relationship is strong ($\hat{\beta} = -0.0706,\ R^2 = 0.328, p = 3.52 \times 10^{-12}, N = 124$), implying $\sim 15\%$ lower boosts for a $10\times$ later first policy. Once solar is split by instrument, precision falls and the gradients flatten: non-FIT-only retains a weak but significant slope ($\hat{\beta} = -0.0128,\ R^2 = 0.060, p = 0.027, N = 81$), while FIT-only shows no detectable timing gradient ($\hat{\beta} = -0.0203,\ R^2 = 0.017, p = 0.399, N = 43$). For wind, the pooled slope is modest but nonzero ($\hat{\beta} = -0.0110,\ R^2 = 0.083, p = 0.019, N = 66$), and neither FIT-only nor non-FIT-only subsamples show a detectable pattern (FIT-only: $\hat{\beta} = -0.0057,\ R^2 = 0.0219, p = 0.356, N = 41$; non-FIT-only: $\hat{\beta} = 0.0008,\ R^2 = 0.0005, p = 0.914, N = 25$).

In summary, earlier policies tend to pack a bigger punch. Looking across countries, when the first substantial policy shows up later in the diffusion journey, the average boost to deployment is smaller-roughly speaking, showing up ten times "later" corresponds to about a 13% smaller boost. This pattern is most visible for solar-especially for feed-in tariff changes-and much fainter for wind or non-FIT instruments. In plain terms: early, well-designed solar support usually moves the needle more; strong late surges are uncommon.

That said, treat this as a smart summary, not iron law. Our "how late is late" proxy also picks up who has capable institutions, pre-existing momentum, and what other policies are running, so part of the gradient reflects selection, not pure timing. Splitting the data by technology or instrument shrinks sample sizes and makes subtle patterns harder to see; very early or very late cases can still tug on the line; and because the boost metric is built from positive events, late policies that simply failed to produce an effect are under-represented. The nuanced claim, then: earlier action usually delivers larger gains-clearest for solar FITs-but the size of that timing premium depends on technology, instrument, and country context.

D Detecting breaks in early-stage diffusion (piecewise exponential)

As a preliminary analysis and a stylised fact (Main text Section 2.3), we analyse the breaks in the diffusion of solar and wind. A concentration of breaks around the timing of policies would indicate that there is a relationship between the two.

D.1 Setup and notation

Let $S_{g,i,t}$ denote cumulative adoption of technology $g \in \{\text{solar}, \text{wind}\}$ in country i and year t. Define the working series

$$y_{g,i,t} \equiv \ln S_{g,i,t},$$

and restrict attention to the early diffusion window used in the main text (years with strictly positive $S_{g,i,t}$; figures annotate years after the 1% penetration threshold for context). In early diffusion we expect $y_{g,i,t}$ to evolve approximately linearly in time with slope k:

$$y_{g,i,t} \approx c + k\,t \quad \text{(early growth approximation).} \tag{43}$$

Departures from a single straight line are our object of interest.

D.2 Continuous piecewise log-linear model

We model $y_{g,i,t}$ as a *continuous* piecewise-linear function of time with unknown breakpoints $\boldsymbol{\tau} = (\tau_1, \dots, \tau_m)$ and segment slopes $\boldsymbol{k} = (k_1, \dots, k_{m+1})$. A hinge representation that enforces continuity is

$$y_{g,i,t} \;=\; \beta_0 + \beta_1 t + \sum_{j=1}^{m} \beta_{1+j}\,(t - \tau_j)_+ + \varepsilon_{g,i,t}, \tag{44}$$

where $(x)_+ \equiv \max\{x, 0\}$. In this parameterization,

$$k_1 = \beta_1, \qquad k_{j+1} = k_j + \beta_{1+j} \;\; (j = 1, \dots, m),$$

so the break magnitude at τ_j is

$$\Delta k_j \equiv k_{j+1} - k_j = \beta_{1+j}. \tag{45}$$

If n denotes the number of segments, then $m = n - 1$ is the number of breakpoints. Because the fit is continuous, the number of free parameters is

$$K \;=\; \underbrace{1}_{\beta_0} + \underbrace{1}_{\beta_1} + \underbrace{m}_{\beta_{2:\,m+1}} + \underbrace{m}_{\tau_{1:\,m}} \;=\; 2n. \tag{46}$$

Segment-length and edge constraints. To avoid spurious short segments, we require a minimum *segment length* of h consecutive observations between adjacent breakpoints and between a breakpoint and the sample edges. We use $h = 4$ as the **default (strong)** setting and $h = 3$ for **weak** sensitivity checks. The edge buffer equals h.

