Do Agronomic Prescription Mandates Reduce Pesticide Use?*

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Abstract

Mandatory agronomic prescriptions are an increasingly common regulatory instrument that inserts a licensed intermediary into pesticide purchase decisions, yet their causal effects on pesticide use remain largely unknown. We exploit Québec's 2018 prescription requirement for neonicotinoids and atrazine as a natural experiment to estimate how mandatory prescriptions affect pesticide use. Using annual compound-level sales data and combining difference-in-differences, slope-break, and synthetic control methods, we identify three main findings. First, the regulation accelerated the exit of atrazine, reducing sales by approximately 92 percent on average relative to the counterfactual trajectory. Second, despite being a primary target of the reform, we find no evidence that it reduced neonicotinoid sales, once we account for their pre-existing downward trend. Third, the response across neonicotinoid substitutes was heterogeneous: diamide sales nearly tripled, while the adoption of sulfoximines and butenolides slowed markedly relative to their pre-reform trajectories. These findings demonstrate that agronomic prescription mandates can be effective in reducing the use of targeted pesticides. However, they can also generate substantial shifts in pesticide use across chemical classes, highlighting the importance of accounting for cross-compound substitution when evaluating prescription-based pesticide policies.

JEL Codes: Q53, Q58, L51, C21.
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1 Introduction

Does requiring farmers to obtain a professional pesticide prescription reduce pesticide use? Governments increasingly rely on mandatory agronomic prescriptions as an alternative to taxes, bans, and use restrictions for regulating high-risk pesticides.[1] Under this approach, farmers must obtain authorization from a licensed agronomist before purchasing designated compounds. The objective is to optimize pesticide use by combining technical expertise with professional accountability, thereby reducing unnecessary applications while preserving access when treatment is agronomically justified. Despite the growing adoption of this regulatory instrument, there is little causal evidence on whether it actually reduces pesticide use or how producers adjust their input choices. This paper addresses these questions by exploiting Québec's 2018 agronomic prescription reform as a natural experiment.

The economic effects of prescription requirements are theoretically ambiguous. Agricultural pesticide markets are characterized by two well-known distortions. First, pesticides provide insurance against uncertain yield losses, leading farmers to apply treatments even when the expected marginal return is low or negative (Lichtenberg and Zilberman, 1986; Schreinemachers and Tipraqsa, 2012). Second, decisions regarding pesticide choice, dosage, and timing are technically complex, while product recommendations are frequently provided by agents with commercial incentives that may not coincide with socially efficient pesticide use (Möhring et al., 2020).[2] Mandatory prescriptions seek to address these distortions by inserting a professional intermediary into the purchase decision. If excessive pesticide use primarily reflects informational frictions or weak agronomic oversight, the policy should reduce demand for targeted compounds. However, if farmers can readily substitute toward unregulated pesticides that provide similar agronomic services, the policy may simply reallocate demand across active ingredients rather than reduce overall pesticide use. Evaluating such policies solely through changes in the sales of targeted compounds can therefore be misleading, and understanding how producers substitute across pesticide classes becomes essential for judging whether prescription mandates achieve their stated objectives.

Québec's 2018 agronomic prescription reform provides a natural setting to examine how prescription-based regulation affects pesticide use. Beginning with the 2018 growing season, farmers were required to obtain a written prescription from a licensed agronomist before purchasing or applying several high-risk pesticides, including atrazine herbicide and three widely used neonicotinoid insecticides. The prescription had to specify the active ingredient, application rate, and agronomic justification, with the prescribing agronomist assuming professional responsibility for the recommendation. Unlike pesticide taxes, the reform did not alter product prices, and unlike product bans, it did not prohibit pesticide use. Instead, it changed the institutional process through which regulated pesticides could be accessed by increasing the administrative cost of adoption while introducing an information and accountability mechanism into pesticide-use decisions.

Existing research has focused primarily on pesticide taxes (Böcker and Finger, 2016; Nielsen et

[1] Brazil has required agronomic prescriptions for pesticide purchases since 1989 (Soares and Porto, 2012), while the European Union has required Member States to establish frameworks for professional pesticide advice and integrated pest management since Directive 2009/128/EC (European Parliament and Council, 2009). Québec followed with a mandatory prescription system for specified pesticides in 2018.

[2] The distortion is not hypothetical: global pesticide use grew by more than 80 percent over the past three decades (Carvalho, 2017), environmental and public-health costs in the United States alone exceed several billion dollars annually (Pimentel, 2005), and documented harms range from neonicotinoid disruption of pollinator communities (Woodcock et al., 2017; European Food Safety Authority , EFSA) to atrazine contamination of drinking-water aquifers (Hayes et al., 2010).

al., 2023), product bans (Schneider et al., 2018), and voluntary advisory programs (Lohr et al., 1999), leaving the effects of mandatory prescription requirements largely unexplored. By providing the first causal evaluation of this regulatory instrument, our paper fills this gap and advances our understanding of how environmental regulations that restrict access to pesticides, rather than changing their prices or prohibiting their use, affect producer behavior.

Implementing this causal evaluation is challenging because prescription requirements can affect pesticide demand through several distinct channels. The most direct channel is a reduction in the use of regulated compounds if professional oversight discourages unnecessary applications. A second possibility is that prescriptions have little effect: because farmers were already following agronomic recommendations before the reform, or because a compound's primary channel of use—such as insecticide-treated seed—sits outside what the requirement can practically reach. A third possibility is that restricting access to selected pesticides induces reallocation toward unregulated substitutes, and that this reallocation need not favor the closest available alternative: producers may instead diversify away from an entire chemical family once its flagship compounds are regulated, anticipating that regulation will eventually reach the rest of it. Under this scenario, the regulation may substantially alter the composition of pesticide use while producing only modest changes for any single targeted compound. Distinguishing among these mechanisms is essential for understanding the effectiveness of prescription-based regulation.
To identify these effects, we exploit Québec's prescription reform as a natural experiment using annual compound-level pesticide sales. Our empirical strategy combines three complementary approaches. First, we estimate two-way fixed-effects difference-in-differences models comparing regulated compounds with counterfactual compounds selected to share similar cropping systems and market conditions, while remaining sufficiently distinct in their agronomic function and substitution potential.[3] Second, we estimate slope-break specifications that separately identify an immediate level effect and a change in the post-reform trajectory. Third, we employ synthetic control methods to construct data-driven counterfactuals for the principal treated compounds. Relying on three approaches with distinct identifying assumptions provides a stringent assessment of the reform: where they agree, that agreement is itself evidence; where they diverge, as they do for neonicotinoids, the divergence reveals which specification was misled by a confound.

Our analysis highlights three key findings. First, the prescription requirement generated a pronounced structural break in atrazine sales, placing them well below the counterfactual trajectory. Second, despite being a primary target of the reform, we find no evidence that it reduced neonicotinoid sales, once we account for their pre-existing downward trend.[4] Third, the response among neonicotinoid substitutes was heterogeneous, and in the opposite direction from what simple substitution would predict. Sales of diamide insecticides increased substantially following the reform, whereas the adoption of sulfoximines and butenolides—chemically closer to neonicotinoids—slowed relative to their pre-reform trajectories, even though neither was itself regulated. A plausible explanation, developed in Section 4.2.4, is that producers and agronomists reallocated away from the entire nicotinic-receptor-active family in anticipation of its regulatory trajectory, rather than toward the nearest chemical substitute—an account that does

[3] Potential pesticide substitutes are agronomically interchangeable compounds that target the same pests or provide similar crop protection functions.

[4] One possible explanation is that neonicotinoids are commonly delivered through treated seeds rather than as separately purchased pesticides. Because the prescription requirement primarily governs direct pesticide purchases, a substantial margin of neonicotinoid use may have remained outside the reform's immediate regulatory scope.

not require neonicotinoid sales themselves to have fallen.

Related Literature. This paper contributes to three strands of the literature. First, it contributes to the literature on pesticide regulation and the effectiveness of environmental policy instruments. Most existing studies examine price-based interventions, particularly pesticide taxes calibrated to environmental hazard. A substantial body of European evidence points to the limitations of these instruments. Fémenia and Letort (2016) show that pesticide demand is relatively price inelastic in the short run, implying that even large tax increases generate only modest reductions in use. Similarly, Böcker and Finger (2016) document a trade-off between effectiveness and political feasibility: tax schemes capable of substantially reducing pesticide use impose welfare costs that are often difficult to sustain, whereas politically feasible policies induce only limited behavioral responses. Finger et al. (2017) and Jacquet et al. (2011) further argue that meaningful reductions in pesticide use generally require broader changes in production practices rather than price incentives alone. We contribute to this literature by providing the first causal evidence on a non-price regulatory instrument that conditions access to pesticides through mandatory agronomic prescriptions.

Second, the paper contributes to the literature on information frictions and intermediary incentives in agricultural input markets. Excess pesticide use has been linked to both imperfect information among farmers and commercial incentives that may bias pesticide recommendations. Schreinemachers and Tipraqsa (2012) document that pesticide use intensifies with agricultural commercialization, consistent with suppliers influencing pesticide demand beyond agronomic necessity. Möhring et al. (2020) argue that durable reductions in pesticide use require changes in the advisory chain itself, highlighting the importance of intermediary incentives. In our setting, the prescription requirement inserts a licensed agronomist into the purchasing process, but its effectiveness depends on whether the relevant production decision is actually subject to that intervention. For neonicotinoid seed treatments, adoption decisions are largely determined upstream through seed suppliers and established production systems, leaving limited scope for prescriptions to influence farmer behavior. The absence of a robust treatment effect is therefore consistent with the limited reach of the prescription requirement over the main channel through which neonicotinoids enter production.

