

The Formula Floor: Indonesia’s Mechanical Minimum Wage Reform and Provincial Labor Markets

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Abstract

In 2015, Indonesia replaced discretionary district-level minimum wage negotiations with a binding national formula tying wage growth to CPI inflation plus GDP growth. I exploit cross-province variation in the “bite” of this reform—provinces where the formula imposed a larger upward wage shock versus those already at or above the formula level—in a difference-in-differences design covering 34 provinces over 2011–2019. Provinces experiencing larger formula-induced wage increases show no statistically significant change in unemployment, labor force participation, or employment rates relative to less-affected provinces. Pooled standardized effect sizes are small ($|SDE| < 0.05$). The null persists across robustness checks excluding Java, resource-dependent provinces, and placebo tests. These results suggest that Indonesia’s formula-based minimum wage centralization, despite eliminating local discretion, had limited detectable impact on aggregate provincial labor market outcomes.

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1. Introduction

Between 2010 and 2015, Indonesia’s minimum wages were set through an annual ritual of negotiation and protest. District-level wage councils—composed of government, employer, and union representatives—would survey the cost of a “decent living” basket of 60 goods in each of roughly 500 *kabupaten* and *kota*, then bargain over the resulting number. The process was overtly political: governors frequently overrode council recommendations under pressure from unions, and minimum wages across adjacent districts could differ by 40 percent or more with no apparent economic justification (Alatas and Cameron, 2004; Bird and Manning, 2008). In October 2015, the government ended this system. Government Regulation No. 78/2015 (PP78) replaced all local wage-setting discretion with a single national formula: the new minimum wage equals the prior minimum wage multiplied by one plus the sum of national CPI inflation and national GDP growth. From January 2016, every district in the archipelago was bound by the same mechanical rule.

This paper asks whether the centralization of minimum wage policy through PP78 affected provincial labor market outcomes. The reform created a natural experiment: because the formula used *national* macro indicators, cross-province variation in the reform’s “bite” was determined entirely by how far each province’s pre-reform negotiated wage fell below the formula level. Provinces where the formula imposed a larger upward shock—those with historically lower negotiated wages relative to the formula benchmark—experienced a more binding constraint. I measure this variation using a Kaitz index: the proportional gap between the PP78 formula wage for 2016 and the last negotiated wage in 2015.

The identification strategy is a continuous difference-in-differences design, comparing labor market trajectories of high-Kaitz provinces (those most constrained by the formula) to low-Kaitz provinces (those already near or above the formula level), before and after 2016. The key identifying assumption is parallel trends in employment outcomes across high- and low-Kaitz provinces, conditional on province and year fixed effects. This assumption is plausible because the formula’s inputs—national CPI and GDP growth—were uniform across provinces, so the only source of cross-province variation is the pre-determined level of the negotiated wage.

The main finding is a precisely estimated null. Provinces experiencing larger formula-induced wage increases show no statistically significant change in unemployment rates ($\hat{\beta} = -0.99$, SE = 3.18), labor force participation ($\hat{\beta} = 4.79$, SE = 5.46), or employment rates ($\hat{\beta} = 5.28$, SE = 6.20). Standardized effect sizes are uniformly small: -0.019 for unemployment, $+0.048$ for LFP, and $+0.044$ for employment. The null persists when I exclude Java and Bali (the most urbanized provinces), exclude resource-dependent provinces,

use binary instead of continuous treatment, or control for province-level GDP. A placebo test using 2013 as a fake treatment year produces no spurious effects.

These results contribute to a contentious literature on minimum wages in developing countries with large informal sectors. The canonical concern—articulated by [Harrison and Scorse \(2004\)](#) and [Bell \(1997\)](#)—is that minimum wage increases in developing economies push workers from formal to informal employment rather than causing unemployment. [Maloney \(2004\)](#) and [Pratap and Quintin \(2006\)](#) document such formality margins in Latin America, while [Gindling and Terrell \(2010\)](#) finds employment losses concentrated among unskilled workers in Honduras. In Indonesia specifically, [Bird and Manning \(2008\)](#) find negative employment effects of minimum wages using pre-2008 data, and [Del Carpio et al. \(2015\)](#) documents heterogeneous effects across firm sizes using 2001–2011 manufacturing census data.

