When Churches Split: Religious Governance, Congregational Exit, and Local Political Change

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Abstract

Denominations govern: they allocate authority over doctrine, property and personnel, and they set the price a congregation pays to leave. I study the schism in the Presbyterian Church (U.S.A.) around the 2011 ratification of Amendment 10-A, which removed denomination-wide barriers to ordaining non-celibate gay and lesbian clergy. I combine PC(USA) congregation-year statistics, General Assembly dismissal records, presbytery exit policies, realized dismissal payments, minister and lay-leadership data, successor-denomination records, and county election returns. I find three things. First, counties on the pro-10-A side of close presbytery borders become less Republican and less electorally extreme over the medium run, most clearly where PC(USA) had a baseline presence. The 2012 effect is small and the 2016–2024 divergence larger. Second, exit depended on outside options, leadership and property rules, not on doctrinal disagreement alone: anti-10-A environments saw more departures, but female pastoral and lay leadership sharply reduced them among conservative congregations. Third, schism reallocated religious capital more than it destroyed it. Departing congregations were larger and more financially intensive than those that stayed, payments to leave scale almost one-for-one with membership, and where I can follow congregations into EPC records their giving largely continues outside PC(USA).

Conflicts of Interest The author declares having no competing financial interests. However, the author is a ruling elder in the Presbyterian Church of the United States of America and his parents are teaching elders in the denomination. Despite these personal connections, the author has received no financial support from any religious traditions to complete this research.

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1 Introduction

A congregation that splits from its denomination is doing something expensive. It argues over who owns the building, over what it owes the body that ordained its pastors, and over which of its members will follow. And it does this in public, in a town where its members are neighbors. The arguments start in theology and end in property law. What they leave behind is a damaged institution in a place that had few others like it: a congregation is not only a building but one of the last settings where people who disagree still see each other weekly. Americans increasingly choose a congregation for its politics rather than its doctrine, so a schism sorts a community as much as it divides a church. Whether that reaches past the congregation and into the surrounding town is not obvious, and it is the question this paper starts from.

I study these terms in the Presbyterian Church (U.S.A.), which spent the 2000s and 2010s in open conflict over the ordination and marriage of LGBTQ members. I leverage cross-border variation in presbytery votes to approve Amendment 10-A to the Book of Order, which removed the denomination-wide language barring partnered gay and lesbian clergy from ordination. On May 10, 2011, the Presbytery of the Twin Cities Area became the 87th of 173 presbyteries to approve the amendment providing the simple majority needed for passage. 10-A took effect in July 2011, as hundreds of congregations left in the years around that decision, most of them for the Evangelical Presbyterian Church (EPC) or for ECO: A Covenant Order of Evangelical Presbyterians, founded in 2012.

I ask three questions. First, does a denominational schism change local politics outside the denomination? Second, what determines which congregations leave: conviction, leadership, or the availability of somewhere to go? Third, what does leaving cost, and where does the money go afterward?

The episode is useful because it generated variation at three levels that are usually collapsed. Presbyteries voted differently on 10-A, so nearby counties on either side of a presbytery border faced different denominational environments. Congregations differ in theology, leadership, size, and finances. Presbyteries differ in property-rule defaults and in how much discretion they exercised over dismissal terms. States differ in how much deference they pay to denominational hierarchy. Those margins let me separate the desire to leave from the cost of leaving, and both from where congregations actually ended up.

I find three things. First, schism has local political spillovers. In matched counties straddling 10-A presbytery boundaries, the pro-10-A side becomes less Republican and less electorally extreme over the medium run. The 2012 difference is small and fragile. The 2016–2024 divergence is larger and survives the same-state and state-year checks. The effect is too large to come from PC(USA) members changing their own votes (the denomination is simply not big enough in these counties), so I read it as a community-level spillover rather than member persuasion.

Second, exit is not a mechanical response to disagreement. Theologically conservative congregations are far more likely to leave, but leadership and outside options matter as much. Female pastoral leadership, and independently measured female lay leadership, sharply reduce departure among conservative congregations. ECO's founding in 2012 changed the destination set facing those churches, while the pre-existing EPC and PCA options offered different routes out.

Third, schism reallocates religious capital rather than destroying it. Departing congregations are larger and more financially intensive than those that stay. Realized dismissal payments scale almost one-for-one with membership and are shaped by property-rule defaults and by how much discretion the presbytery holds. Denominational records treat a departure as a loss, but adding back the post-departure EPC giving I can recover cuts the relationship between departure rates and local giving losses by roughly two-fifths.

Two limitations shape how far these results travel. The first is that I can follow congregations after departure into only one successor denomination. EPC records let me track giving and membership for congregations that went there. ECO and PCA destinations are observed at the point of departure and not afterward. The reallocation result is therefore a lower bound on how much money crosses denominational boundaries rather than an estimate of how much survives, and I say "untracked" rather than "lost" throughout.

The second is that a schism is not randomly assigned. Presbyteries that voted for 10-A differ from those that voted against it in ways I cannot fully observe, and departures are themselves an outcome. I lean on the border design, which compares adjacent counties facing different denominational environments, and I report the same-state and state-year restrictions that discipline it. I treat the political estimates as the most fragile results in the paper and label them that way where they appear. Figure 1 maps the matched counties and how each side's presbytery voted.