Estimation. For fixed $\boldsymbol{\tau}$ the model is linear in $\boldsymbol{\beta}$ and is estimated by OLS. We search over admissible $\boldsymbol{\tau}$ subject to spacing/buffer constraints using a standard piecewise-linear routine (Python `pwlf`), which implements non-linear least squares with continuity enforced via Equation 44. For each admissible m we retain the fit with the lowest residual sum of squares; we then select m by the information criterion in Equation 47.

D.3 Model selection: BIC (primary) and sensitivity criteria

Let x be the number of observations used in the fit and SSR the residual sum of squares. Our *primary* selection criterion is BIC with the parameter count from Equation 46:

$$\mathrm{BIC}(n) \;=\; x\,\ln\bigl(\mathrm{SSR}/x\bigr) + K\,\ln x, \qquad K = 2n. \tag{47}$$

We also assess two sensitivities:

$$\mathrm{AIC}(n) = x\,\ln(\mathrm{SSR}/x) + 2K, \tag{48}$$

$$\mathrm{SSR}{+}p\,n \text{ with } p \text{ calibrated on unit } S\text{-curve simulations to prefer a single break near the inflection.} \tag{49}$$

In synthetic checks (available on request), BIC is more conservative and shows the best hit/false-positive trade-off; AIC tends to over-segment; the calibrated SSR+pn rule sits between them but is sensitive to p away from its calibration design. We therefore base reported results on BIC with $h = 4$.

D.4 Inference for slopes and break locations

For a given $\boldsymbol{\tau}$, OLS yields standard errors for $\boldsymbol{\beta}$ and hence for segment slopes k_j and break magnitudes Δk_j. Breakpoint locations are non-regular, so closed-form OLS SEs for τ_j are unreliable. We construct percentile bootstrap confidence intervals as follows:

1. Fit the preferred model to obtain $\widehat{y}_{g,i,t}$ and residuals $\widehat{\varepsilon}_{g,i,t}$.
2. Resample residuals with replacement within each country–technology series to form $\widehat{\varepsilon}^{*}_{g,i,t}$.
3. Set $y^{*}_{g,i,t} = \widehat{y}_{g,i,t} + \widehat{\varepsilon}^{*}_{g,i,t}$; re-estimate the full model (including breakpoint search) and store $\boldsymbol{\tau}^{*}, \boldsymbol{k}^{*}$.
4. Repeat B times (we use $B = 1000$ in practice) and take 2.5/97.5 percentiles for CIs on τ_j, k_j, and Δk_j.

CIs shown in exemplar panels in the main text are bootstrap intervals for τ_j.

D.5 Validation (design summary)

We validate detection on simulated data generated from (i) a piecewise-exponential process with multiplicative noise and (ii) a logistic process restricted to the early window (e.g., up to 20% penetration) with additive noise. We vary noise level, number of breaks, spacing, and slope changes, and evaluate (a) correct break count, (b) location error $|\widehat{\tau}_j - \tau_j|$, (c) sign accuracy of Δk_j, and (d) false positives. Detailed validation figures and tables are available upon request. Briefly, BIC with $h = 3$ performs best overall; AIC over-selects short segments; the calibrated SSR+pn rule is intermediate; and ISAT-based designs (below) underperform in our setting.

D.6 Indicator saturation (ISAT) as a sensitivity

We experimented with Indicator Saturation (Sucarrat et al. 2022) in two designs:

AR(1)-ISAT (mean via persistence).

$$y_t \;=\; \beta_0 + \phi\, y_{t-1} + \sum_{j=1}^{T} \gamma_j I_{j,t} + u_t, \tag{50}$$

optionally adding a deterministic trend. On synthetic exponential data with planted breaks and multiplicative noise, AR(1)-ISAT produced many false positives and, under large slope changes, frequently implied $\phi < 0$ (spuriously stationary dynamics). For cumulative adoption series, this mean representation is mis-specified.