Third, the paper contributes to the literature on behavioral responses to environmental regulation. A central insight from this literature is that regulations targeting one dimension of behavior often induce adjustment along other margins. For example, Greenstone (2002) documents the relocation of economic activity in response to environmental regulation, while Ryan (2012) shows that stricter environmental standards can impose substantial sunk adjustment costs that reshape industry structure. In the context of pesticide regulation, this logic suggests that restricting access to one group of compounds may increase demand for close substitutes. The closest evidence is provided by Perry and Moschini (2020), who show that the adoption of neonicotinoid seed treatments in U.S. maize displaced foliar pyrethroid and organophosphate applications. We extend this literature by showing that a prescription requirement can generate anticipatory reallocation across a whole chemical family, rather than narrow substitution toward the nearest available alternative. Diamides, which share no mode of action with the regulated compounds, saw sales rise substantially, while sulfoximines and butenolides—chemically closer to neonicotinoids, and later brought under the same requirement themselves—lost momentum.

Outline. The remainder of the paper is organized as follows. Section 2 describes the institutional setting of Québec's 2018 agronomic prescription reform. Section 3 presents the data, the construction of the treatment and control groups, and the empirical strategy. Section 4 reports the main empirical findings and presents a series of robustness analyses. Section 5 concludes.

2 Institutional Background

In 2018, Québec introduced a province-wide mandatory agronomic-prescription requirement for agricultural pesticides, making it the only North American jurisdiction with such a regime. This institutional change provides the basis for the empirical analysis that follows.

Agricultural pesticide use in the province expanded substantially after the mid-1990s, driven by intensification in corn and soybean production (Carvalho, 2017; Pimentel, 2005). Two compound classes concentrated regulatory concern: atrazine, a persistent groundwater contaminant with endocrine-disrupting effects on aquatic organisms (Hayes et al., 2010; Pest Management Regulatory Agency , PMRA), and neonicotinoid insecticides, whose use became nearly universal in corn and soybean seed treatments after 2005 despite accumulating evidence of adverse effects on pollinators and insect biomass more broadly (Woodcock et al., 2017; European Food Safety Authority , EFSA). Québec had already implemented several measures to reduce pesticide risks.[5] Nevertheless, concerns over atrazine contamination and neonicotinoid use remained prominent. In response, Québec introduced a mandatory prescription requirement for selected high-risk pesticides in 2018. The reform established a compound-specific regulatory mechanism with no precedent in Canadian agricultural regulation.

Pesticide governance in Québec is structured around two provincial instruments: the *Loi sur les pesticides* (LP, RLRQ c. P-9.3) and the *Code de gestion des pesticides* (CGP). Together, these instruments classify active ingredients into several regulatory classes based on environmental and health risks, and regulate their sale, storage, and agricultural use. In this context, the purchase and application of certain regulated compounds require a written prescription issued by a certified agronomist who is a member of the *Ordre des agronomes du Québec* (OAQ).[6]

The prescription requirement was progressively applied to the highest-risk agricultural pesticides in Québec, following the *Stratégie québécoise sur les pesticides 2015–2018*, and became fully binding from the 2018 growing season onward. From that point, the purchase and application of atrazine, chlorpyrifos, and three neonicotinoids (clothianidin, imidacloprid, and thiamethoxam), as well as certain insecticide-coated seeds, required prior agronomic justification and prescription.[7]

The prescription mechanism operates as a sequential, multi-actor process. A farmer wishing to pur-

[5] Before 2018, Québec had adopted several pesticide-mitigation policies, including the Pesticides Act (1987) and the Pesticides Management Code (2003), which restricted cosmetic pesticide and herbicide use in urban areas, limited applications near sensitive sites and waterways, promoted integrated pest management, and were complemented by a 2001 ban on chemical herbicides on Crown forest lands.

[6] The prescription is a standardized legal document specifying the parcel, crop, authorized active ingredient(s) (up to three), permitted quantities, expiry date, and the signing agronomist's OAQ membership details. Non-compliance is subject to administrative penalties ranging from $250 to $2,500 and criminal fines of up to $100,000 (LP, art. 87; CGP, art. 86.4–86.6).

[7] The compound *chlorpyrifos* was also targeted by the regulation in April 2019. However, due to its harmful health effects, the Government of Canada announced in 2019 its intention to ban its sale nationwide. This ban came into effect in December 2021. Consequently, we excluded this ingredient from our treated group. Therefore, the list of targeted compounds retained for the analysis is *atrazine* and the three neonicotinoids *clothianidin*, *imidacloprid*, and *thiamethoxam*.

chase a regulated compound must first obtain a standardized document provided by a licensed agronomist. This document is then presented at the point of retail sale as the legal condition for purchase. The MELCC staff carry out verification missions of agronomic prescriptions among farmers and, when necessary, report any deficiencies in prescription quality to the agronomists' professional association. The latter may summon the agronomists concerned before the professional disciplinary committee and can restrict or suspend their right to issue prescriptions (MELCC, 2021).

3 Empirical Strategy

This section describes our empirical strategy for estimating the effect of Québec's mandatory prescription policy on agricultural pesticide use. The main identification challenge is that pesticide use may have evolved differently across treated and untreated units even in the absence of the reform. We address these identification concerns using three estimators that rely on progressively weaker assumptions about pre-treatment trends. The two-way fixed effects (TWFE) specification serves as a baseline and requires parallel trends in levels between treated and control units. The slope-break specification relaxes this assumption by allowing for differential linear trends prior to 2018 and identifies the treatment effect from deviations relative to these trends. The synthetic control approach imposes no functional-form restriction on pre-treatment dynamics, requiring only that a weighted combination of control units reproduces the treated unit's pre-reform trajectory. Before outlining the specific estimators, we first describe the data employed in our analysis.

3.1 Data

Data source. The primary data come from the *Ministère de l'Environnement, de la Lutte contre les Changements Climatiques, de la Faune et des Parcs* (MELCC, 2021), which compiles annual records of pesticide sales at the compound level for the entire province. The dataset covers the period 2000–2022 and reports the total quantity sold (in kilograms of active ingredient) for each registered commercial product.[8] The outcome variable in all specifications is the logarithm of annual quantity sold.[9]

Treated groups. The treated compounds comprise the herbicide atrazine, the three neonicotinoid insecticides (clothianidin, imidacloprid, and thiamethoxam), and potential substitutes. The potential substitutes for the regulated compounds consist of agrochemically related insecticides that target the same pest spectrum but were not subject to the prescription requirement, thereby providing a natural setting to examine substitution effects. Although atrazine has numerous agronomic alternatives, they are dispersed across several herbicide classes rather than concentrated in a single close substitute.[10] Conse-

[8] For three herbicides used as atrazine controls—2,4-D, MCPA, and dicamba—we aggregate sales to the active-ingredient level because multiple commercial formulations may contain the same active ingredient under different trade names; for 2,4-D, all registered products whose names begin with "2,4-D" (excluding 2,4-DB) are grouped into a single series, and MCPA and dicamba are treated analogously (Table A.1).

[9] For compound-year cells with zero recorded sales, we assign $\ln(0.01)$, imposing a lower bound of 0.01 kg. Across all estimation panels, 108 compound-year cells (17.4% of observations) are imputed; these are concentrated in the substitute panels (sulfoximines/butenolides and diamides) before 2010, when these classes were pre-diffusion in Québec.

[10] These include (i) photosystem II inhibitors, such as simazine and metribuzin; (ii) HPPD inhibitors, including mesotrione, tembotrione, and isoxaflutole; (iii) ALS inhibitors, such as nicosulfuron and rimsulfuron; (iv) synthetic auxins, including 2,4-D and dicamba; (v) PPO inhibitors, such as flumioxazin and sulfentrazone; and (vi) non-selective herbicides, notably glyphosate

quently, producers responding to the prescription requirement are unlikely to substitute toward a single active ingredient. Instead, they are more likely to re-optimize weed-management strategies by combining herbicides from different chemical classes or adjusting application practices. For this reason, we do not identify a potential substitute for atrazine. Our analysis focuses on the reduction in its use rather than attempting to identify substitution toward a specific herbicide.

We identify diamides, sulfoximines, and butenolides as potential substitutes for neonicotinoids based on agronomic evidence that they provide effective control of many of the same economically important insect pests while relying on distinct active ingredients. Anthranilic diamides have been shown to achieve pest control comparable to neonicotinoid seed treatments in vegetable crops (Lahm et al., 2009; Larson et al., 2012; Schmidt-Jeffris and Nault, 2016). Sulfoximines and butenolides were subsequently developed as novel insecticide classes targeting the same nicotinic acetylcholine receptor as neonicotinoids, while addressing resistance and other limitations associated with earlier chemistries (Sparks et al., 2013; Nauen et al., 2015; Sparks et al., 2019). Because these compounds offer agronomically viable alternatives without being subject to the prescription requirement, they represent the most plausible candidates for substitution following the 2018 reform.

