

Ms. Ref. No.: YJCEC-D-25-01052R1

Title: Development at the Point of a Bayonet? Assessing the Timing and Origins of the Chilean Miracle

Journal of Comparative Economics

Dear Professor Paniagua,

Thank you for submitting the revised version of your paper to the Journal of Comparative Economics. I have now heard back from two referees, both of whom find that the manuscript has substantially improved, but both of them have some remaining concerns that they would like to see addressed. I see a clear path for convergence, so I am quite optimistic about the prospects of the paper as long as you fully address the issues raised by the referees.

Both referees provide a number of very useful comments, and I encourage you to address all the points raised by the referees.

Please provide, along with the revision, a point-by-point answer to all the comments raised by the referees (or provide a suitable rebuttal to each reviewer's comment not addressed).

I look forward to receiving your revised manuscript.

Yours sincerely,

Ruben Enikolopov

Editor

Journal of Comparative Economics

Reviewers' comments:

Reviewer #2: Overall impression. The authors have engaged seriously and constructively with my first-round comments. The manuscript has been materially improved: the framing around the "missing counterfactual" is now sharpened from page 4, the "Pinochet-alone sufficiency" language has been adopted in Section 3.2, the new Appendix 7 gives a substantive treatment of anticipation and Washington-Consensus contamination, and the new Appendix 8.5 documents robustness to alternative pre- and post-treatment windows. Most of my concerns have been addressed. A few issues remain where the revision is partial rather than complete, and I flag them below in the hope they help in a final polish.

Framing (suggestion 1 — missing counterfactual). Adequately addressed. The revised introduction now states explicitly that the paper's contribution is to provide a "credible forward-looking counterfactual that has been largely missing from the literature," and contrasts this directly with Escalante (2022)'s backward-looking design. The sharper contrast with prior SCM work centered on 1973 is welcome.

Framing (suggestion 2 — flag the conceptual innovation earlier). Partially addressed. The dual empirical strategy is now mentioned upfront, but the specific reframing — democracy as the intervention, dictatorship as the baseline — could still be stated more sharply in the first two pages. A single sentence in the abstract or in the first paragraph of the introduction making this explicit would pay off.

Comment 1 — bundled treatment. Well addressed. The new paragraph in Section 3.2 (pp. 22-23) adopts the suggested framing almost verbatim, and the interpretive language throughout the paper has been softened. One small residual: the title ("Development at the Point of a Bayonet?") and the abstract's "casts doubts on the narrative of the 'Pinochet model'" still lean causal. I would not insist on changing the title, but it is worth noting the mild tension between the new cautious framing in the body and the more assertive framing at the front of the paper.

Comment 2 — donor pool. Partially addressed. The authors respond with a battery of useful checks: cross-validation (Table 4, Figure 4), a Bayesian re-estimation, alternative GDP measures (Appendix 8.1), a leave-one-out jackknife (Appendix 8.4), and — new in this revision — a specification that simultaneously drops all four positively weighted

donors (Figure 10). This last exercise is a genuinely strong robustness check and does most of the work my comment was asking for. What remains somewhat unaddressed is the specific request in my first-round comment to restrict the donor pool to more institutionally comparable countries (e.g., Latin American middle-income peers, or a Polity-score-restricted pool). The jackknife tells us the result does not hinge on any one (or all) of the top-weighted donors; it does not directly tell us what happens when the donor pool is redefined by institutional similarity. I think a single additional specification — for instance, restricting donors

to Latin American countries that were democracies in 1990 — would close this loop at low cost. I would also encourage a short sentence acknowledging that the 72.5% weight on Colombia in the benchmark is unusually concentrated, and that this is one of the reasons the cross-validated weights (which distribute far more evenly) are informative.

Comment 3 — anticipation. Well addressed. Appendix 7 is now one of the strongest parts of the paper. The distinction between anticipation of the event and anticipation of the policy content is genuinely useful, the Boas (2015) and Kornbluh (2018) evidence on pre-plebiscite regime intentions is compelling, and the bias-direction argument (footnote 20 in particular) is correct as stated for temporary level effects, with the appropriate caveat for permanent growth-rate effects. I am satisfied with this response.

Comment 4 — globalization and external conditions. Partially addressed. The three-pronged response (SCM controls for common shocks; average placebo near zero post-1990; Chile's reform package qualitatively distinct in Table 5) is reasonable, and Table 5 is a useful addition. However, the third argument is doing a lot of the work, and it shades into a slightly different claim than what I originally raised: it effectively says that Chile benefited differentially from globalization because its reform package allowed it to. That may well be true, but it means the treatment is now conceptually "democracy + reforms + differential capacity to absorb global integration," which further complicates the already bundled nature of the treatment (see Comment 1). A brief acknowledgment of this conceptual point would be helpful; the paper does not need to resolve it, only to recognize it.