Covariate-ISAT (mean via controls). We also specified a covariate-based mean (using the macro controls from the main paper) and saturated with impulses/trends/steps (IIS/TIS/SIS). Given short per-country series and imprecision in the mean, selection instability and location error were substantial relative to the piecewise approach.

Conclusion. ISAT is powerful when a credible mean specification exists or for long, high-frequency series. In our context (short panels; trending cumulative processes), the *continuous piecewise log-linear model with BIC and spacing constraints* provides the most reliable summary of non-causal, stylized breaks.

D.7 Practical details and robustness

- **Zeros and missing values.** We begin at the first strictly positive $S_{g,i,t}$; zeros are treated as missing for the log transform. Fits require at least h consecutive observations per segment.
- **Maximum segments.** The search respects $m_{\max}$ implied by h and the sample length; when $x < 2h+1$ only the no-break model is admissible.
- **Weak vs. strong.** Reported results use *strong* settings ($h = 4$, BIC). Sensitivities with $h = 3$, AIC, and SSR+pn yield similar cross-country patterns; counts increase modestly under AIC/$h = 3$, as expected.
- **Interpretation.** A positive (negative) Δk_j indicates an acceleration (deceleration) in the growth rate of $\ln S$. We do not assign mechanisms here; these are descriptive stylized facts.

D.8 Empirical summaries (counts and distributions)

