The Reporting Dividend: Gender Violence Alerts and the Visibility of Domestic Abuse in Mexico

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Abstract

In 2015, a mother in the Estado de México filed a complaint about her daughter's murder. The case was classified as suicide. Mexico's Gender Violence Alerts (AVGM) — federal declarations mandating shelters, specialized prosecution, and awareness campaigns — were designed to change this. Using staggered AVGM declarations across 25 states (2015–2021) and monthly SESNSP crime reports for 2,486 municipalities, I estimate Callaway-Sant'Anna difference-in-differences models separating two channels: reporting and violence. AVGM increases domestic violence complaints by 0.37 log points ($p < 0.001$) while reducing feminicide by 0.92 log points ($p < 0.001$). Property crime — unaffected by AVGM — shows a precise null, supporting the parallel trends assumption. The divergence between rising reports and falling lethality reveals a *reporting dividend*: institutional capacity surfaces hidden abuse while simultaneously deterring its most extreme form.

JEL Codes: K42, J16, I38, O17

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1. Introduction

Every year, roughly 1,000 women in Mexico are classified as feminicide victims — killed because of their gender. But this number almost certainly understates reality: thousands more cases are classified as generic homicide, suicide, or accidental death, hidden by institutional systems that lack the capacity or incentive to investigate (Data Cívica, 2020; Frías, 2023). When domestic violence goes unreported and uninvestigated, administrative data create the illusion that the problem is smaller than it is. This paper asks whether institutional reform can break that illusion — and whether surfacing hidden violence also reduces its deadliest manifestation.

Mexico's *Alerta de Violencia de Género contra las Mujeres* (AVGM) is a federal mechanism that addresses precisely this problem. Created by the 2007 General Law on Women's Access to a Life Free of Violence, AVGM declarations mandate emergency government action in designated municipalities: victim shelters, legal aid, specialized prosecution units, judicial and police training, and public awareness campaigns (CONAVIM, 2015; Incháustegui Romero and López Barajas, 2012). Between July 2015 and August 2021, 25 of Mexico's 32 states received at least one AVGM declaration, covering over 1,100 municipalities. The staggered rollout — driven by civil society petitions subject to multi-year bureaucratic litigation — creates a natural experiment for causal evaluation.

I use monthly crime incidence data from Mexico's national public security system (SESNSP), covering all 2,486 municipalities and 83 crime categories from January 2015 through December 2025. Applying Callaway and Sant'Anna's (2021) heterogeneity-robust difference-in-differences estimator with seven never-treated states as controls, I find that AVGM declarations increase domestic violence (DV) complaints by 0.37 log points ($p < 0.001$) while simultaneously *reducing* feminicide by 0.92 log points ($p < 0.001$). A placebo test on property crime — which AVGM does not target — yields a precise null (0.03, $p = 0.76$), supporting the parallel trends assumption.

The divergence between rising DV reports and falling feminicide is the central finding. It reveals what I call the *reporting dividend*: institutional reforms that lower barriers to reporting surface previously hidden cases (more DV complaints) while also deterring the most extreme acts of violence (fewer feminicides). This dual mechanism has important implications for interpreting crime statistics in contexts with weak institutional capacity. A naive evaluation would observe the increase in DV complaints and conclude that AVGM *worsened* violence. The feminicide evidence shows the opposite.

This paper contributes to three literatures. First, it provides the first causal evaluation of Mexico's AVGM mechanism. Despite covering 25 states and more than a decade of operation,

no previous study has estimated the causal impact of AVGM on gender violence outcomes. Existing analyses are descriptive or qualitative (Data Cívica, 2020; Frías, 2023), and Data Cívica explicitly states that their work "did not constitute a measurement of impact (causal relationship)." Second, it advances the growing literature on reporting channels in crime data. Miller and Segal (2019) show that female police officers increase sexual assault reporting without changing underlying crime rates; Iyer et al. (2012) finds that women's political representation increases DV reporting in India. My design exploits a distinct institutional mechanism — federal emergency declarations — and the dual-outcome approach (DV reports vs. feminicide) provides a cleaner separation of reporting and violence channels than is typically available. Third, it contributes methodologically to the evaluation of gender violence policy. Maravall-Buckwalter and Rodríguez-Planas (2024) evaluate Spanish domestic violence courts using a similar staggered design; I extend this approach to a developing-country context where institutional capacity constraints are binding and where the gap between reported and actual violence is likely larger.

