Discussion of

"Assessing the Effects of Recent Provisioning Rules on Consumer Credit Allocation in Colombia"

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The views expressed are my own and do not necessarily reflect those of Banco de México.

Summary: big picture

- **Context:** post-pandemic boom in consumer credit (2021–22), followed by a deterioration in portfolio quality.
- **Intervention (Jan 2023):** higher provisions on new consumer loans

$$K = \begin{cases} 1 & m \leq 72 \\ 1.1 & 72 < m \leq 108 \\ 1.4 & m > 108 \end{cases}$$

- **Question:** effect on (i) provisions and (ii) credit conditions: rate, amount, LTV.
- **Overall:** relevant policy question, excellent granular data, carefully executed.

Summary: methodology

- **Challenge:** loans and borrowers differ sharply across maturities.
  - E.g. payroll loans: 6% of control vs. 74% of loans > 108 months.
- **Solution:** two steps
  1. **Exact matching** on quarter, bank, sub-segment, rating, PD, LGD, borrower sector, ...
  2. **Weighted DiD** with quarter and bank FE:

$$Y_{ift} = \alpha + \mu\, T_i + \beta\,(T_i \times Post_t) + \sigma_t + \delta_f + \Gamma X_{ft} + e_{ift}$$

- **Heterogeneity:** triple interaction with large-bank dummy ($> 10\%$ market share).

Findings:

- Coverage ratio $\uparrow \Rightarrow$ resilience objective met.
- No average effect on rates, amounts or LTV.
- Small lenders cut amounts ($\approx -21\%$) and LTV (≈ -9 pp) for $m > 108$.

Comment 1: Demand may have shifted at the same time

- **Key assumption:** without the policy, long and short loans would have moved in parallel.
- Flat pre-trends do not rule out a demand shift -unrelated to the policy **starting in 2023**.
- This could **hide** a supply contraction in long maturity loans (↑ demand, ↓ supply).

Suggestion: placebo → rerun the main specification on **payroll loans to pensioners**.

- The rule does not apply to them, but they face the same (potential) demand shift.
- No demand shift ⇒ effect should be **zero**.

Long vs. short loans, after 2023	Change	Captures
Non-pensioners	0%	policy + demand
Pensioners	+10%	demand
Difference	−10%	policy

Placebo assumption: the demand shift is similar for pensioners and non-pensioners.

Comment 2: The policy may act on quantities, not terms

- Paper compares **similar** loans: no effect on loan size, rate or LTV ⇒ "no effect on credit supply".
- But effects may appear in the **composition** of new lending:
  1. Long maturity loans look similar to short maturity loans, yet their **total volume** may fall.
  2. Risk is now more heavily penalized ⇒ **riskier** long loans may shrink as a share of lending.

Simple solution: show number and **volume** of new loans, pre vs. post, by maturity, risk (rating, PD) and sub-segment.

A fuller design (borrower level) in Comment 3.

Comment 3: Borrower-level specification

- **Suggestion:** alternative specification at the borrower level.
- **Idea:**
  1. Pre-policy **bank** exposure: share of new loans $>$ 108 months in 2022.
  2. **Borrower** exposure: weighted average of bank exposure across their banks (2022 credit shares).
  3. Estimate effect of borrower exposure on credit growth, average rate and average LTV.
- **Suggested design:**

$$\text{Exposure}_i = \sum_b w_{ib,2022}\, \text{LongShare}_{b,2022}$$

$$y_{it} = \beta\,(\text{Exposure}_i \times \text{Post}_t) + \alpha_i + \gamma_{g(i),t} + \varepsilon_{it}$$

- $\gamma_{g(i),t}$: cell $\times$ quarter FE (sector, rating, sub-segment, collateral) $\Rightarrow$ absorb demand.
- Midpoint growth $\in [-2, 2]$ includes borrowers who lose all credit $\Rightarrow$ captures the **extensive margin**.

Comment 4: Control loans may also be treated

- Maturity is chosen **after** the rule is known: loans can be shortened to 72 or 108 to avoid K.
- Then the control group absorbs **moved loans**, and both groups change composition.
- Paper states the maturity distribution did not change, but more evidence is needed (number, volume, or by month).

Suggestion: compare neighbors, **excluding** loans right at each cutoff.

Cutoff	Control	Excluded	Treated
72 months	67–69	70–75	76–78
108 months	103–105	106–111	112–114

- Check **composition** near cutoffs, pre vs. post (payroll share, rating, PD).
- Report several exclusion and window widths, as in an RD.

Minor comments

- **Coverage effect is smaller than mechanical.**
  - Pure $\times 1.1$ / $\times 1.4$ would imply ≈ 0.5 pp and ≈ 4.2 pp.
  - Estimates: 0.17 pp and 0.86 pp. Internal models? K not fully binding?
- **Bad controls:** EM2/EM3 match on other contract terms (e.g. rate when studying amounts).
  - If the policy moves terms jointly, this absorbs part of the effect $\Rightarrow$ bias toward zero.
  - Match only on pre-determined characteristics; treat EM1 as main specification.
- **Inference:** 31 clusters and only 3 "large" banks $\Rightarrow$ wild-cluster bootstrap.
- **Small samples:** LTV regressions for > 108 months use 2,064 obs. (EM3).
- **Timing:** announcement Nov 29, 2022 falls in reference quarter Q4 2022 $\Rightarrow$ anticipation biases toward zero. Use Q3 2022 as baseline or drop Dec 2022.