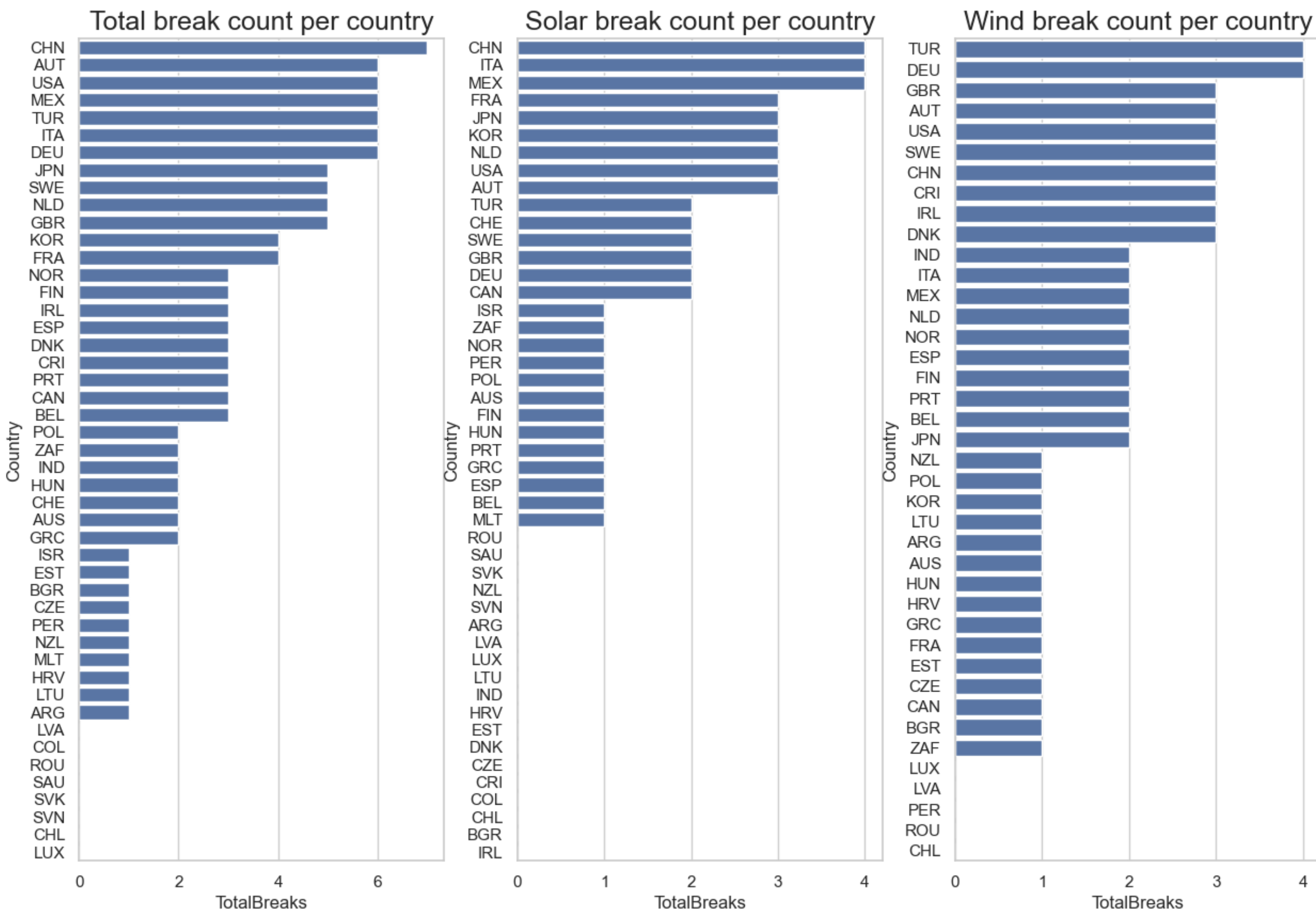


Figure D.1: Break counts by country under BIC selection with strong spacing ($h = 4$). Left: total (solar + wind). Middle: solar only. Right: wind only. Bars show the number of detected breaks per country.

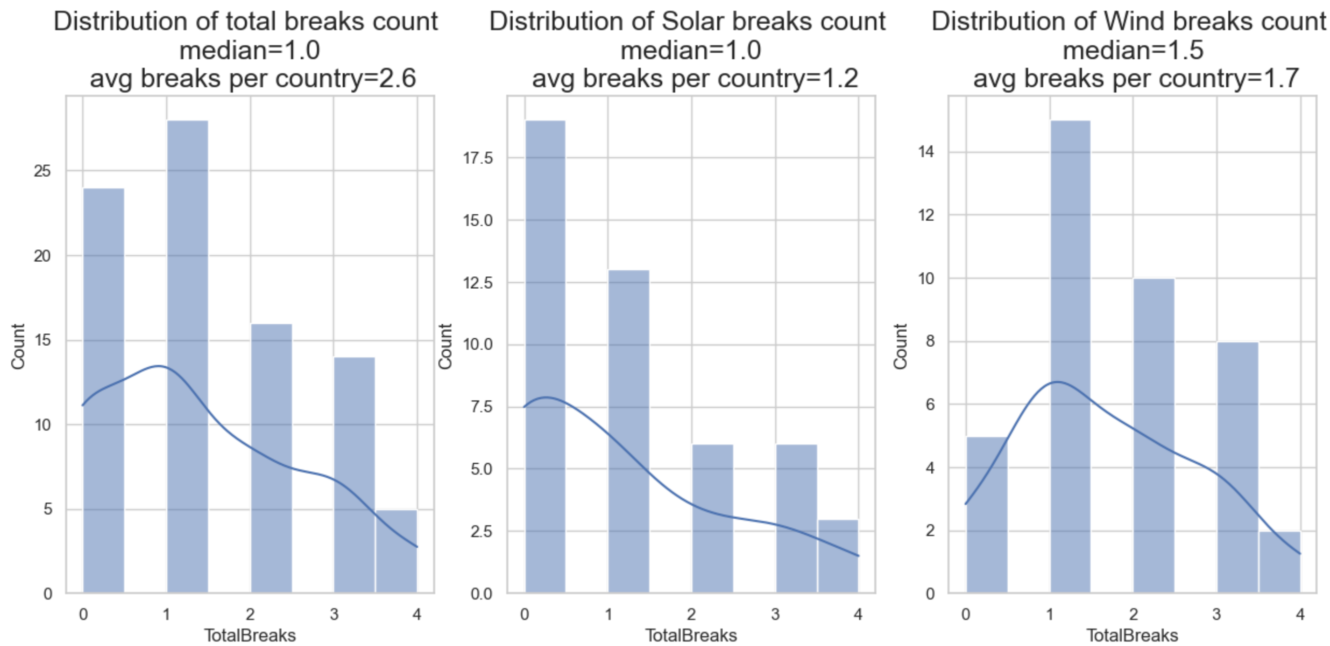


Figure D.2: Distribution of the number of breaks per country–technology adoption curve under BIC with $h = 4$. Left: combined. Middle: solar. Right: wind.