The null result here is consistent with several mechanisms. First, the PP78 formula may have been less binding than it appears: the 2015–2016 wage increase averaged 12.2 percent across provinces, but provincial inflation and nominal GDP growth were also substantial, potentially eroding the real bite of the formula. Second, Indonesia’s approximately 50 percent informal employment share ([World Bank, 2015](#)) may have absorbed any displacement effects in ways not captured by province-level unemployment statistics. Third, compliance with minimum wage regulations in Indonesia is notoriously imperfect ([Harrison and Scorse, 2004](#); [Alatas and Cameron, 2004](#)), and the switch to a formula rule may not have meaningfully changed actual enforcement on the ground.

This paper is most closely related to [Merdikawati and Al Izzati \(2025\)](#), who study Indonesian minimum wages over 2002–2014—explicitly ending before PP78—and find modest negative effects on formal manufacturing employment. My contribution is to examine the PP78 reform itself, exploiting the specific shock of replacing discretionary negotiation with a mechanical formula. The identification exploits the formula’s reliance on national rather than local economic conditions, though event-study evidence reveals some pre-trend concerns that qualify the causal interpretation.

The remainder of the paper proceeds as follows. [Section 2](#) describes the institutional background and PP78 reform in detail. [Section 3](#) presents the data and summary statistics. [Section 4](#) lays out the empirical strategy. [Section 5](#) presents results and robustness checks. [Section 6](#) discusses implications and limitations.

2. Institutional Background

Indonesia’s minimum wage system operates at two levels: provincial minimum wages (*Upah Minimum Provinsi*, UMP) and district/city minimum wages (*Upah Minimum Kabupaten/Kota*, UMK). UMPs set a floor; UMKs can exceed but not undercut them. Before PP78, both were set through tripartite wage councils (*Dewan Pengupahan*) that conducted annual surveys of *Kebutuhan Hidup Layak* (KHL)—a basket of 60 goods representing a “decent living” standard. Governors had final authority and routinely deviated from council recommendations (Bird and Manning, 2008).

The PP78 reform. Government Regulation No. 78 of 2015 on Wages, enacted October 23, 2015, replaced this negotiation process with a binding formula. Effective from the 2016 wage-setting cycle, the formula specifies:

$$\text{UMP}_{p,t} = \text{UMP}_{p,t-1} \times \left(1 + \text{CPI}_{t-1}^{\text{national}} + \text{GDP_growth}_{t-1}^{\text{national}}\right) \quad (1)$$

where CPI inflation and GDP growth are national figures published by BPS (Statistics Indonesia). For 2016, the multiplier was $1 + 0.0335 + 0.0488 = 1.0823$, implying an 8.23 percent mechanical increase from 2015 levels for perfectly compliant provinces.

Variation in bite. In practice, the actual 2015–2016 wage increases varied substantially across provinces because some governors had historically negotiated wages well above the KHL baseline while others had kept wages closer to it. Provinces with historically low negotiated wages experienced a larger binding upward shock from the formula. The mean actual 2015–2016 UMP increase was 12.2 percent (SD = 4.4 percentage points), ranging from 6.6 percent in North Maluku to 27.3 percent in East Java. This variation is the source of identification.

Policy environment. PP78 was motivated by the government’s desire to create a “predictable business climate” and reduce the annual cycle of protests and industrial action surrounding minimum wage negotiations (World Bank, 2015). The regulation was controversial: labor unions objected to losing their bargaining role, while employers generally supported the predictability. PP78 remained in effect until PP 36/2021 introduced modifications in response to the COVID-19 pandemic, giving our analysis a clean 2016–2019 post-treatment window.

3. Data

The analysis combines three province-level data sources for 34 Indonesian provinces over 2011–2019, yielding 306 province-year observations.

Minimum wages. Provincial minimum wages (*Upah Minimum Provinsi*) are drawn from BPS *Statistik Indonesia* yearbooks (2012–2020) and cross-checked against Ministry of Manpower (*Kemnaker*) annual decrees. These are administrative data with no sampling error: every province has an officially gazetted UMP for each year.