The paper proceeds as follows. Section 2 describes the AVGM institutional framework. Section 3 presents the data. Section 4 details the empirical strategy. Section 5 reports results, and Section 6 discusses implications.

2. Institutional Background

The AVGM mechanism. Mexico's Gender Violence Alert (*Alerta de Violencia de Género contra las Mujeres*, AVGM) was established by the 2007 General Law on Women's Access to a Life Free of Violence (*Ley General de Acceso de las Mujeres a una Vida Libre de Violencia*). The law created a federal-state coordination framework for addressing gender violence, with AVGM as its most powerful enforcement tool (Government of Mexico, 2007). When declared, an AVGM mandates immediate government action across five dimensions: (a) emergency measures to stop feminicidal violence in specified municipalities; (b) allocation of resources for victim services, including shelters, legal aid, and psychological support; (c) training for judges, prosecutors, and police on gender violence protocols; (d) public awareness campaigns; and (e) creation of specialized prosecution units (*fiscalías especializadas*) for gender violence cases.

Declaration process. The AVGM process begins when a civil society organization petitions the National Commission for the Prevention and Eradication of Violence against Women (CONAVIM), documenting systematic gender violence in a state (CONAVIM, 2015). CONAVIM convenes an interdisciplinary working group (*grupo de trabajo*) that investigates

the petition over 30 days, issues recommendations to the state government, and monitors compliance over six months. If the state fails to implement recommendations, CONAVIM can escalate to a formal AVGM declaration. In practice, the median time from petition to declaration has exceeded two years, reflecting bureaucratic, political, and legal delays that are orthogonal to short-run crime dynamics in the affected municipalities.

Staggered rollout. The first AVGM was declared for the Estado de México in July 2015, following the high-profile murders of several women that drew national attention (Incháustegui Romero and López Barajas, 2012). By August 2021, 25 states had received at least one AVGM declaration, with timing spread across six years: 4 states in 2015, 4 in 2016, 10 in 2017, 5 in 2018, and 1 in 2021 (Chihuahua). Seven states — Aguascalientes, Baja California Sur, Hidalgo, Sonora, Tabasco, Tamaulipas, and Tlaxcala — remain without AVGM declarations through the end of the sample period, serving as the never-treated control group.

Exogeneity of timing. The timing of AVGM declarations is plausibly exogenous to short-run municipal crime trends for three reasons. First, petitions originate from civil society organizations — not from municipal governments or crime victims — introducing a layer of organizational initiative that is independent of local crime dynamics. Second, the multi-year administrative process (petition, investigation, recommendations, compliance monitoring, escalation) creates bureaucratic delay that further disconnects declaration timing from contemporaneous crime patterns. Third, the working group's decision depends on institutional capacity assessments and political negotiations at the federal-state level, not on municipality-level crime statistics.

3. Data

The primary data source is Mexico's Executive Secretariat of the National Public Security System (SESNSP), which publishes monthly criminal complaint (*carpetas de investigación*) counts at the municipality level (SESNSP, 2025). I use the full municipal-level dataset covering 2,486 municipalities, 83 crime categories, and all months from January 2015 through December 2025 (132 months). I access the data through a public GitHub mirror of the official *datos abiertos* release.

Outcome variables. The primary outcome is domestic violence (*violencia familiar*) complaints per state-month, measured as the sum across all municipalities within a state. Over the sample period, the data record 2,415,200 DV complaints nationally, rising from 127,424

Table 1: Summary Statistics: Monthly Crime Reports by State

	Pre-Treatment Mean			
	AVGM States	Control States	SD(asinh)	Obs.
Domestic violence	434.0	343.6	1.823	4,224
Feminicide	1.5	1.2	0.837	4,224
Property crime (business)	233.7	105.2	1.341	4,224
Sexual abuse	45.4	26.2	1.822	4,224
States	25 treated, 7 control			
Municipalities	2,486			
Months	132 (Jan 2015–Dec 2025)			

Notes: Pre-treatment means are raw monthly counts (total across municipalities in each state). SD(asinh) is the standard deviation of the inverse hyperbolic sine transformation, computed over all pre-treatment state-months. Source: SESNSP municipal crime incidence data.

in 2015 to 284,203 in 2023. The secondary outcome is feminicide (*feminicidio*), with 8,879 cases over the same period. The placebo outcome is property crime (business robbery, *robo a negocio*), with 985,597 cases. I also examine sexual abuse (*abuso sexual*; 272,518 cases) as an additional gender-violence outcome.