D.9 Policy-break proximity (non-causal diagnostic)

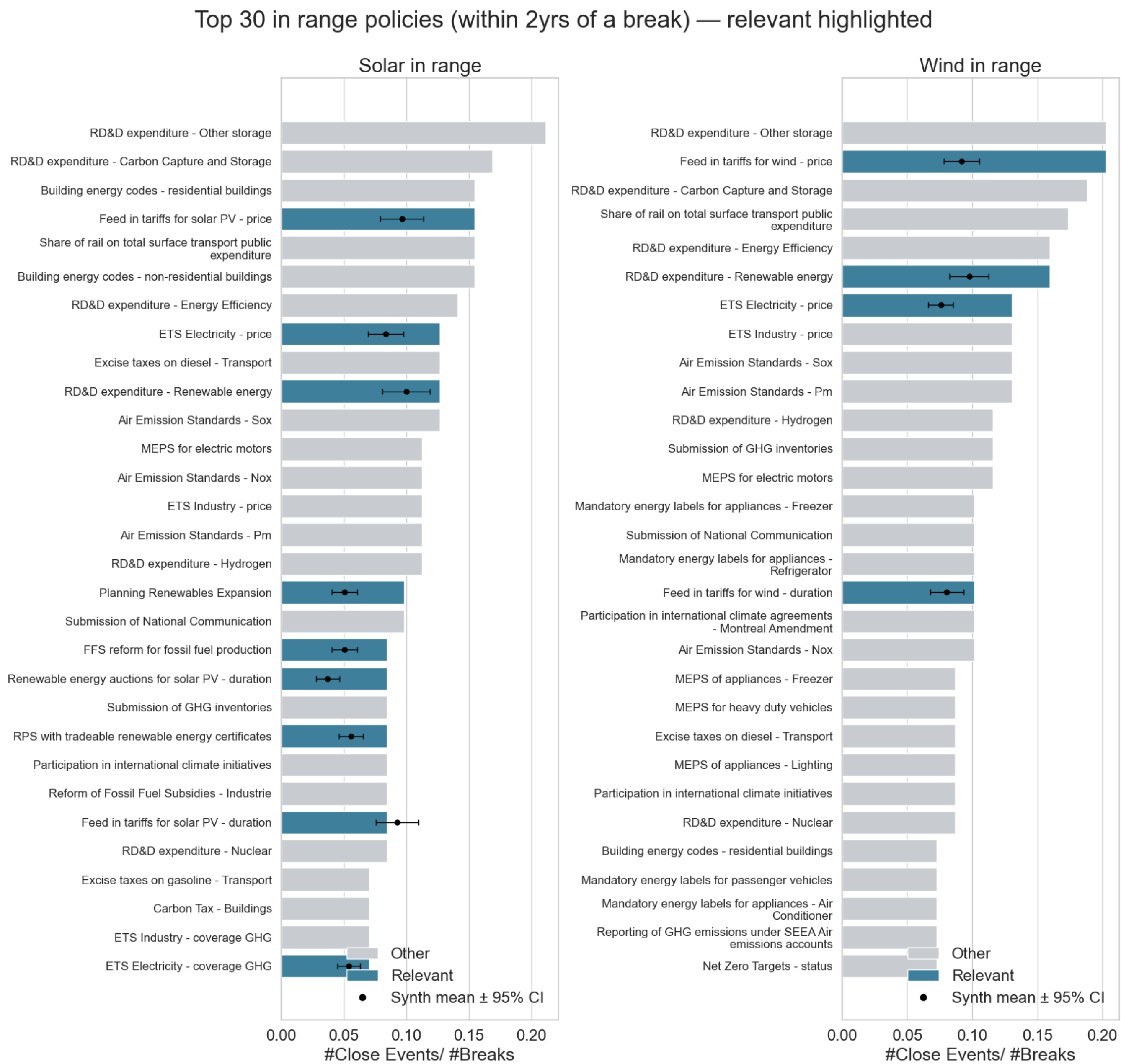


Figure D.3: Incidence of policy events within ±2 years of detected breaks (events-per-break). Left: solar. Right: wind. Top 30 policies by incidence shown. Mean and 95%CI for synthetically generated policies matched with real breaks also shown (black).