Comment 5 — sample window ending in 2009. Partially addressed. Appendix 8.5 and the new footnote 9 demonstrate that results are robust to a shorter (1990-2000) post-treatment window and a longer (1973-1989) pre-treatment window. This is useful. What I originally asked for, though, was an ex ante justification for why 1990-2009 is the preferred window — e.g., avoiding contamination from the post-2010 slowdown, the

commodity supercycle, or the 2019 social unrest. The current footnote 13 comes close but frames 2009 as "might be considered a relatively large window," which is defensive rather than affirmative. One or two sentences giving the positive reason for the 2009 cutoff would strengthen the paper.

Comment 6 — tone. Partially addressed. The language has been moderated in several places, and Section 2 in particular reads more neutrally. Some residual normative framing remains — e.g., the closing line of the conclusion about "the recent resurgence of authoritarian narratives," and the "development at the point of a bayonet" framing that runs through the paper. Given the topic, a fully neutral tone is probably neither achievable nor desirable, but a final editing pass on the introduction and conclusion to remove the strongest value-laden phrases would help. This is a presentation issue, not a substantive one.

Comment 7 — mechanisms (minor). Adequately addressed. The four-channel discussion at the end of Section 3.2 (credibility, human capital, institutional upgrades, trade openness) does what I asked for in my minor comment. Nothing more is needed here.

New issues arising from the revision

1. SCM vs. Bayesian gap. The Bayesian model delivers a ~46% premium while the benchmark SCM delivers ~58%. The authors describe the two as "aligned" and "consistent," which is fair qualitatively, but the ~12 percentage-point gap is non-trivial and goes unremarked. A sentence noting the gap and attributing it to the different weighting approaches (point-optimized vs. posterior-averaged Dirichlet weights) would add precision.
2. Pre-treatment volatility in the extended window. In Appendix Section 8.5, the 1973-1989 pre-treatment extension incorporates the 1975 stabilization shock and the 1982 crisis, both of which are unusually volatile years. The pre-treatment fit still looks good in Figure 11, which is reassuring, but a brief comment on how the synthetic accommodates these episodes would reinforce the credibility of the robustness check.
3. Engagement with Ormeño and Korolev (2026). The new discussion of this closely related paper currently lives only in footnote 7 and in the Reviewer 1 memo. Given the methodological overlap, a short paragraph in the main text would better serve the reader and make the paper's incremental contribution easier to see.

**Report (Second Round/ Revised Manuscript): “Development at the Point of a Bayonet?
Assessing the Timing and Origins of the Chilean Miracle”**

I appreciate the opportunity to review the revised version of this manuscript. The revision is stronger in several respects: the manuscript now uses more cautious language regarding what the Synthetic Control Method (SCM) identifies, acknowledges more explicitly the bundled nature of the treatment, and adds additional robustness exercises. These are meaningful improvements.

That said, while the paper has improved, I do not believe all central concerns have been fully resolved. My remaining comments focus on treatment timing, novelty relative to prior literature, anticipation effects, persistence of authoritarian legacies, and the limits of mechanism identification.

1. Novelty should be positioned more precisely

The revised manuscript improves its engagement with related literature, which I appreciate. However, the contribution still appears somewhat overstated. On page 5 authors claim: “*this paper also fills an important gap in the literature not merely by joining a debate, but rather by providing a credible forward-looking counterfactual that has been largely missing from the literature.*”

This is not accurate. There is already prior work applying SCM to Chile’s transition to democracy. For example, Uhr, Uhr, and Ely (2017) explicitly analyze Chile’s re-democratization using SCM, place treatment in 1988, and conclude that the transition had a positive long-run effect on GDP per capita. Escalante (2025) likewise applies a synthetic control to the 1988 transition and consider the mechanisms through which democracy might influence growth. These contributions are not acknowledged or briefly mentioned in the appendix or a footnote, but they deserve a more prominent place in the narrative. The authors should clarify how their study differs from these earlier analyses. Is the main contribution the choice of 1990 as the treatment year? Is it a methodological improvement? Or do they provide new insights into the channels through which Chile’s post-transition growth occurred?