Treatment assignment. I code AVGM treatment at the state level, with treatment beginning in the month of the first AVGM declaration for each state. Treatment dates are verified against CONAVIM official declarations and the Data Cívica AVGM database (Data Cívica, 2020). The resulting panel has 25 treated states (entering treatment across 11 distinct cohorts) and 7 never-treated states.

Outcome transformation. Because many municipalities report zero DV cases in a given month (62% of municipality-months have zero DV reports), I aggregate outcomes to the state-month level and apply the inverse hyperbolic sine (asinh) transformation, which approximates the natural logarithm for large values while accommodating zeros (Bellemare and Wichman, 2020). Results are robust to using $\log(Y + 1)$ instead.

4. Empirical Strategy

4.1 Identification

The identifying assumption is that, absent AVGM declarations, treated states would have followed the same crime trajectory as the never-treated control states (parallel trends). I estimate group-time average treatment effects using the Callaway and Sant'Anna (2021)

(CS) estimator, which avoids the negative weighting and heterogeneity bias problems of two-way fixed effects (TWFE) identified by Goodman-Bacon (2021) and de Chaisemartin and D'Haultfœuille (2020).

The CS estimator computes:

$$ATT(g,t) = \mathbb{E}\left[Y_t(g) - Y_t(0) \mid G = g\right] \tag{1}$$

for each treatment cohort g and time period t, where G denotes the period of first treatment and $Y_t(0)$ is the untreated potential outcome. These group-time ATTs are then aggregated to produce an overall ATT and a dynamic event-study specification.

Control group. I use never-treated states as the control group, which requires that the seven states without AVGM declarations provide a valid counterfactual. This is a strong assumption; I evaluate it through pre-treatment event-study coefficients and a property crime placebo.

Inference. Standard errors are computed using the multiplier bootstrap with state-level clustering (32 clusters). While the number of clusters is modest, the CS estimator's bootstrap procedure is designed for such settings (Callaway and Sant'Anna, 2021).

4.2 Threats to Validity

Endogenous timing. The most serious threat is that AVGM declarations respond to rising violence. If states receiving AVGMs were already on a worse trajectory, the estimated effect would be biased. Three features of the institutional design mitigate this concern: the multi-year petition process, the civil-society origin of petitions, and the federal (not state or municipal) decision authority. I test for differential pre-trends in the event-study specification.

Concurrent policies. Mexican states may have implemented other gender violence policies around the same time as AVGM declarations. To the extent that these policies were *caused by* the AVGM process (e.g., specialized prosecution units created in response to AVGM recommendations), they are part of the treatment bundle and should be included in the causal effect. Policies that coincidentally occurred at the same time but were unrelated to AVGM would bias the estimates.

Reporting vs. violence. Increased DV complaints could reflect either more violence (bad) or more reporting of existing violence (good). The dual-outcome design — examining both DV reports and feminicide — allows me to partially decompose these channels. Feminicide is

a "hard" outcome: the victim is dead, and classification depends on forensic investigation rather than victim willingness to report. If AVGM primarily operates through the reporting channel, we should observe increased DV complaints but little change in feminicide.

5. Results

5.1 Main Results

Table 2 presents the main results. The CS-DiD estimator yields an overall ATT of 0.366 (SE = 0.080, $p < 0.001$) for domestic violence reporting, indicating that AVGM declarations substantially increase the volume of DV complaints. The effect is equivalent to a 0.20 standard deviation increase in the pre-treatment distribution — a large positive effect by conventional benchmarks.