Control groups. For each treatment panel, we construct a control group of compounds that were not subject to the prescription requirement and can therefore be used to estimate the counterfactual evolution of treated compounds. Control compounds are selected according to three criteria. First, they are used in similar cropping systems and therefore face comparable weather conditions and market demand. Second, they are not intended to control the same pests as the treated compounds, reducing the scope for reform-induced substitution and potential contamination of the control group. Third, they exhibit sufficiently continuous sales throughout the sample period to permit reliable estimation.

According to these criteria, the control group for atrazine consists of herbicides used in the same cereal and broadleaf cropping systems.[11] The control group for the sulfoximine and butenolide consists of selective aphicides and broad-spectrum pyrethroids. For the diamide, the control group comprises ecdysone agonists and broad-spectrum pyrethroids, whereas the control group for the neonicotinoid analysis includes synthetic pyrethroids and organophosphate insecticides used in the same cropping systems. The complete list of control compounds for each treatment panel is reported in Tables A.1–A.4.

3.2 Baseline Estimation: Two-Way Fixed Effects

The outcome variable, y_{ct}, is the natural logarithm of the annual quantity of compound c sold in year t, in kilograms of active ingredient. The baseline specification is

$$y_{ct} = \alpha_c + \gamma_t + \beta\, T_c \times \mathbf{1}_{\{t \geq 2018\}} + \varepsilon_{ct}, \tag{3.1}$$

and glufosinate. Among these alternatives, simazine is the closest chemical substitute because it belongs to the same triazine family. Simazine shares the HRAC Group 5 mode of action (photosystem II inhibition), and has overlapping applications in corn production. However, simazine accounts for only a small share of the Québec herbicide market (0.1 percent of provincial herbicide sales, 2012–2017).

[11] To assess the sensitivity of the results to the composition of the control group, we classify these compounds into three alternative control groups based on their agronomic proximity to atrazine.

where α_c are compound fixed effects, γ_t are year fixed effects, T_c indicates the treated compounds, and $\mathbf{1}_{\{t\geq 2018\}}$ marks the years from the reform onward. The compound fixed effects absorb time-invariant differences in scale across active ingredients; the year fixed effects absorb shocks common to all compounds, such as weather and market-wide price movements. The coefficient of interest, β, is the average change in the log sales of the treated compounds, relative to the controls, from 2018 onward.

We estimate Equation (3.1) separately for each treated class: atrazine, neonicotinoids, sulfoximines and butenolides, and diamides. Each estimation uses the compounds in the treated class together with a matched set of control compounds.[12] Because the treatment indicator T_c is defined at the class level, the coefficient β identifies the average treatment effect for the corresponding compound class rather than for individual active ingredients. Table 1 reports the estimates for the four treated classes side by side.

The coefficient β admits a causal interpretation under the parallel trends assumption. Specifically, in the absence of the prescription requirement, the average evolution of pesticide sales for treated compounds would have been the same as that of the control compounds. In other words, any pre-existing differences between treated and control compounds are assumed to be time-invariant, while common shocks affect both groups similarly over time. Under this condition, the year fixed effects γ_t capture the common counterfactual time path, and the within-compound change in y_{ct} after 2017 identifies the treatment effect.

To assess the validity of the parallel trends assumption and to characterize the dynamic effects of the reform, we estimate the following event-study specification:

$$y_{ct} \;=\; \alpha_c + \gamma_t + \sum_{\substack{k=\underline{k} \\ k\neq -1}}^{\bar{k}} \theta_k \, T_c \cdot \mathbf{1}_{\{t-2018=k\}} \;+\; \varepsilon_{ct}, \tag{3.2}$$

where k indexes event time relative to the reform year, and $k=-1$ (corresponding to 2017) is omitted as the reference period. The coefficients θ_k trace the dynamic evolution of treated compounds relative to the control group before and after the reform.

The pre-treatment coefficients, $\{\theta_k\}_{k<0}$, provide a direct assessment of the parallel-trends assumption. Under this assumption, coefficients $\{\theta_k\}_{k<0}$ should be jointly indistinguishable from zero, indicating the absence of differential pre-treatment trends between treated and control compounds.

3.3 Slope-Break Specification

The baseline two-way fixed-effects (TWFE) specification in Equation (3.1) estimates the average post-2017 effect of the reform under the parallel trends assumption. However, for compounds exhibiting pre-existing secular trends, this specification may attribute part of the underlying trend to the policy. To distinguish an immediate level shift from a change in the post-reform trajectory, we estimate a slope-break specification that allows the reform to affect both the level and the slope of pesticide sales, following the trend-adjusted difference-in-differences framework discussed by Angrist and Pischke (2009):

$$y_{ct} = \alpha_c + \gamma_t + \delta_1 T_c \times \mathbf{1}_{\{t\geq 2018\}} + \delta_2 T_c \times \mathbf{1}_{\{t\geq 2018\}} \cdot (t-2018) + \varepsilon_{ct}, \tag{3.3}$$

[12] Tables A.1–A.4 report the complete list of control compounds for each treatment panel.

where compound fixed effects α_c absorb time-invariant heterogeneity across compounds, and year fixed effects γ_t capture aggregate shocks common to all compounds. The coefficient δ_1 measures the discrete level shift at the reform date; δ_2 captures the post-2017 change in slope relative to the common time path. Identification rests on the parallel-trends assumption applied to the pre-2018 window.

In the sulfoximine and butenolide panel, treated compounds were still entering the Québec market during the pre-reform years, producing a substantial differential pre-trend relative to the control group. Imposing common year effects would conflate this entry dynamic with the reform's causal effect. Following Pischke (2005), we augment Equation (3.3) with a treated-group-specific linear pre-treatment trend for sulfoximine and butenolide panel:

$$y_{ct} = \alpha_c + \gamma_t + \rho\, T_c \cdot (t - t_0) + \delta_1 T_c \times \mathbf{1}_{\{t \geq 2018\}} + \delta_2 T_c \times \mathbf{1}_{\{t \geq 2018\}} \cdot (t - 2018) + \varepsilon_{ct}, \tag{3.4}$$

where $t_0 = 2014$ denotes the first year of positive treated-group sales, and ρ captures the differential pre-2018 growth trend. Under Equation (3.4), the coefficient δ_2 measures the post-reform change in slope relative to the pre-existing trend.

3.4 Synthetic Control Method

The difference-in-differences estimators described above identify treatment effects under assumptions on the evolution of untreated outcomes. As a complementary approach, we estimate treatment effects using the synthetic control method (SCM), which constructs a data-driven counterfactual for the treated group without imposing a parametric specification on pre-treatment dynamics. SCM, developed by Abadie et al. (2010) , is particularly useful when treated and control units exhibit heterogeneous trends prior to the intervention, provided that the treated trajectory can be closely reproduced by a weighted combination of untreated compounds.

The synthetic control method assigns non-negative weights to donor compounds so that a weighted combination of control units closely matches the treated group's pre-2018 trajectory. The weights are chosen to minimize the pre-treatment distance between the treated unit and its synthetic counterpart over a set of lagged outcomes, subject to the weights summing to one. The estimated treatment effect is the post-2017 difference between the observed outcome and the synthetic control. To improve comparability across compounds, all series are normalized to their base-year value.

Statistical inference follows the permutation procedure proposed by Abadie et al. (2015). The treatment is iteratively reassigned to each donor compound, and the synthetic control is re-estimated to generate the distribution of placebo effects under the null hypothesis of no treatment effect. The one-sided p-value is computed as the proportion of placebo units whose post-to-pre RMSPE ratio is at least as large as that of the treated unit. Following Abadie et al. (2015), we exclude placebo units with poor pre-treatment fit, defined as having a pre-treatment RMSPE exceeding twice that of the treated unit. Synthetic controls are implemented using the algorithm of Abadie et al. (2011).

3.5 Inference With Few Clusters

The number of clusters ranges from five to thirteen across estimation panels. With so few clusters, conventional cluster-robust standard errors may be downward biased and over-reject the null hypothesis (Cameron et al., 2008). We therefore report wild cluster bootstrap p-values following Roodman

et al. (2019), based on 999 bootstrap replications with Rademacher weights, alongside conventional cluster-robust standard errors. When the two inference procedures differ, we base our conclusions on the bootstrap results.

The wild cluster bootstrap provides valid inference with a limited number of clusters, provided that at least two treated clusters are available (MacKinnon and Webb, 2017). This condition is satisfied for all estimation classes except atrazine. For atrazine, we therefore place greater weight on the joint F-test from the slope-break specification and the synthetic control permutation test for statistical inference.

3.6 Estimation Windows

The difference-in-differences and slope-break specifications are estimated over a common 2012–2022 window for all four classes. We do not extend these regressions to an earlier pre-reform period because sulfoximines and butenolides entered the Québec market only around 2012. Applying a common window across all classes also keeps the estimates comparable across pesticide classes. The synthetic-control analysis instead uses the longest pre-treatment history available for each class, conditional on positive sales, because a longer pre-treatment period provides more information for constructing the synthetic counterfactual and assessing its pre-treatment fit. For atrazine and neonicotinoids, this period is 2000–2017; for diamides, 2008–2017; and for sulfoximines and butenolides, 2012–2017.

The two approaches therefore use different calendar windows by design. The difference-in-differences and slope-break specifications prioritize comparability by using a common estimation window across pesticide classes, whereas the synthetic-control analysis prioritizes counterfactual fit by using the longest credible pre-treatment history available for each class, beginning with its observed entry into the market.