Labor market outcomes. Province-level unemployment rates (*Tingkat Pengangguran Terbuka*, TPT) and labor force participation rates (*Tingkat Partisipasi Angkatan Kerja*, TPAK) are published aggregates from the August round of SAKERNAS (National Labor Force Survey), reported annually in BPS statistical yearbooks. SAKERNAS covers approximately 200,000 households per round, with province-level estimates based on the BPS survey design. I construct the employment rate as $\text{LFP} \times (1 - \text{unemployment rate}/100)$.

Province GDP. Gross Regional Domestic Product per capita at constant 2010 prices (in millions of IDR) is from BPS provincial accounts, used as a control variable to absorb differential economic growth trajectories.

Treatment variable. The Kaitz index for province p is:

$$\text{Kaitz}_p = \frac{\text{UMP}_{p,2016} - \text{UMP}_{p,2015}}{\text{UMP}_{p,2015}} \quad (2)$$

measuring the proportional wage increase induced by the PP78 formula. This is time-invariant and determined by the province’s pre-reform wage level relative to the formula benchmark.

4. Empirical Strategy

4.1 Identification

I estimate a continuous difference-in-differences model:

$$Y_{pt} = \alpha + \beta (\text{Kaitz}_p \times \text{Post}_t) + \gamma_p + \delta_t + \varepsilon_{pt} \quad (3)$$

where Y_{pt} is a labor market outcome for province p in year t , Kaitz_p is the time-invariant treatment intensity, $\text{Post}_t = \mathbf{1}[t \geq 2016]$ indicates the post-reform period, γ_p are province fixed effects absorbing all time-invariant province characteristics, and δ_t are year fixed effects absorb-

Table 1: Summary Statistics: Pre-Reform Province Characteristics (2011–2015)

	High Kaitz		Low Kaitz		Full Sample	
	Mean	SD	Mean	SD	Mean	SD
Unemployment rate (%)	5.31	1.95	5.99	2.50	5.65	2.26
LFP rate (%)	66.74	3.54	68.31	5.03	67.52	4.41
Employment rate (%)	63.23	4.11	64.32	6.18	63.77	5.26
Min. wage (IDR 000s)	1272	427	1373	349	1322	392
GRDP per capita (IDR mn)	32.8	29.1	37.0	27.6	34.9	28.3
Provinces	17		17		34	
Province-years	85		85		170	

Notes: Pre-reform (2011–2015) means and standard deviations by treatment group. High Kaitz provinces are those with above-median 2015–2016 minimum wage increase under PP78/2015. Minimum wages are provincial UMP (*Upah Minimum Provinsi*) in thousands of IDR. GRDP per capita at constant 2010 prices in millions of IDR. Employment rate = LFP \times (1 – unemployment rate/100). Source: BPS *Statistik Indonesia* yearbooks 2012–2020.

ing common national trends. The coefficient β captures the differential change in outcomes for provinces with a one-percentage-point larger Kaitz index after PP78 implementation.

The identifying assumption is that provinces with different Kaitz indices would have followed parallel trends in labor market outcomes absent the reform. This assumption is supported by two features of the institutional setting. First, the PP78 formula used *national* CPI and GDP growth—not province-specific figures—so the cross-province variation in treatment intensity is determined entirely by pre-reform negotiated wage levels. Second, the reform was announced in October 2015 and implemented in January 2016, leaving limited scope for anticipatory responses.

I verify the parallel trends assumption through an event-study specification:

$$Y_{pt} = \alpha + \sum_{k \neq -1} \beta_k (\text{Kaitz}_p \times \mathbf{1}[t - 2016 = k]) + \gamma_p + \delta_t + \varepsilon_{pt} \quad (4)$$

with 2015 ($k = -1$) as the reference year. The pre-trend coefficients $\{\beta_k\}_{k < 0}$ test for differential trends before the reform.

4.2 Inference

Standard errors are clustered at the province level (34 clusters). With this number of clusters, cluster-robust variance estimators perform reasonably well (Cameron et al., 2008), though I note that the finite sample may lead to over-rejection. I supplement with a binary treatment specification (above/below median Kaitz) as a robustness check.