Following the break detection, we compute *policy-break proximity* as the count of policy events within a ±2-year window around each detected break, normalized by the number of breaks (events-per-break). This is a descriptive diagnostic only and is not interpreted causally because (i) proximity can occur by chance, (ii) policy intensity and variance differ by instrument, and (iii) break detection is estimated with uncertainty. Placebo re-timings (e.g., random shifts of event years) provide useful benchmarks (see Figure D.3).

We generate synthetic policies (see Appendix D.9.1 below) and compare with real data and find that the close proximity incidence of several relevant policies is higher for the real data than the synthetic simulated policy events.

D.9.1 Construction of Synthetic Policy Events

This subsection describes the procedure for generating synthetic timelines of *policy events* only. The objective is to reproduce first-order empirical regularities observed in the historical record-(i) how many events occur per country, (ii) how far apart they are in time, (iii) when in the calendar they tend to occur, and (iv) the distribution of event magnitudes-while respecting a fixed calendar window and simple spacing constraints.

Calibration to the data. For each policy, the observed data are used to calibrate four ingredients:

1. **Counts per country.** Let K denote the number of policy events observed in a country. The empirical mean across countries provides a rate λ_{pol}, and synthetic counts are drawn as

$$K \sim \text{Poisson}(\lambda_{\text{pol}}).$$

2. **Year-of-occurrence marginal.** An empirical discrete distribution over calendar years $t \in \{1990, \dots, 2022\}$ is formed by tabulating observed event years. Sampling the first event from this marginal preserves historical clustering and secular timing patterns.

3. **Inter-event separations.** For countries with at least two observed events, adjacent differences Δt are computed. Their mean and dispersion parameterize a family of waiting-time distributions used to draw synthetic separations between consecutive events. Two options are supported:

   - a *normal* distribution on the original scale (with nonnegativity enforced ex post), and
   - a *lognormal* distribution on the positive scale, which accommodates right-skewed waiting times.

   Drawn separations are mapped to the integer-year grid (by rounding up) so that $\Delta t \in \{1, 2, \dots\}$.

4. **Event magnitudes.** From observed event sizes, an empirical distribution is constructed; optionally, a *significance* filter is applied so that only observations with $|\text{EventSize}| > 2$ enter calibration. Synthetic magnitudes are then generated by one of:

   - *empirical resampling* (bootstrap from the calibrated sample),
   - *normal* draws with mean and variance matched to the calibrated sample, or
   - *lognormal* draws (for strictly positive sizes) with parameters matched to the calibrated mean and variance.

   When a significance rule is imposed, acceptance–rejection is used so that synthetic magnitudes satisfy $|\text{EventSize}| > 2$.

Synthetic timeline generation (per country, per policy). Given the calibrated components:

1. Draw a synthetic count $K \sim \text{Poisson}(\lambda_{\text{pol}})$.

2. If $K = 0$, return an empty timeline. If $K = 1$, draw a single year Y_1 from the empirical year-of-occurrence marginal on $[1990, 2022]$.

3. If $K > 1$, construct a sequence of event years as follows:

$$Y_1 \sim p_t, \qquad Y_{i+1} \;=\; Y_i + \Delta t_i, \quad i = 1, \ldots, K-1,$$

   where each Δt_i is drawn from the selected separation family and rounded to an integer ($\Delta t_i \geq 1$). Any realized years beyond 2022 are discarded to respect the calendar window.

4. For each realized event year, draw an event magnitude from the selected size distribution; if a significance rule is active, resample until $|\text{EventSize}| > 2$.

Assumptions and constraints. The construction enforces the fixed calendar window 1990–2022 and an integer-year grid for event timing. Counts and separations are calibrated separately for each policy; conditional on these calibrations, draws are independent across countries. The procedure targets first-order features (mean counts, spacing, timing marginals, and size distributions); higher-order cross-country or cross-policy dependence is not imposed.

D.10 Limitations

- The log-linear approximation is most appropriate in early growth; segments after maturation may capture genuine slowdowns but should be interpreted cautiously.
- Even with BIC and spacing constraints, short series limit precision for τ_j.
- Breaks summarize pattern changes; because we do not account for other co-moving confounders, we cannot conclude causal relationships to policies.