2. Treatment timing (1988 versus 1990) remains a central unresolved issue

The manuscript now acknowledges that 1988 is a plausible alternative treatment date, but retains 1990 as the preferred intervention year. I continue to view this as a substantive methodological issue rather than a cosmetic choice. Authors seem to use the 1988 events to defend a 1990’s choosing, on page 19-20: “*Thus, our identification strategy relies on the unexpected nature and timing of the democratic transition, rather than claiming pure*

exogeneity. The above can be defended from the historical and political account provided in the Appendix in section 6, considering that the transition to democracy was mainly a citizens-driven process with an explicit and well-documented resistance from the dictatorial regime of Pinochet to leave power.”

The distinction matters because the two dates capture different phenomena:

- 1988 corresponds to the plebiscite rejecting the continuation of Pinochet’s rule. This was a discrete political shock that plausibly altered expectations, firm valuations, investment decisions, and the perceived future institutional environment. Immediate effects on the Chilean economy altering expectations and subsequent trajectory (See Gonzalez and Prem, 2018a, 2018b, 2020).
- 1990 corresponds to the formal transfer of executive authority, but many of the reforms emphasized in the manuscript were implemented gradually over subsequent years rather than as a sharp break at that moment.

Thus, 1988 may be the cleaner treatment date for a political transition shock, whereas 1990 is more naturally interpreted as the beginning of a gradual governing and policy process which invites additional concerns.

3. Uniqueness of the 1990s reforms

The previous concern relates to the manuscript’s discussion of post-1990 reforms, which is largely relegated to the appendix. The policies listed by the authors comprise a broad bundle of heterogeneous measures introduced over an extended period rather than a single coherent reform shock associated with 1990. These include expansions of targeted social spending during the 1990s, trade liberalization in the late 1990s and early 2000s, a free-floating exchange rate in 1999, competition law reforms in 1999 and 2004, and fiscal rule innovations beginning in 2000. Even where some institutional changes began earlier, many were phased in gradually over time.

This matters for identification. If the treatment year is 1990, but many of the policies invoked to explain subsequent growth occurred years later, then the paper risks conflating a discrete political transition with a prolonged sequence of reforms. In that case, the estimated post-1990 divergence is difficult to attribute specifically to democratization, to early transition policies, or to later institutional innovations or even different subsequent democratic administrations.

4. Persistence of authoritarian legacies deserves more attention

The paper frames the post-1990 period as a break from dictatorship, but the transition literature emphasizes important continuities.

Political influence, institutional constraints, inherited elites, and dictatorship-era appointments persisted into the democratic period. For example, research shows that mayors appointed during the Pinochet era enjoyed incumbency advantages in the first democratic local elections, allowing continued influence over spending and local governance (Gonzalez, Muñoz, and Prem 2021).

This matters for interpretation. If authoritarian political networks and institutional legacies remained influential after 1990, then the post-treatment period cannot be understood simply as “democracy replacing dictatorship.” It was instead a hybrid period combining democratic competition with inherited authoritarian structures.

I encourage the authors to incorporate this nuance more directly, as it bears on both treatment interpretation and mechanism claims.

5. Mechanisms remain underdeveloped relative to the strength of conclusions

The manuscript acknowledges that it cannot fully disentangle democracy from policy reforms. This is an improvement. However, the discussion still tends to imply a more singular mechanism than the design can support.

A post-treatment divergence in GDP could reflect several overlapping channels:

- enhanced credibility of property rights,
- smoother business-state coordination under democratic governments,
- continuation and refinement of prior market reforms,
- post-crisis rebound dynamics after the 1982 crisis,
- improved public goods provision,
- reduced social conflict,
- favorable external conditions.

These channels are not mutually exclusive. The paper would benefit from a fuller discussion of these possibilities and a clearer distinction between what the SCM results show (a divergence in outcomes) and what remains interpretive (the causes of that divergence).

6. Overall assessment

This is a stronger manuscript than the original submission. Nevertheless, important issues remain regarding treatment timing, novelty relative to prior SCM studies, anticipation effects, persistence of authoritarian legacies, and mechanism identification. These concerns appear addressable through further revision rather than requiring a fundamentally

new paper. Several key clarifications currently placed in the appendices, particularly regarding treatment timing and the distinction between political transition and later reforms, would be more effective if integrated into the main text.

References

Daniel A. P. Uhr & Julia G. Z. Uhr & Regis A. Ely, 2017. "A synthetic control approach on Chile's transition to democracy," *Economics Bulletin*, AccessEcon, vol. 37(3), pages 2219-2233.

Escalante, E. (2025). Por la razón o la fuerza: What caused Chile's economic miracle? In *The Pinochet Shock: Radical Economic Change and Life Under Dictatorship in Chile*, pages 259–286. Springer.

Gonzalez, Felipe, Pablo Munoz, and Mounu Prem. 2021. "Lost in Transition? The Persistence of Dictatorship Mayors." *Journal of Development Economics* 151.

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