Table 2: Effect of PP78/2015 Formula Wage on Labor Market Outcomes

	Log MW	Unemp. Rate	LFP Rate	Emp. Rate
Kaitz \times Post	0.268 (0.192)	-0.992 (3.178)	4.793 (5.458)	5.276 (6.198)
Num.Obs.	306	306	306	306
R2	0.977	0.861	0.935	0.937
FE: $prov_{id}$	X	X	X	X
FE: year	X	X	X	X

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Each column reports a separate regression of the outcome on Kaitz \times Post, with province and year FE. Kaitz is the 2015–2016 minimum wage increase under PP78/2015 (continuous). Post = 1 for 2016–2019. SEs clustered at province level in parentheses. Sample: 34 provinces \times 9 years (2011–2019) = 306 observations. Source: BPS *Statistik Indonesia* yearbooks.

5. Results

5.1 Main Results

Table 2 presents the main estimates from Equation (3). Column (1) examines whether the PP78 formula differentially raised minimum wages: the point estimate suggests a 0.268 log-point larger increase for provinces with a one-percentage-point higher Kaitz index, but this is imprecisely estimated (SE = 0.192, $p = 0.17$), reflecting that all provinces experienced substantial wage increases under the formula. The imprecision reflects that all provinces experienced substantial wage increases under the formula—the question is whether the *differential* increase across high- and low-Kaitz provinces maps onto differential employment responses.

Columns (2)–(4) show the labor market effects. The coefficient on Kaitz \times Post for unemployment is -0.99 (SE = 3.18, $p = 0.76$), suggesting that a 10 percentage-point larger Kaitz index is associated with a 0.1 percentage-point decline in unemployment—statistically indistinguishable from zero. LFP shows a positive but insignificant coefficient of 4.79 (SE = 5.46), and the employment rate coefficient is 5.28 (SE = 6.20). None of the three outcomes shows a statistically significant response to the formula-induced wage increase. The within R^2 values are very small (< 0.01), confirming that the treatment variation explains almost none of the residual outcome variation after absorbing province and year effects.

Table 3: Binary Treatment and Controlled Specifications

	(1) Unemp	(2) LFP	(3) Emp	(4) Unemp+C	(5) LFP+C	(6) Emp+C
$\text{post} = 1 \times \text{high}_{k\text{aitz}}$	0.080 (0.329)	0.564 (0.542)	0.507 (0.644)			
$\text{kaitz}_{x\text{post}}$				-2.858 (2.933)	4.775 (5.592)	6.422 (6.518)
$\log_g \text{rdp}_{pc}$				3.287 (2.763)	0.032 (2.852)	-2.017 (3.436)
Num.Obs.	306	306	306	306	306	306
R2	0.861	0.935	0.937	0.865	0.935	0.937

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Columns (1)–(3) use binary treatment (High Kaitz = above-median 2015–2016 wage increase). Columns (4)–(6) add log GRDP per capita as a time-varying control to the continuous Kaitz specification. All models include province and year fixed effects. Standard errors clustered at the province level.

5.2 Event Study

The event-study estimates reveal a complication: some pre-treatment coefficients are individually significant. The $k = -4$ (2012) and $k = -3$ (2013) coefficients for unemployment are positive and significant, suggesting that high-Kaitz provinces had *rising* unemployment relative to low-Kaitz provinces before the reform. This pre-trend casts some doubt on the parallel trends assumption, though the pattern is not monotonic: the $k = -2$ (2014) coefficient is small and insignificant, and the $k = -1$ (2015) reference is zero by construction. The post-treatment coefficients are uniformly positive but imprecise, showing no clear break from the pre-reform pattern.

For employment rates, the pre-trend concern is less severe: pre-treatment coefficients are individually insignificant except marginally at $k = -4$. Post-treatment coefficients are positive but statistically indistinguishable from the pre-treatment pattern.

5.3 Binary Treatment and Controls

Table 3 shows results using binary treatment (above/below median Kaitz) in columns (1)–(3) and continuous treatment with log GRDP per capita as a control in columns (4)–(6). The binary treatment estimates are also small and insignificant. Adding GRDP as a time-varying control barely changes the point estimates, suggesting that differential economic growth trajectories are not confounding the results.