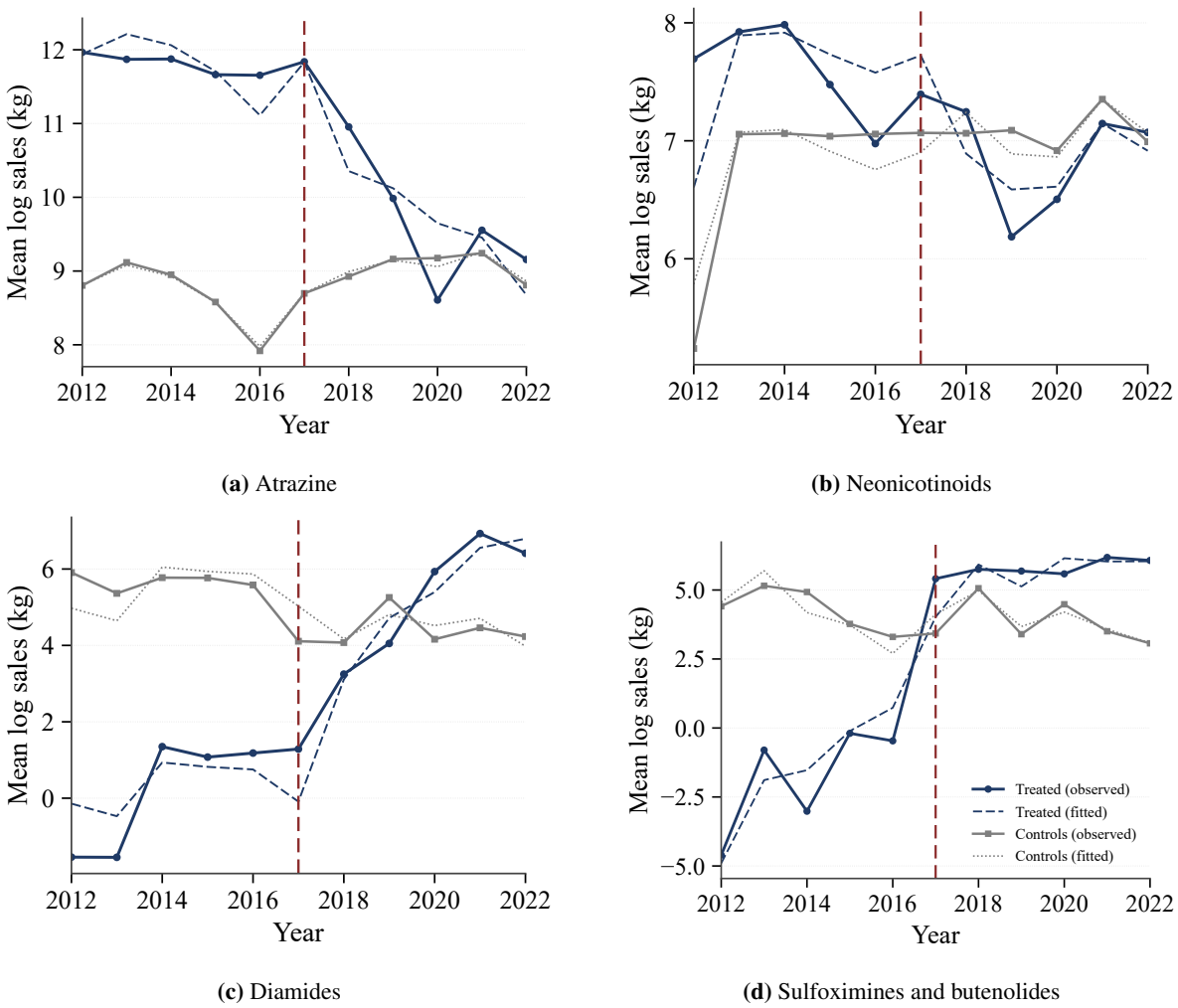
4 Results

This section presents the main empirical results on the effects of the 2018 prescription requirement on pesticide use. We first report the baseline difference-in-differences and slope-break estimates for the four treated classes: atrazine, neonicotinoids, diamides, and sulfoximines/butenolides. We then examine each class separately, assessing the identifying assumptions and complementing the regression estimates with event-study and synthetic-control analyses.

4.1 Main Results

This section provides an overview of the main findings. The subsequent class-by-class discussions examine each estimate in detail, assess the corresponding identifying assumptions, and evaluate the evidence from alternative estimators.

Table 1 presents the class-specific estimates. Columns (1), (3), and (6) report the average post-2017 effects, $\hat{\beta}$, from the two-way fixed-effects specification in Equation (3.1) for atrazine, neonicotinoids, and diamides, respectively. Columns (2), (4), and (5) report the corresponding slope-break estimates for atrazine, neonicotinoids, and sulfoximines/butenolides, respectively. These specifications distinguish between an immediate level shift following the reform, $\hat{\delta}_1$, and a change in the post-reform trajectory, $\hat{\delta}_2$. For sulfoximines and butenolides, the specification additionally accounts for the treated group's differential pre-reform trend using Equation (3.4).



(a) Atrazine

(b) Neonicotinoids

(c) Diamides

(d) Sulfoximines and butenolides

FIGURE I: Mean log quantity sold by compound group: treated and control trajectories.

Notes: This figure plots, by year, the mean log quantity sold (in kilograms of active ingredient) for each treated compound group and its control group. Solid lines show the treated group as observed; long-dashed lines show the controls as observed. Short-dashed lines (with markers) overlay the fitted trajectories from the slope-break specification in Equation (3.3) for panels (a)–(c), and from the pre-trend-adjusted variant in Equation (3.4) for panel (d). Fitted lines include the absorbed compound and year fixed effects, so the post-2017 gap between observed and fitted visualises the estimated reform effect $\hat{\delta}_1 + \hat{\delta}_2(t - 2018)$. The vertical dashed line marks the prescription boundary (2017, the year before the requirement took effect). Treated- and control-group compositions are listed in Table 1 and Section 4.

The estimates reveal substantial heterogeneity in the response to the prescription requirement. Three findings stand out. First, the reform accelerated the decline in atrazine use, generating both an immediate reduction and a steeper post-reform decline in sales. Second, for neonicotinoids, we find no evidence of a reform-induced decline once pre-existing trends are taken into account. Third, diamide sales increased substantially following the reform, consistent with a reallocation of demand toward this class of insecticides.

Figure I provides a visual summary of these results by plotting observed and fitted mean log sales for each treated class alongside its corresponding control group.[13] For neonicotinoids, diamides, and

[13]The choice between the TWFE and slope-break specifications reflects the pre-reform dynamics of each treatment panel. When treated and control compounds exhibit differential trends before the reform, the TWFE estimator may confound these underlying dynamics with the policy effect. In such cases, the slope-break specification provides a more appropriate characterization of the reform's impact. For sulfoximines and butenolides, which were still in the early stages of market adoption, the

atrazine, the short-dashed lines show the fitted trajectories implied by the slope-break specification. For sulfoximines and butenolides, the fitted trajectory is based on the pre-trend-adjusted specification in Equation (3.4), which controls for the differential pre-2018 growth path before estimating $\hat{\delta}_1$ and $\hat{\delta}_2$.

The figure reveals clear heterogeneity across compound classes. In the atrazine panel (Figure Ia), the treated series remains close to a stable plateau of approximately 12 log points over 2012–2017, compared with about 9 log points for the control group. Following the reform, treated sales fall sharply and continue to decline, nearly converging toward the control level by 2020; the fitted trajectory captures this steep and sustained post-reform decline. In the neonicotinoid class (Figure Ib), the treated and control series move largely in parallel before 2018, with treated sales declining gradually from their 2013 peak. After the reform, both series continue to drift downward together, and the fitted trajectory remains close to the pre-reform path, providing little visual evidence of any reform-induced break. The diamide class (Figure Ic) shows the opposite pattern. Treated sales start well below the control level in 2012 but grow rapidly through 2017, with growth accelerating after 2018 and the treated series crossing above the control group by 2021. The widening gap is closely tracked by the fitted trajectory and is consistent with increased diamide sales following the reform. Finally, in the sulfoximine and butenolide class (Figure Id), the treated compounds exhibit a steep pre-reform diffusion trend. Although sales continue to increase after 2017, they flatten relative to the pre-trend-adjusted projection, indicating a deceleration in adoption rather than an outright decline.