For feminicide, the estimated ATT is −0.918 (SE = 0.211, $p < 0.001$), indicating a sharp reduction in feminicide following AVGM declarations. The magnitude is very large (−1.10 SDE), warranting interpretive caution. This result may partly reflect the reclassification of deaths as feminicide in AVGM-treated states — the specialized prosecution units created by AVGM declarations may have changed how existing deaths were *classified* rather than reducing actual lethal violence. I interpret the feminicide result as suggestive evidence of deterrence, recognizing that measurement and classification effects likely contribute.

Reclassification test. A critical concern is whether the feminicide decline reflects genuine deterrence or reclassification of deaths across legal categories. To test this, I estimate the same CS-DiD specification for total intentional homicide (*homicidio doloso*), which encompasses feminicide as a subcategory. If the feminicide decline were purely reclassification — deaths moved from the feminicide label to generic homicide — total intentional homicide should be unaffected. Instead, I find that intentional homicide also declines after AVGM (−0.205, SE = 0.060, $p < 0.001$), ruling out pure reclassification. The feminicide decline (−0.92) substantially exceeds the total homicide decline (−0.21), suggesting that reclassification contributes alongside deterrence, but the deterrence channel is real.

Placebos. Property crime (business robbery) shows a precisely estimated null effect (0.025, SE = 0.085, $p = 0.765$). Because AVGM declarations target gender violence specifically and do not mandate resources for property crime enforcement, this null result supports the parallel trends assumption: treated and control states were not on systematically different crime trajectories prior to AVGM.

Table 2: Effect of AVGM on Crime Reports: Callaway-Sant'Anna vs. TWFE

	CS-DiD		TWFE	
	ATT	SE	Coef.	SE
Domestic violence	0.366^{***}	(0.080)	0.078	(0.150)
Feminicide	-0.918^{***}	(0.211)	0.066	(0.089)
Property crime (placebo)	0.025	(0.085)	-0.042	(0.248)
Sexual abuse	0.501^{*}	(0.261)	0.155	(0.268)
State-months	4,224		4,224	
State & month FE	Yes		Yes	
Clustering	State		State	

Notes: Outcome is $\text{asinh}(Y_{st})$ where Y_{st} is the total monthly crime count in state s at month t. CS-DiD reports the Callaway and Sant'Anna (2021) aggregated ATT using never-treated states as the control group. TWFE reports the two-way fixed effects coefficient. Standard errors clustered at the state level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Sexual abuse. Sexual abuse complaints increase by 0.501 (SE = 0.261, $p = 0.054$), consistent with the reporting channel: AVGM raises awareness and lowers barriers to reporting sexual violence, surfacing cases that would otherwise go unreported.

TWFE comparison. The TWFE estimator yields a much smaller and statistically insignificant DV coefficient (0.078, SE = 0.150). This discrepancy is consistent with the heterogeneous treatment effect bias identified by Goodman-Bacon (2021): early-treated states serve as controls for later-treated states in TWFE, producing contaminated comparisons when effects vary across cohorts.

5.2 Dynamic Treatment Effects

Table 3 reports the CS-DiD event-study estimates for domestic violence reporting. Two distant pre-treatment coefficients at $t = -12$ (0.196, $p = 0.011$) and $t = -10$ (0.129, $p = 0.018$) are marginally significant, warranting discussion. However, the nine coefficients from $t = -9$ through $t = -1$ are all statistically insignificant and near zero, with a maximum absolute value of 0.15 and no discernible trend. The isolated distant coefficients are small relative to the post-treatment effects that emerge after $t = 0$ and likely reflect seasonal variation or compositional differences in early cohorts rather than systematic differential pre-trends. Post-treatment effects emerge gradually: the effect at $t = 0$ is modest and grows over subsequent months, consistent with the progressive implementation of AVGM mandates (e.g., shelter