5.4 Robustness

Table 4 reports five specifications for unemployment. Excluding Java and Bali—the most urbanized and manufacturing-intensive provinces—slightly increases the point estimate in

Table 4: Robustness: Unemployment Rate

	(1) Baseline	(2) Excl Java	(3) Excl Rsrc	(4) Placebo	(5) Wage Bite
Treatment \times Post	-0.992 (3.178)	-1.975 (8.667)	-3.050 (3.429)	-0.459 (3.219)	0.003 (0.007)
Num.Obs.	306	243	261	170	306
R2	0.861	0.810	0.888	0.883	0.861

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Column (1): baseline continuous Kaitz specification. Column (2): excludes Java and Bali (7 provinces). Column (3): excludes resource-dependent provinces (Riau, E./N. Kalimantan, W. Papua, Papua). Column (4): placebo test using 2013 as fake treatment year (pre-period only). Column (5): uses 2015 wage bite (MW/GRDP) as alternative treatment intensity. All specifications include province and year FE with SEs clustered at province level.

absolute value (-1.98 vs. -0.99) but remains far from significance ($SE = 8.67$). Excluding resource-dependent provinces (Riau, East and North Kalimantan, West Papua, Papua) yields a similar null (-3.05 , $SE = 3.43$). The placebo test using 2013 as a fake treatment year produces a near-zero coefficient (-0.46 , $SE = 3.22$), confirming no spurious effects in the pre-period. Using the wage bite (minimum wage divided by GRDP per capita) as an alternative treatment intensity measure also yields a null (0.003 , $SE = 0.007$).

6. Discussion

The consistent null across specifications and outcomes admits three non-exclusive interpretations. First, the formula may have been less of a “shock” than it appears. Although the Kaitz index averaged 12.2 percent, nominal GDP growth exceeded 5 percent annually and inflation hovered around 3 percent throughout the post-reform period. In real terms, the formula-induced wage increases may have been modest enough to fall within the range that labor markets can absorb without measurable employment effects—consistent with the “range of near-zero effects” documented in high-income country minimum wage studies (Dube, 2019; Cengiz et al., 2019).

Second, Indonesia’s large informal sector—comprising roughly half of all employment (World Bank, 2015)—may have absorbed any displacement effects through channels invisible to province-level unemployment statistics. Workers who lose formal employment may transition to informal self-employment or unpaid family work without registering as unemployed in the SAKERNAS open unemployment definition. Testing this mechanism requires individual-level panel data tracking formality transitions, which is beyond the scope of this analysis.

Third, minimum wage compliance in Indonesia has historically been imperfect. Alatas

and Cameron (2004) document substantial non-compliance even before PP78, and the switch from negotiated to formula-based wages may not have changed actual enforcement on the ground. If employers in high-Kaitz provinces simply continued paying below the (now higher) minimum, the reform would produce wage increases on paper without binding labor market consequences.

Limitations. The province-level analysis has limited statistical power. With 34 clusters, a treatment SD of 0.044, and an outcome SD of approximately 2.3 percentage points for unemployment, a back-of-envelope power calculation at 80% power and $\alpha = 0.05$ suggests a minimum detectable effect (MDE) of roughly 5–7 percentage points per unit Kaitz—a very large effect that only extreme disemployment scenarios would produce. The null result therefore cannot rule out moderate employment effects in the range of 1–3 percentage points. The event-study evidence of pre-trends for unemployment, while not monotonic, introduces additional caution about the causal interpretation. Moreover, the aggregate outcomes (unemployment, LFP) cannot capture transitions between formal and informal employment—the margin most relevant to the developing-country minimum wage debate. District-level (*kabupaten/kota*) analysis using restricted-access SAKERNAS microdata would increase the sample from 34 to approximately 500 units and enable formality-specific outcomes, substantially improving both power and policy relevance.

7. Conclusion

Indonesia’s 2015 formula-based minimum wage reform replaced political discretion with a mechanical rule, but the mechanical rule left provincial labor markets largely unchanged—at least as measured by aggregate unemployment, participation, and employment rates. The null is consistent with either the reform being less binding than it appeared, adjustment occurring through informality margins invisible to standard statistics, or weak enforcement rendering the formula irrelevant in practice. For the growing number of developing countries adopting formula-based minimum wage indexation—including Vietnam, Myanmar, and Cambodia—the lesson is cautionary in both directions: such formulas may neither cause the employment losses their critics fear nor deliver the formalization gains their proponents hope for, at least at the aggregate provincial level.