Table 1: Effects of the 2018 Prescription Requirement on Pesticide Sales

	Atrazine		Neonicotinoids		Sulfoximines	Diamides	
	(1) TWFE	(2) Slope-break	(3) TWFE	(4) Slope-break	(5) Slope-break	(6) TWFE	(7) Slope-break
$\hat{\beta}$ (TWFE level)	-2.545^{***}		-1.073^{*}			5.995^{**}	
	(0.513)		(0.511)			(2.041)	
$\hat{\delta}_1$ (slope-break level)		-1.771^{***}		-1.172^{**}	-0.830		4.058^{*}
		(0.432)		(0.457)	(3.047)		(2.164)
$\hat{\delta}_2$ (slope, per year)		-0.387^{***}		0.049	-1.370^{***}		0.968
		(0.110)		(0.085)	(0.199)		(0.728)
$F(\delta_1, \delta_2)$		12.553		3.825	63.608		4.416
p-value [F]		0.002		0.068	0.000		0.046
Bootstrap p-value	—[a]	—[a]	0.082	0.062	0.000	0.033	0.132
N (compound × year)	110	110	99	99	66	110	110

Notes: This table presents difference-in-differences estimates of the effect of the 2018 prescription requirement on log annual sales (kg of active ingredient) for the four treated compound classes. Columns (1), (3), and (6) report the two-way fixed-effects level effect $\hat{\beta}$ from Equation (3.1); columns (2), (4), (5), and (7) report the slope-break decomposition ($\hat{\delta}_1$ = discrete level shift at 2018; $\hat{\delta}_2$ = post-reform slope change) from Equation (3.3). Standard errors clustered at the compound level are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$. Bootstrap p-values are from wild cluster bootstrap with 999 replications and Rademacher weights. Significance stars reflect conventional cluster-robust standard errors; given the small number of clusters (Section 3.5), the bootstrap p-value row is the primary basis for inference where the two diverge, and is what Section 4.2.2's discussion of the neonicotinoid results relies on. [a] Bootstrap p-value not reported for atrazine: the panel contains a single treated cluster (atrazine enters as one aggregated compound), a case in which the wild cluster bootstrap is unreliable. Inference rests on the joint F-test ($F = 12.55$, $p = 0.003$) and the synthetic-control permutation ($p_{\text{perm}} = 0.100$) shown in Table B.1.

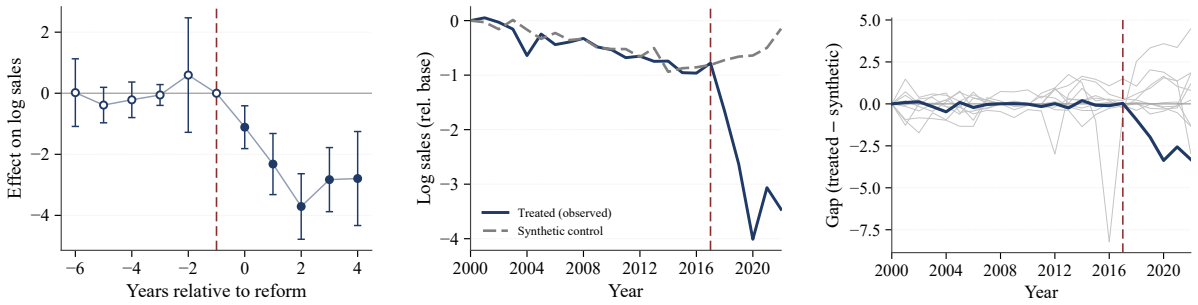
parallel-trends assumption underlying the TWFE specification is clearly violated (see Figure Id); we therefore report only the pre-trend-adjusted slope-break estimates from Equation (3.4).

4.2 Results by Compound Class and Robustness Checks

We now examine each compound class in turn. Each subsection follows a common structure. We first discuss the corresponding estimates reported in Table 1, then assess identification using the event-study and synthetic-control evidence, and finally interpret the magnitude and pattern of the estimated response.

4.2.1 Atrazine

The prescription requirement produced a large and persistent reduction in atrazine sales, with evidence of both an immediate level shift and a subsequent change in the sales trajectory. As shown in Table 1, the slope-break DiD estimate implies an immediate decline of $\hat{\delta}_1 = -1.771$ log points relative to the control group, corresponding to a reduction of approximately 83 percent relative to the pre-2018 level.[14] The initial decline was followed by an additional reduction of $\hat{\delta}_2 = -0.387$ log points per year relative to the pre-reform trajectory. Thus, the reform did not simply produce a one-time contraction in atrazine sales; the gap continued to widen after 2018. The joint null hypothesis $\hat{\delta}_1 = \hat{\delta}_2 = 0$ is rejected at the 1 percent level ($p = 0.002$), providing evidence of both a discrete break at the time of the reform and a sustained departure from the pre-reform trajectory. Averaged over the post-reform period, the estimated gap corresponds to a reduction of approximately 92 percent relative to the counterfactual path.



(a) Event study **(b)** Actual vs. synthetic atrazine **(c)** Placebo gaps

FIGURE II: Atrazine: three independent pieces of evidence.

Notes: This figure presents three independent pieces of evidence for the atrazine result. Panel (a) plots the event-study coefficients from Equation (3.2); the reference year is 2017, and the vertical dashed line marks that boundary so that 2018 onward is post-reform. Panel (b) plots observed atrazine log sales against its synthetic counterpart (normalised to 2000); the synthetic series is constructed as described in Section 3.4. Panel (c) plots the treated minus synthetic gap (navy) against the placebo-donor gaps (grey); the treated gap leaves the placebo cloud only after the reform. Vertical dashed lines mark the prescription boundary (2017).

The event-study estimates provide a direct assessment of the timing of this break. Figure IIa shows that the estimated pre-treatment coefficients are small and jointly insignificant, indicating no detectable differential trend between atrazine and the control group before the reform. This pattern is important because the identifying variation in the slope-break specification comes from the change in the trajectory at the time of the policy. Following the reform, the estimated treatment effect becomes sharply negative in 2018 and grows increasingly negative in subsequent years. The post-reform coefficients therefore trace a pattern that is difficult to reconcile with a continuation of the pre-2018 decline alone: the policy

[14] Log-point coefficients are converted to proportional changes as $\exp(\hat{\delta}) - 1$. The immediate decline is $\exp(-1.771) - 1 \approx -0.83$. The total post-reform effect in year t is $\hat{\delta}_1 + \hat{\delta}_2(t - 2018)$. Averaged over 2018–2022, this effect is -2.545 log points, implying a reduction of $1 - \exp(-2.545) \approx 0.92$, or approximately 92 percent relative to the counterfactual path.

is associated with an immediate downward shift followed by a persistent widening of the gap.

The synthetic-control results provide a second assessment of the counterfactual trajectory without relying on the common-trend structure of the DiD specification. As shown in Figure IIb, observed atrazine sales closely track the synthetic control throughout the pre-reform period, indicating a strong pre-treatment fit. From 2018, however, the two series diverge sharply, with observed atrazine sales falling substantially below the synthetic trajectory. The timing of this divergence closely coincides with the introduction of the prescription requirement, while the pre-treatment correspondence provides little indication of a comparable gap emerging in the absence of the reform.

The permutation analysis further indicates that this divergence is unusually large relative to the distribution of placebo effects. Figure IIc compares the treated-minus-synthetic gap for atrazine with the corresponding gaps obtained by assigning the treatment to each donor compound. Before the reform, the atrazine gap lies within the placebo distribution; after 2018, it falls well below the placebo gaps and remains separated from them throughout the post-treatment period. The post-to-pre RMSPE ratio of 16.0 is by far the largest in the placebo distribution—atrazine is the single most extreme of the ten herbicide units—though with so few donors the permutation p-value attains only its floor of $1/10 = 0.100$ (Table B.1). The synthetic-control results therefore provide evidence that the post-2017 divergence is unlikely to reflect the type of idiosyncratic movement observed among untreated compounds.

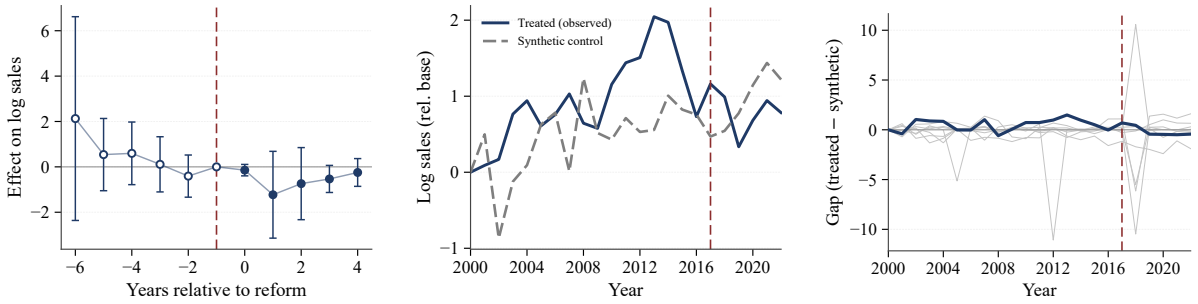
Taken together, the slope-break DiD, event-study, and synthetic-control estimates point to the same conclusion: the 2018 prescription requirement generated a large immediate reduction in atrazine sales that persisted and intensified over the subsequent years.

4.2.2 Neonicotinoids

Neonicotinoids were among the principal targets of public concern surrounding the 2018 reform. The baseline two-way fixed-effects specification suggests that the reform reduced neonicotinoid sales, with an estimated effect of -1.073 log points (Table 1, column 3). This estimate would imply a sizable reduction in sales following the introduction of mandatory agronomic prescriptions. However, the interpretation of the TWFE estimate is sensitive to the identifying assumption that, absent the reform, treated and control compounds would have followed parallel trends. We therefore turn to specifications that explicitly account for the pre-reform trajectory.