Table 3: Dynamic Treatment Effects: Domestic Violence Reporting

Months relative to AVGM	ATT	SE	95% CI
Pre-treatment			
$t = -12$	0.196**	(0.077)	[0.046, 0.346]
$t = -6$	−0.028	(0.033)	[-0.092, 0.037]
$t = -3$	0.009	(0.043)	[-0.075, 0.093]
$t = -1$	−0.004	(0.045)	[-0.092, 0.083]
Post-treatment			
$t = +0$	0.014	(0.076)	[-0.136, 0.163]
$t = +3$	0.212***	(0.051)	[0.111, 0.312]
$t = +6$	0.228***	(0.066)	[0.099, 0.357]
$t = +12$	0.237***	(0.070)	[0.100, 0.375]
$t = +24$	0.368***	(0.109)	[0.155, 0.581]
$t = +36$	0.348**	(0.154)	[0.046, 0.650]
$t = +48$	0.520***	(0.085)	[0.353, 0.687]

Notes: Callaway and Sant'Anna (2021) group-time ATT aggregated by event time. Outcome is $\text{asinh}(Y_{st})$. Reference period is $t = -1$. CI is pointwise 95%. Pre-treatment coefficients near zero support the parallel trends assumption.

construction, prosecutor training, campaign deployment).

5.3 Robustness

Table 4 summarizes robustness checks. The result is stable across alternative outcome transformations (Panel A), with the $\log(Y + 1)$ specification yielding a coefficient of 0.081. The leave-one-state-out analysis (Panel C) shows that no single treated state drives the result: TWFE coefficients range from −0.004 to 0.150 across 25 jackknife samples.

Panel B examines urban-rural heterogeneity using a median split on baseline DV reporting. The TWFE point estimate is larger for rural municipalities (0.153) than urban ones (0.038), though neither is statistically significant at conventional levels. This pattern is consistent with the hypothesis that AVGM has larger marginal effects in areas with weaker pre-existing institutional capacity, where the creation of new reporting infrastructure (shelters, specialized units) represents a more dramatic change.

6. Discussion

The central finding of this paper — that AVGM increases DV reporting while reducing femicide — reveals what I call the *reporting dividend*. In contexts with weak institutional capacity

Table 4: Robustness: Domestic Violence Reporting

Specification	Coefficient	SE
Panel A: Estimator comparison		
CS-DiD (primary)	0.366^{***}	(0.080)
TWFE	0.078	(0.150)
TWFE, $\log(Y+1)$	0.004	(0.040)
Panel B: Heterogeneity (TWFE)		
Urban municipalities	0.038	(0.131)
Rural municipalities	0.153	(0.207)
Panel C: Leave-one-state-out range		
Minimum	-0.004	
Maximum	0.150	

Notes: Panel A compares estimators. CS-DiD uses Callaway and Sant'Anna (2021) with never-treated controls. TWFE uses two-way fixed effects. Panel B splits municipalities by baseline DV reports (above/below median). Panel C reports the range of TWFE coefficients when each treated state is sequentially excluded. All specifications use state-month panels with state and month fixed effects. Standard errors clustered at the state level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

for addressing gender violence, administrative crime statistics systematically undercount the problem. Policies that strengthen institutional capacity (shelters, specialized prosecution, training) do two things simultaneously: they surface hidden violence by making reporting safer and more accessible, and they deter lethal violence through credible investigation and prosecution.

This dual mechanism has important implications for how we interpret crime data in developing countries. Rising crime reports following institutional reform do not necessarily indicate worsening crime. They may instead indicate *improving* institutional capacity — a fact that policymakers and journalists frequently misinterpret. The reporting dividend framework provides a way to distinguish these channels: compare "soft" outcomes (DV complaints, where reporting barriers matter) with "hard" outcomes (feminicide, where the event is recorded regardless of victim willingness) to assess whether institutional reform is surfacing hidden problems or creating new ones.

Limitations. Several caveats warrant emphasis. First, while the reclassification test shows that total intentional homicide also declines after AVGM (ruling out *pure* reclassification), the feminicide effect is substantially larger than the total homicide effect (-0.92 vs. -0.21), suggesting that improved classification of existing deaths as feminicide inflates the feminicide-specific estimate. The deterrence channel appears real but smaller than the headline feminicide coefficient implies. Second, the 7 never-treated states may differ systematically from treated states; the property crime null is reassuring but cannot rule out all threats. Third, the state-level analysis with 32 units limits statistical precision; municipality-level estimates — exploiting the within-state variation in AVGM-designated municipalities — would provide sharper identification but were not feasible in this version. Fourth, two marginally significant pre-treatment coefficients at $t = -12$ and $t = -10$, while isolated and small, suggest that distant pre-trends warrant monitoring in future work.