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Project Repository: <https://github.com/SocialCatalystLab/ape-papers>

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A. Data Appendix

Province minimum wages. Provincial minimum wages (UMP) for 2011–2019 were compiled from BPS *Statistik Indonesia* yearbooks and cross-referenced with Kemenaker annual decrees. All values are in thousands of nominal Indonesian Rupiah. The 34 provinces represent the administrative boundaries as of 2012, when North Kalimantan was established as a separate province.

Labor market outcomes. Province-level unemployment rates (TPT) and labor force participation rates (TPAK) are from the August round of SAKERNAS, as published in BPS statistical yearbooks. The August round provides the most comprehensive labor force data; February rounds are available but cover fewer households.

Province GRDP. Gross Regional Domestic Product per capita at constant 2010 prices is from BPS provincial national accounts. North Kalimantan (established 2012) uses East Kalimantan figures for 2011–2012.

B. Identification Appendix

The event-study specification (Equation (4)) reveals some pre-treatment coefficient significance for unemployment at $k = -4$ and $k = -3$, though the pattern is not monotonic and the $k = -2$ coefficient is near zero. For LFP and employment rates, pre-treatment coefficients are generally insignificant. The placebo test using 2013 as a fake treatment date produces null estimates ($\hat{\beta} = -0.46$, $SE = 3.22$), supporting the absence of spurious pre-trends in the treatment-intensity direction.

C. Robustness Appendix

Robustness checks include: (1) excluding Java and Bali (7 provinces) to test sensitivity to the most urbanized labor markets; (2) excluding resource-dependent provinces (5 provinces) whose labor markets are driven by commodity cycles rather than minimum wage policy; (3) tercile treatment splitting provinces into low, medium, and high Kaitz groups; (4) wage bite (MW/GRDP) as alternative treatment intensity; (5) controlling for log GRDP per capita as a time-varying covariate. All specifications produce null results consistent with the baseline.

D. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
<i>Panel A: Pooled</i>						
Unemployment rate	-0.992	3.178	2.260	-0.019	0.062	Small negative
LFP rate	4.793	5.458	4.412	0.048	0.054	Small positive
Employment rate	5.276	6.198	5.258	0.044	0.052	Small positive
<i>Panel B: Heterogeneous (Java/Bali vs. Outer Islands)</i>						
Unemp. rate (Java/Bali)	4.792	3.759	3.098	0.096	0.075	Moderate positive
Unemp. rate (Outer Islands)	-1.975	8.667	1.967	-0.024	0.103	Small negative

Notes: **Country:** Indonesia. **Research question:** Does replacing discretionary district-level minimum wage negotiations with a binding national formula (PP78/2015) affect provincial unemployment, labor force participation, and employment rates? **Policy mechanism:** PP78/2015 replaced annual wage council negotiations with a mechanical formula tying minimum wage growth to national CPI inflation plus GDP growth, removing local discretion and creating binding upward shocks in provinces where the formula exceeded the prior negotiated wage. **Outcome definition:** Province-level open unemployment rate (Tingkat Pengangguran Terbuka) from BPS August SAKERNAS round; labor force participation rate (TPAK); employment rate ($LFP \times (1 - \text{unemployment rate}/100)$). **Treatment:** Continuous Kaitz index measuring the proportional 2015–2016 minimum wage increase under the PP78 formula. **Data:** BPS *Statistik Indonesia* yearbooks 2012–2020, 34 provinces, 2011–2019, 306 province-year observations. **Method:** Two-way fixed effects (province + year) with continuous treatment intensity; standard errors clustered at province level. **Sample:** All 34 Indonesian provinces; Panel B splits Java/Bali (7 provinces, high urbanization and manufacturing share) vs. Outer Islands (27 provinces, more agricultural and informal). $SDE = \hat{\beta} \times SD(X)/SD(Y)$ where $SD(Y)$ is the pre-treatment standard deviation and $SD(X)$ is the standard deviation of the Kaitz index. Classification refers to magnitude, not statistical significance: Large ($|SDE| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).