Once the pre-existing trend is incorporated, the evidence for a policy-induced reduction largely disappears. The slope-break specification does not yield a statistically significant post-reform change in either the level or the trajectory of neonicotinoid sales. The event-study estimates in Figure IIIa tell the same story: the coefficients show no clear discontinuity at the time of the reform and no sustained divergence thereafter, while the pre-treatment estimates provide no indication of a distinct break that would predict the subsequent pattern. Thus, the decline captured by the TWFE specification appears to reflect, at least in part, the continuation of a downward trajectory that was already underway before 2018 rather than a discrete response to the prescription requirement.

The synthetic-control analysis provides a further check on this interpretation. The synthetic series tracks observed neonicotinoid sales closely before the reform and continues to follow the treated series from 2018, with no pronounced post-reform divergence. In contrast to atrazine, therefore, the reform does not generate a gap that is large relative to the variation observed among the donor compounds. Taken together, the slope-break, event-study, and synthetic-control estimates do not support any reform-



(a) Event study **(b)** Actual vs. synthetic **(c)** Placebo gaps

FIGURE III: Neonicotinoids: event-study and synthetic-control evidence.

Notes: Event-study and synthetic-control evidence for the neonicotinoid aggregate. Panel (a) plots the event-study coefficients from Equation (3.2); Panel (b) plots the observed neonicotinoid aggregate against its synthetic counterpart (normalised to 2000). Panel (c) plots the treated minus synthetic gap (navy) against the placebo-donor gaps (grey); the treated gap stays within the placebo cloud throughout, consistent with a null.

induced reduction in neonicotinoid sales. The contrast with the TWFE estimate is informative: allowing the treated and control series to have different pre-reform trajectories substantially changes the estimated effect, indicating that the negative TWFE coefficient should not be interpreted as evidence of a causal policy effect.

One possible interpretation of this result is that the reform did not substantially alter an input decision that was already well established in Québec crop production. By 2018, neonicotinoid use was widespread in major field crops, and the prescription requirement may therefore have added an administrative step without materially changing established production practices. The absence of a statistically detectable and sustained decline is consistent with this interpretation, although the aggregate sales data do not allow us to directly test the underlying behavioral mechanism.

The way neonicotinoids enter production provides a further explanation for the absence of a detectable decline in measured sales. Unlike products such as atrazine that are typically purchased and applied directly by producers, neonicotinoids are widely delivered through treated seed. Health Canada identifies clothianidin, imidacloprid, and thiamethoxam as active ingredients used in treated corn and soybean seed, while Québec field studies document widespread use of neonicotinoid seed treatments in corn and soybean production before the reform (Pest Management Regulatory Agency, 2016; Labrie et al., 2020). This distinction matters because a prescription requirement applied to pesticide purchases directly affects separately purchased products but only indirectly affects insecticidal seed treatments. Thus, even if the reform affected separately purchased neonicotinoids at the margin, a substantial component of neonicotinoid use remained outside the principal regulatory margin.[15]

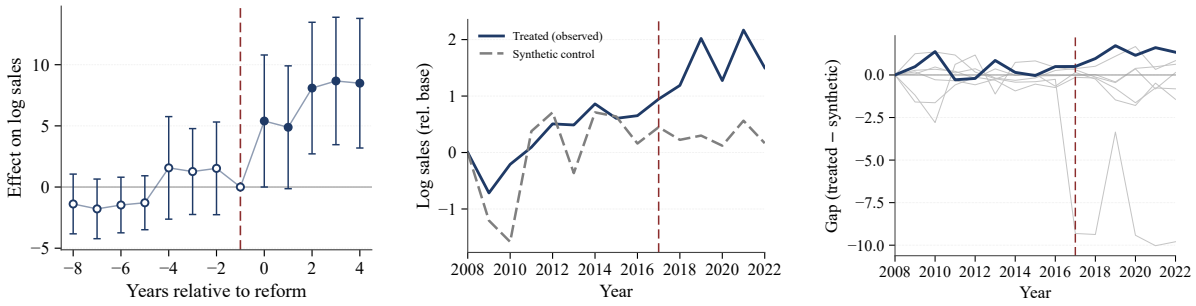
Overall, the evidence suggests that Québec's 2018 agronomic prescription reform did not produce a reduction in measured neonicotinoid sales. This result should not be interpreted as evidence that the reform had no effect on neonicotinoid use more broadly. Rather, the combination of established produc-

[15]Farm-level purchase records from the Québec Farm Production Cost Survey provide additional descriptive evidence consistent with this interpretation. The survey tracks pesticide transactions for a panel of Québec reference farms in 2009 and 2014. Among 461 distinct product entries in 2009 and 677 in 2014, fewer than two transactions involve clothianidin or imidacloprid purchased as standalone insecticides. By comparison, glyphosate (225 entries, 30,407 L in 2009) and atrazine (146 entries, 14,597 L) account for a substantial share of recorded purchases. The near absence of neonicotinoids from these purchase records is consistent with their widespread use through treated seed rather than separately purchased insecticides.

tion practices and the importance of treated seed limits the extent to which a prescription requirement targeting pesticide purchases would be expected to translate into a measurable reduction in aggregate sales.

4.2.3 Diamides

The baseline TWFE estimate in column 6 of Table 1 indicates a statistically significant increase in diamide sales of approximately 6.0 log points relative to the control group. The result is robust to wild cluster bootstrap inference. The slope-break estimate in column 7 yields a similar effect, with no significant additional change in the post-reform trajectory.[16]



(a) Event study **(b)** Actual vs. synthetic **(c)** Placebo gaps

FIGURE IV: Diamides: evidence of post-2017 substitution.

Notes: Event-study and synthetic-control evidence for the diamide aggregate. Panel (a) plots the event-study coefficients from Equation (3.2); the reference is 2017, marked by the vertical dashed line so that 2018 onward is post-reform. Panel (b) plots the observed diamide aggregate against its synthetic counterpart (normalised to 2008). Panel (c) plots the treated minus synthetic gap (navy) against the placebo-donor gaps (grey); the treated gap rises above the cloud after the reform.

The event-study estimates in Figure IVa provide further support for this pattern. Moreover, the synthetic-control analysis offers a complementary perspective (Figure IVb). The synthetic series tracks observed diamide sales reasonably well during the pre-reform period, although the pre-treatment fit is less close than for atrazine. The pre-period RMSPE is 0.75, compared with 0.16 for atrazine, reflecting the smaller and noisier donor pool available for diamides. Beginning in 2018, observed sales rise above the synthetic counterpart. The permutation exercise provides less precise evidence of this divergence (Figure IVc): the post-to-pre RMSPE ratio is 2.50, below four of the six placebo ratios, yielding a permutation p-value of 0.833 (Table B.1). The limited number of close biological comparators also constrains the available donor pool.[17]

Taken together, the results indicate an increase in diamide sales following the reform, rather than a continuation of the pre-reform trajectory. Before the reform (2012–2017), annual diamide sales averaged approximately 2,511 kg; afterward (2018–2022), they averaged approximately 6,948 kg (Table B.2).

[16] A potential concern is that pyrethroids, which are included among the control compounds for diamides, may themselves have been affected by the reform through substitution toward diamides. Pyrethroids and diamides target different biological pathways but are both widely used against insect pests, making substitution between the two classes plausible. Such reallocation could overstate the estimated diamide effect. We therefore test whether pyrethroid sales changed relative to organophosphates using the same difference-in-differences framework. Table B.4 in Appendix B shows a small and statistically insignificant post-reform effect, while Figure B.1 shows event-study coefficients close to zero before and after 2018. These results provide no evidence that pyrethroids were materially affected by the reform, supporting their use as controls.

[17] Only two ecdysone-agonist compounds and one chitin-synthesis inhibitor are available as close biological comparators.

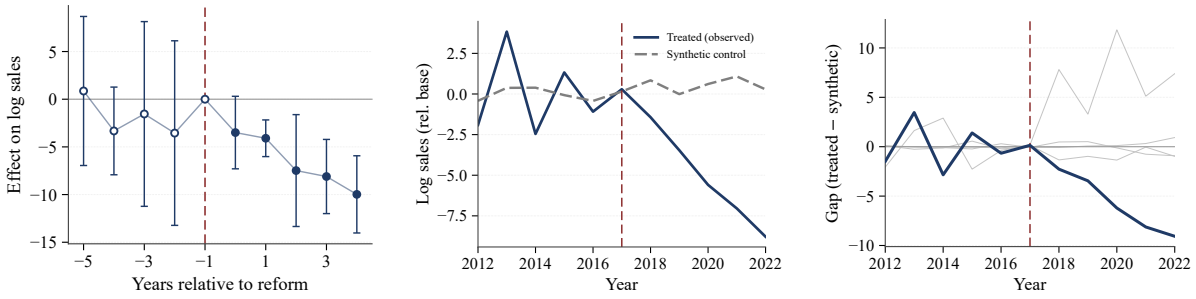
Diamides provide an alternative for controlling many of the same pests as neonicotinoids, and Kathage et al. (2018) document a similar pattern in Europe, where farmers facing restrictions on neonicotinoids reallocated toward alternative insecticide chemistries.

This reallocation is not simply diamides absorbing a realized decline in neonicotinoid purchases: as Section 4.2.2 shows, the negative association between the reform and neonicotinoid sales does not survive once we account for their pre-existing downward trend. Section 4.2.4 offers a more direct account of the diamide expansion, tied to the regulatory trajectory facing the broader nicotinic-receptor-active family rather than to a change in neonicotinoid purchases specifically.