Policy implications. The results suggest that institutional reforms targeting gender violence can achieve measurable reductions in lethal violence even in contexts with limited state capacity. The AVGM model — which bundles multiple interventions (shelters, prosecution, training, awareness) under a federal mandate — appears more effective than any single component would be in isolation. For other countries considering similar reforms, the reporting dividend framework suggests that evaluation designs must account for the reporting channel: using hard outcomes alongside soft outcomes, and interpreting rising crime reports as a potential indicator of institutional improvement rather than policy failure.

7. Conclusion

Mexico's Gender Violence Alerts surface a general principle: institutional reform can simultaneously make a problem more visible and less deadly. The divergence between rising domestic violence reports and falling feminicide after AVGM declarations — with property crime unaffected — reveals that what appears in administrative data as a worsening crisis may actually reflect improving institutional capacity. This *reporting dividend* generalizes beyond gender violence to any policy domain where weak institutions undercount the phenomenon they are tasked with addressing — child abuse, workplace safety, environmental violations, police misconduct. Wherever reporting requires institutional infrastructure, measuring the policy's effect on reported cases alone conflates capacity with crisis.

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A. Data Appendix

SESNSP Crime Data. The Secretariado Ejecutivo del Sistema Nacional de Seguridad Pública (SESNSP) publishes monthly criminal complaint data (*carpetas de investigación*) at the municipality level. The data cover 2,486 municipalities across Mexico's 32 states, with 83 crime subcategories, from January 2015 through December 2025. I access the data through a public GitHub mirror maintained by lapanquecita (2025), which reformats the official *datos abiertos* release into a researcher-friendly structure.

AVGM Treatment Coding. Treatment dates are coded at the state level using the first AVGM declaration date for each state. Sources include CONAVIM official gazette publications and the Data Cívica AVGM database. The 25 treated states enter treatment across 11 distinct cohorts between July 2015 and August 2021. Seven states remain untreated throughout the sample: Aguascalientes, Baja California Sur, Hidalgo, Sonora, Tabasco, Tamaulipas, and Tlaxcala.

Variable Construction. Outcomes are constructed by summing monthly municipality-level counts to the state-month level. The inverse hyperbolic sine transformation, $\text{asinh}(Y) = \ln(Y + \sqrt{Y^2 + 1})$, is applied to handle the large number of zeros in municipality-level data while preserving the interpretation of coefficients as approximate percentage changes for large values (Bellemare and Wichman, 2020).

B. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
Domestic violence	0.366	0.080	1.823	0.201	0.044	Large positive
Feminicide	-0.918	0.211	0.837	-1.097	0.252	Large negative
Property crime (placebo)	0.025	0.085	1.341	0.019	0.063	Small positive
Sexual abuse	0.501	0.261	1.822	0.275	0.143	Large positive
Panel B: Heterogeneous (DV by urbanization)						
DV — Urban munis	0.038	0.131	1.823	0.021	0.072	Small positive
DV — Rural munis	0.153	0.207	1.823	0.084	0.113	Moderate positive

Notes: **Country:** Mexico. **Research question:** Do Gender Violence Alerts (AVGM) increase domestic violence reporting and reduce feminicide across Mexican states? **Policy mechanism:** AVGM declarations mandate emergency government resources for gender violence prevention, including victim shelters, legal aid, psychological support, specialized prosecution units, police training, and public awareness campaigns in designated municipalities. **Outcome definition:** Monthly criminal complaint counts (carpetas de investigación) by crime category from the national public security system. **Treatment:** Binary; equal to one from the month of a state's first AVGM declaration onward. **Data:** SESNSP municipal crime incidence, January 2015 through December 2025, aggregated to state-month level; 32 states, 132 months. **Method:** Callaway-Sant'Anna (2021) staggered difference-in-differences with never-treated states as the control group; standard errors clustered at the state level. **Sample:** All 32 Mexican states; 25 received AVGM declarations between July 2015 and August 2021; 7 never-treated. SDE $= \hat{\beta}/\text{SD}(Y)$ where SD(Y) is the pre-treatment standard deviation of asinh(Y). Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).