University of St.Gallen
Swiss Institute for International Economics
and Applied Economic Research

Tax Management and Insurance Investments in Infrastructure: Changes in Municipal Bond Holdings Following the Tax Cuts and Jobs Act

by E. Tice Sirmans, Ruby Moor, Dylan Norris, Krzysztof M. Ostaszewski, and James Jones

Discussion by Anastasia Kartasheva

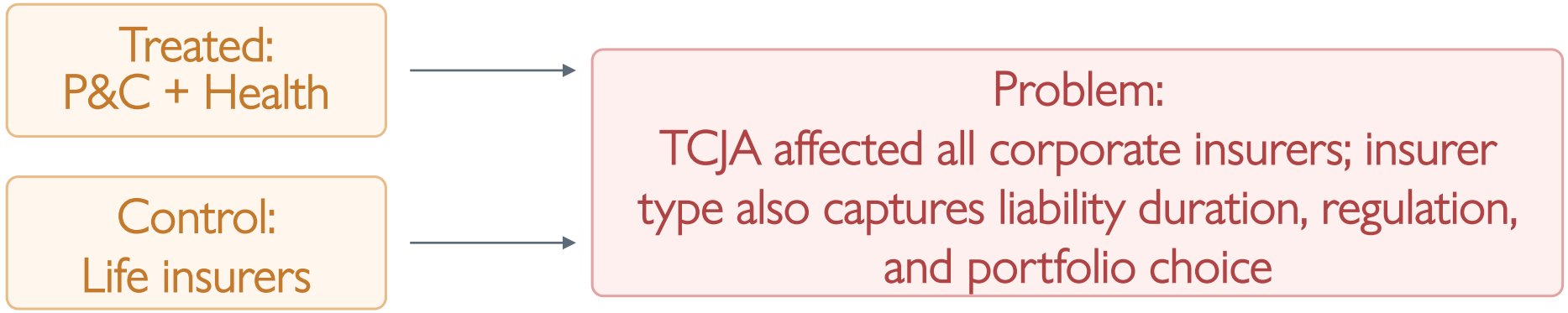
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From insight to impact.

Identification: improve the counterfactual

- Do life insurers provide the right unteated counterfactual?



- The coefficient currently measures the differential change between insurers' types after 2017
- The event study is useful, but insignificant pre-treatment coefficients do not establish the identifying assumption
  - P&C, health, and life insurers have very different liabilities and initial portfolios, and the post-period includes COVID and the sharp change in interest rates after 2021

Identification: improve the counterfactual

- Suggestion 1: Move from insurer type to a continuous, predetermined measure of tax exposure
  - Possible tax exposure measures: predicted marginal tax rates, taxable income, or reserve-related tax shield
  - The event study could ask whether insurers that experienced a larger decline in the value of the tax exemption reduced municipal holdings more strongly
  - Insurer type would then become an economically interesting heterogeneity test rather than the only source of identification
- Suggestion 2: Exploit the security-level Schedule D data
  - Compare tax-exempt and taxable bonds across insurers with different pre-reform tax exposure, using insurer-by-year and bond-by-year fixed effects
  - Taxable municipal bonds may offer a particularly useful comparison because they allow to hold the municipal issuer sector more nearly constant

Measurement & implementation: match variables to mechanism



- The current outcome includes **all municipal bonds**, but **not every municipal bond is federally tax-exempt**
  - Suggestion 3: The paper should identify the actual tax status of each CUSIP
- Changes in portfolio shares need not represent active trading
  - they can reflect maturities or changes in the denominator
  - Suggestion 4: Net acquisitions, disposals, and changes in par value scaled by lagged assets would map much more directly into portfolio reallocation

- The data identify municipal issuers, but they do not identify infrastructure investment
  - Unless the authors add use-of-proceeds information, I would frame the conclusion as a result about municipal-bond demand rather than infrastructure financing

Specification and Inference

- The standard errors do not account explicitly for serial correlation or common decisions within insurance groups. The analysis should cluster at the insurance-group level and use inference appropriate to the actual source of policy variation
- Avoid potentially post-treatment controls
- Use external market yields
- Make 2018 the first treatment year
- Use the full unbalanced sample including insurers with zero municipal holdings as the baseline
- Equation 1 includes insurer and year fixed effects, but the three interest-rate controls are each constructed as a single national annual average. As a result, these controls are perfectly collinear with the year fixed effects, and the treatment and post indicators are likewise absorbed by the insurer and year effects. Because Tables 7–9 report coefficients for the annual rates in the controls-and-fixed-effects specifications, the exact estimating equation requires clarification
- Clarifying the sample construction will help readers understand the exact estimand. Section 3.1 describes 2,974 insurers observed throughout all 14 years, which implies 41,636 total observations rather than the reported 37,759. In Table 5, Panel B reports a treated-group pre-policy count of 24,716, which exceeds the entire pre-period count of 21,598 shown in Panel A
- The dependent variable is calculated as municipal bonds divided by total bonds. A lower share does not automatically establish reduced municipal-bond dollar holdings, net selling, or lower market demand. Indeed, Table 5 shows the full-sample mean municipal holdings are nearly unchanged before and after 2017 ($280.4 million versus $281.4 million). Furthermore, Table 9 finds no significant increase in corporate-bond shares within the zero-inclusive full sample, which complicates the narrative of a reallocation toward corporate securities.
- The economic translations also rely on control baselines that require verification. Section 4.A utilizes the 39% full-sample pre-period municipal share rather than the life-insurer baseline, while Section 4.B uses 19% and 41%, even though Table 4 reports life-insurer government and corporate means of 10.7% and 66.5%. Because the empirical results center on portfolio shares rather than dollar purchases, issuance, financing costs, or physical infrastructure spending, framing the infrastructure effects in Section 5 strictly as possible downstream implications—rather than estimated consequences—will ensure the conclusions remain aligned with the empirical findings.