4.2.4 Sulfoximines and Butenolides

The active ingredients in sulfoximines and butenolides were still in the diffusion phase of their product life cycle before 2018. A conventional difference-in-differences specification is therefore less suitable because the treated compounds were following a steep upward adoption path rather than a stable pre-reform trajectory. The relevant question is whether the prescription requirement changed the rate at which these compounds were entering the market relative to their pre-existing growth path. The evidence suggests that it did. Column 5 of Table 1 shows that, after controlling for the compound-specific pre-treatment trend, the estimated post-reform slope change is negative, $\hat{\delta}_2 = -1.370$ ($p = 0.001$). The estimate remains significant under wild cluster bootstrap inference, indicating a marked deceleration in market adoption following the reform.

The event-study estimates in Figure Va provide a similar picture. Because these compounds were still diffusing before 2018, the event-study coefficients measure deviations from their joint pre-reform trend rather than changes relative to a fixed pre-treatment level. Coefficients close to zero before the reform indicate that adoption remained close to this underlying trajectory, while the negative post-reform coefficients indicate a departure from it.



(a) Event study **(b)** Actual vs. synthetic **(c)** Placebo gaps

FIGURE V: Sulfoximines and butenolides: event-study and synthetic-control evidence.

Notes: Panel (a): event-study coefficients from the slope-break specification with the compound-specific pre-trend absorbed; coefficients measure deviation from the sulfoxaflor/flupyradifurone joint pre-2018 growth trajectory, not from a level baseline, and negative post-2017 values indicate adoption deceleration. Because these compounds enter the market essentially at the reform, the synthetic control in panels (b)–(c) is run on the pre-trend-adjusted outcome (each unit's deviation from its own pre-2018 linear trend, the same adjustment as panel (a)) rather than on levels.

The synthetic-control analysis provides a further check (Figures Vb and Vc). A standard synthetic control based on raw log sales does not reproduce the steep pre-2018 adoption path of sulfoximines and butenolides. We therefore detrend each unit before matching, which yields an acceptable pre-treatment

fit (RMSPE = 2.03, Table B.1). Following the reform, the treated series shows a smooth and sustained decline relative to its detrended synthetic counterpart. The corresponding gap in Figure Vc widens after 2018, while the placebo gaps display more irregular movements, providing additional descriptive evidence of a post-reform slowdown in adoption.

Taken together, the slope-break estimate, its robustness to wild cluster bootstrap inference, and the event-study pattern point to a deceleration in sulfoximine and butenolide adoption beginning in 2018. The synthetic-control analysis provides additional visual support for this interpretation.

One notable feature of this result is that sulfoxaflor and flupyradifurone act on nicotinic acetylcholine receptors and provide control over pest complexes that overlap with those targeted by neonicotinoids, making them plausible alternatives as producers adjusted to the new prescription requirement. Their adoption nevertheless slowed after 2018.

The contrasting patterns for sulfoximines and butenolides and for diamides are informative when considered together. Diamide sales increased following the reform, whereas the adoption of sulfoximines and butenolides slowed relative to their pre-reform trajectories. The two groups differ in their modes of action and pest spectra. Sulfoxaflor and flupyradifurone act on nicotinic acetylcholine receptors and therefore overlap more closely with neonicotinoids, whereas diamides act on a different biological target and are particularly effective against chewing pests such as Lepidoptera and Coleoptera. The post-reform pattern is therefore not consistent with a simple replacement of neonicotinoids by insecticides with similar modes of action. Instead, the simultaneous expansion of diamides and slowdown in sulfoximine and butenolide adoption indicate that the adjustment in insecticide sales extended beyond pharmacologically similar alternatives.

A unifying explanation is that producers and agronomists reallocated away from the entire nicotinic-receptor-active family, not just the compounds the 2018 requirement named. Neonicotinoids (IRAC 4A), sulfoximines (4C), and butenolides (4D) share a mode of action and, evidently, a regulatory trajectory; diamides (IRAC 28) share neither. Once the family's flagship compounds were brought under prescription, producers and agronomists had reason to expect the rest of the group would eventually follow too—an expectation the 2025 extension to sulfoximines and butenolides later confirmed. Diamides offered a way to diversify insecticide use without that overhang. This account does not require a corresponding decline in realized neonicotinoid sales: it requires only that the 2018 reform was read as a signal about the regulatory trajectory of the whole family.

5 Conclusion

This paper examines whether mandatory agronomic prescriptions reduce pesticide use and how producers adjust their pesticide choices when access to selected compounds becomes subject to professional authorization. Exploiting Québec's 2018 prescription reform, we combine difference-in-differences, slope-break, and synthetic control methods to identify the effects of the policy on targeted pesticides and alternative insecticide classes. The results show that prescription-based regulation can substantially alter pesticide use, but its effects vary across compounds and do not necessarily lead to substitution toward the closest chemical alternatives.

The reform had its clearest effect on atrazine, whose sales declined substantially relative to the counterfactual trajectory, suggesting that pre-reform use exceeded agronomically justified levels and

that the prescription requirement reduced applications that may not have been agronomically necessary. Neonicotinoids, in contrast, show no reform-induced decline once we account for their pre-existing downward trend, despite being a primary target of the reform. The contrasting responses suggest that the effectiveness of prescription requirements depends on the decision margin through which the regulation operates and on the extent to which producers can adjust their pesticide choices outside that margin.

The responses of alternative insecticides further qualify the interpretation of the reform. Diamide sales increased substantially following the policy, while the adoption of sulfoximines and butenolides—chemistries pharmacologically closer to neonicotinoids—slowed relative to their pre-reform trajectories. This is the opposite of what a simple substitution story would predict, in which producers replace a regulated compound with its closest available alternative. A more consistent account is that producers and agronomists reallocated away from the entire nicotinic-receptor-active family, anticipating its regulatory trajectory—an anticipation later confirmed when sulfoximines and butenolides were themselves brought under prescription in 2025. Diamides, sharing no mode-of-action lineage with the regulated class, absorbed that reallocation.

Taken together, these findings demonstrate that the effects of pesticide regulation cannot be assessed solely from the response of the compounds directly targeted by the policy. Mandatory prescriptions can accelerate the decline of individual pesticides, but they may also induce broader changes in the composition and trajectory of pesticide use. The nature of these responses depends on the agronomic role, market position, and adoption dynamics of alternative compounds. Accounting for such cross-compound responses is therefore essential for determining whether access-based pesticide regulation reduces overall pesticide use and, ultimately, pesticide-related environmental risk.

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A Data Appendix

Tables A.1–A.4 report the treated and control compounds for each panel, along with IRAC/HRAC mode-of-action group.

Table A.1: Atrazine Panel: Treated and Control Compounds

Compound	Pesticide Mode of Action Group	Role
atrazine	HRAC 5	Treated
MCPA (aggregated)	HRAC 4	Control 1
2,4-D (aggregated)	HRAC 4	Control 1
bromoxynil	HRAC 6	Control 1
bentazone	HRAC 6	Control 2
clethodim	HRAC 1	Control 2
sethoxydim	HRAC 1	Control 2
fenoxaprop-P-ethyl	HRAC 1	Control 2
dicamba (aggregated)	HRAC 4	Control 3
pendimethalin	HRAC 3	Control 3

Notes: HRAC = Herbicide Resistance Action Committee mode-of-action classification. MCPA, 2,4-D, and dicamba are aggregated across commercial formulations (regex match on the active-ingredient name). Control definitions: Control 1 = selective broadleaf herbicides; Control 2 = graminicides; Control 3 = synthetic auxin (dicamba) and dinitroaniline (pendimethalin). *Glyphosate* is excluded because its post-2000 trajectory is dominated by the diffusion of glyphosate-tolerant GM crops, an independent shock orthogonal to the prescription requirement. *Metolachlor* and *S-metolachlor* are excluded as agronomic complements to atrazine in corn weed management, generating mechanical co-movement. *Carfentrazone-ethyl* is excluded because it has no recorded Québec sales over 2000–2022 in the MELCCFP dataset.

Table A.2: Neonicotinoid Panel: Treated and Control Compounds

Compound	Pesticide Mode of Action Group	Role
clothianidin	IRAC 4A	Treated
imidacloprid	IRAC 4A	Treated
thiamethoxam	IRAC 4A	Treated
lambda-cyhalothrin	IRAC 3A	Control 1
cypermethrin	IRAC 3A	Control 1
deltamethrin	IRAC 3A	Control 1
bifenthrin	IRAC 3A	Control 1
permethrin	IRAC 3A	Control 1
tefluthrin	IRAC 3A	Excluded
malathion	IRAC 1B	Control 2
phosmet	IRAC 1B	Control 2

Notes: IRAC = Insecticide Resistance Action Committee mode-of-action classification. Control 1 = pyrethroids (same crop and target pest spectrum); Control 2 = organophosphates (broader spectrum alternative). Treatment restricted to the three IRAC 4A compounds under the prescription requirement as of September 8, 2018 (clothianidin, imidacloprid, thiamethoxam). Acetamiprid and thiacloprid are not subject to the prescription requirement. Tefluthrin is excluded from controls: as a seed-treatment pyrethroid, it is a direct substitute for neonicotinoid seed coatings and would contaminate the control group.

Table A.3: Sulfoximine/Butenolide Panel: Treated and Control Compounds

Compound	Pesticide Mode of Action Group	Role
sulfoxaflor	IRAC 4C	Treated
flupyradifurone	IRAC 4D	Treated
pymetrozine	IRAC 9	Control 1
flonicamid	IRAC 9	Control 1
lambda-cyhalothrin	IRAC 3A	Control 2
cypermethrin	IRAC 3A	Control 2
bifenthrin	IRAC 3A	Control 2

Notes: Control 1 = aphicides with distinct chemical scaffolds (selective insecticides targeting same pest guild); Control 2 = pyrethroids (broad-spectrum insecticides). Pirimicarb (IRAC 1A carbamate aphicide) is excluded: no recorded Québec sales over the panel window 2012–2022.

Table A.4: Diamide Panel: Treated and Control Compounds

Compound	Pesticide Mode of Action Group	Role
chlorantraniliprole	IRAC 28	Treated
cyantraniliprole	IRAC 28	Treated
tetraniliprole	IRAC 28	Treated
cyclaniliprole	IRAC 28	Treated
methoxyfenozide	IRAC 18	Control 1
tebufenozide	IRAC 18	Control 1
novaluron	IRAC 15	Control 2
deltamethrin	IRAC 3A	Control 3
cypermethrin	IRAC 3A	Control 3
lambda-cyhalothrin	IRAC 3A	Control 3

Notes: Control 1 = diacylhydrazines (IRAC 18, ecdysone agonists, same lepidopteran target as diamides); Control 2 = benzoylureas (IRAC 15, chitin synthesis inhibitors); Control 3 = pyrethroids (broad-spectrum insecticides, similar crop use).

B Output Appendix

B.1 Robustness: All Specifications

Table B.1 reports the companion synthetic-control permutation tests for all four classes.

Table B.1: Synthetic-Control Permutation Tests

	pre-2018 RMSPE	RMSPE ratio	p_{perm} (donors)
Atrazine	0.162	16.007	0.100 (1 of 10)
Neonicotinoids	0.744	0.510	1.000 (7 of 7)
Diamides	0.754	2.498	0.833 (5 of 6)
Sulfoximines/butenolides (detrended)	2.028	3.148	0.600 (3 of 5)

Notes: This Table shows the outputs from the synthetic control tests. Permutation p-values are based on the post/pre RMSPE ratio; pre-2018 RMSPE reports the pre-reform matching fit (smaller = tighter), and the count in parentheses is the number of units (treated + donors) whose ratio is at least the treated unit's. The sulfoximine synthetic control is estimated on detrended log sales.

B.2 Class-Level Aggregation

The compound-level panel in Table 1 treats each active ingredient as a separate observation. To summarise each class in a way that removes the within-class new-entry log artifact (Section 4.2.3), we collapse each class to a single summed series and report the change in total class sales in levels—the economically interpretable magnitude when a class enters from near zero. Table B.2 reports these total-sales changes: atrazine and neonicotinoids contract (-84.1% and -54.4%), while diamides and the sulfoximine/butenolide class expand ($+177\%$ and $+463\%$). The sulfoximine/butenolide figure is inflated by a near-zero pre-2014 base. These compounds were only entering the Québec market at the start of the window, so its magnitude should be read with caution. The direction is nonetheless informative.

We deliberately do not estimate a difference-in-differences regression on the collapsed series: with a single treated unit, cluster-robust standard errors and the wild cluster bootstrap are not reliable, so such a regression cannot support credible inference. The aggregate estimate therefore provides a descriptive measure of the magnitude, while causal inference relies on the event-study and synthetic-control tests.

Table B.2: Class-level total-sales changes (descriptive)

	Atrazine	Neonicotinoids	Diamides	Sulfoximines/butenolides
Total class sales (mean annual kg a.i.)				
Pre (2012–17)	135,558	7,750	2,511	190
Post (2018–22)	21,583	3,531	6,948	1,067
Change (%)	-84.1	-54.4	176.7	463.2

Notes: Each class is collapsed to a single series equal to the log of total annual sales summed over its compounds. "Total class sales" rows report mean annual volume (kg active ingredient) before (2012–2017) and after (2018–2022) the reform and the percentage change. Because each class collapses to a single treated series, these volume changes are reported descriptively; we do not estimate a difference-in-differences regression on the aggregate, since one treated unit cannot support credible clustered inference. Inference is drawn from the compound-level panel and the synthetic control.

B.3 Control Group Falsification Tests

Table B.3 tests whether the control compounds themselves shifted post-2017, which would violate the exclusion restriction. For the atrazine panel, Control-1 herbicide controls (MCPA, 2,4-D, bromoxynil) are treated as pseudo-treated and Control-2 controls (bentazone, clethodim, sethoxydim, fenoxaprop) serve as the comparison. For the neonicotinoid and sulfoximine panels, pyrethroids are pseudo-treated against organophosphates. A null result in both rows indicates that within-control-group trajectories were parallel from 2018. The reform did not selectively displace one tier of controls relative to another, supporting the validity of the joint control pool.

Table B.3: Control Group Falsification Tests

Panel	Pseudo-treated vs. comparison	$\hat{\beta}$	(s.e.)
Atrazine	Control-1 controls vs. Control-2 controls	-1.056	(0.919)
Neonicotinoids/sulfoximines	Pyrethroids vs. organophosphates	0.309	(0.590)
Compound FE	✓		
Year FE	✓		

Notes: Each row treats one subset of the baseline control group as pseudo-treated and uses the remaining controls as the comparison group. Outcome: log annual sales (kg a.i.). Post $= \mathbf{1}_{\{t \geq 2018\}}$. A null result indicates the two control subgroups followed parallel trajectories from 2018, supporting their joint use as controls in the main DiD. *Atrazine*: Control-1 = MCPA, 2,4-D, bromoxynil; Control-2 = bentazone, clethodim, sethoxydim, fenoxaprop. *Neonicotinoids/sulfoximines*: pyrethroids (IRAC 3A) vs. organophosphates (IRAC 1B). Standard errors clustered by compound. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

B.4 Pyrethroid Falsification Test

Table B.4 and Figure B.1 present the pyrethroid falsification test. The test treats pyrethroids as a pseudo-treated group and uses organophosphates (malathion, phosmet) as the control group, estimating a TWFE DiD for the post-2017 period. If pyrethroids were themselves displaced from field applications as a result of the reform, thereby contaminating the diamide DiD, their sales should diverge from those of organophosphates after 2017.

Table B.4: Pyrethroid Falsification Test: Did Pyrethroid Sales Change Post-2018?

Pseudo-treated	Estimate	(s.e.)
Pyrethroids vs. organophosphates	0.309	(0.590)
Bootstrap p-value	0.531	
Compound FE	✓	
Year FE	✓	
N (compound × year)	138	

Notes: Pseudo-treated group is pyrethroids (IRAC 3A: lambda-cyhalothrin, cypermethrin, deltamethrin, permethrin). Bifenthrin excluded (zero recorded sales in every year 2014-2022 except a single 2018 entry). Control group is organophosphates (IRAC 1B: malathion, phosmet). Outcome: log annual sales (kg a.i.). A null result indicates pyrethroids followed the same post-2018 trajectory as organophosphates, supporting their use as controls in the diamide panel. Post $= \mathbf{1}_{\{t \geq 2018\}}$. Standard errors clustered by compound. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

The estimate is $\hat{\beta} = 0.678$ (s.e. $= 0.599$, wild-bootstrap $p = 0.312$). The event-study coefficients in Figure B.1 are flat throughout both the pre- and post-reform periods: no pre-trend violation, and no post-2017 divergence. Pyrethroid sales tracked organophosphate trends before and after the reform. The contamination concern is theoretically coherent but empirically absent.

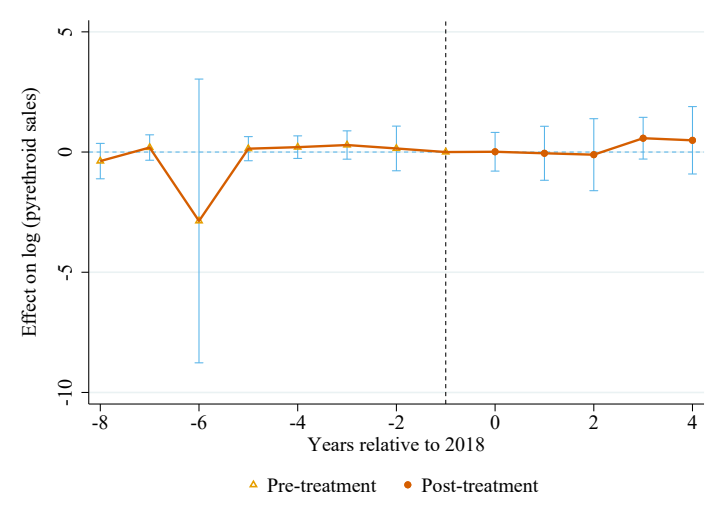

Figure B.1: Event-study: pyrethroids vs. organophosphates (falsification test), 2005–2022.

Notes: Pyrethroids are the pseudo-treated group; organophosphates (malathion, phosmet) are the control. Each coefficient is the deviation in pyrethroid log sales relative to the organophosphate trend in year t, normalized to the pre-reform mean. The vertical dashed line marks 2018. A null post-2017 path confirms pyrethroids are valid controls for diamides. Wild-cluster bootstrap p-values used throughout. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.