Table 3 compares the two sides on observables in 2008, the omitted year and the last election before the amendment. Most of what a reader would want held constant is: household income, poverty, the Republican vote share, votes cast per resident, net migration, and the adherent shares of the three largest religious traditions are all statistically indistinguishable across the boundary. Three things are not, and they are the ones worth stating plainly. Counties on the voted-yes side are larger, about 87,000 residents against 58,000, which is the urbanisation gap that also shows up when the county membership series is decomposed. They have fewer PC(USA) members per resident. And they contain more EPC congregations before the schism, which is a pre-existing difference in exactly the denominational environment the design is trying to use. I read the political estimates in light of these three and not as though the boundary were random.

Some further description locates the denomination and the variation the paper uses. 97 presbyteries voted for Amendment 10-A and 74 against, which is the variation behind every border estimate. 1,094 congregations are identified with the Confessing Church movement. The 985 of them reporting membership in 2010 account for 267,017 members, with a median congregation of 134. Women hold office widely: across congregations reporting their sessions the mean share of women elders is 53.0% (8,457 congregations) and among deacons 73.6% (4,857), while 2,978 of the 9,768 congregations whose pastor's gender is known have a woman in the pulpit, 30.5%.[1]

The dismissal policies themselves are the instrument the presbyteries actually wrote, and Table 1 shows what they say about property. I collected 267 policy documents from 137 presbyteries (presbyteries revise, so the counts below are of documents). The striking feature

[1]Pastor gender is taken from the record where stated and inferred from first names otherwise, so it is reported over congregations where it is known rather than over all congregations. Those with no pastor recorded, or whose pastor's name is not gender-distinctive, are excluded rather than assigned.

is how little most of them commit to: the largest category specifies no property default at all, and the next largest leaves the terms to be negotiated. A congregation reading its presbytery's policy before deciding whether to seek dismissal usually could not learn from it what it would have to pay.

Property-retention default	Documents	%
Not specified	119	44.6
Negotiated case by case	84	31.5
Explicitly case by case	35	13.1
Congregation retains property	20	7.5
Presbytery retains property	6	2.2
Unclear	3	1.1
Total	267	100.0

Table 1: What presbytery dismissal policies say about property, across 267 documents from 137 presbyteries. Counts are of documents rather than presbyteries because presbyteries revise their policies.

Table 2 describes a congregation on each side of the boundary the paper follows congregations across. The median PC(USA) congregation is smaller than the median EPC or PCA one, which matters for reading the departure estimates: the congregations that left were not a random draw from the denomination. The columns are not definitionally identical, since each body reports its finances under its own accounting rules, and that is the same break the within-congregation estimates cross at departure rather than a defect of the table.

I draw on four literatures. The first is the economics of religious organization. Economic models of religion emphasize religious groups as producers of club goods that use doctrine, norms, and participation costs to shape commitment and reduce free riding [Iannaccone, 1992, 1998, Iyer, 2016, Carvalho, 2019, Carvalho and Sacks, 2021]. In that tradition, schism and switching are organizational responses to mismatch, authority, and the cost of remaining inside a group [Hirschman, 1970, Beard et al., 2014]. The closest historical evidence on American Protestant denominations treats schism as an organizational process shaped by denomination structure and prior consolidation [Sutton and Chaves, 2004]. I bring that logic to a modern congregation-level setting in which congregations can be followed before exit, through bargaining over dismissal terms, and, for one successor denomination, after exit.

The second literature studies religious sorting and the alignment of religion and politics. Politics is linked to religious disaffiliation, switching, and identity-inconsistent attendance [Hout and Fischer, 2002, Djupe et al., 2018, 2024]. Recent work also emphasizes the thinning of politically moderate congregational spaces and the tighter alignment of religious identity with partisan identity since the late twentieth century [Burge, 2026]. I study that alignment at the organizational margin: not only whether individuals sort across affiliations, but whether conflict inside a denomination reallocates whole congregations, their leadership structures, and their financial capacity.

Third, the paper contributes to work on religious institutions as local economic actors. Churches provide services, receive charitable contributions, and interact with government

	PC(USA)	EPC	PCA	ECO
Congregations	11,178	195‡	684	454†
Received from PC(USA), 2007–2024	—	572	11	350
share of congregations ever observed (%)	—	61	0.9	77
Communicant members (median)	120	135	164	—
congregations reporting	11,178	177	685	—
Total annual income ($) (median)	75,869	333,885	236,205	—
congregations reporting	11,170	165	683	—
Total annual expenditure ($) (median)	—	335,940	207,559	—
congregations reporting	—	164	684	—
Mission share of spending (%) (median)	13.2	9.9	—	—
congregations reporting	10,047	164	—	—

Table 2: Congregations by denomination in 2000, before any of the treatment era. Medians, with the number of congregations reporting each item beneath. Finance rows are not definitionally identical across bodies: PC(USA) income at 2000 is total contributions (the 2000 statistical vintage reports no total-expenditure aggregate), the EPC reports total receipts and disbursements, the PCA total church income and grand total disbursements. ‡: the parsed EPC statistics begin in 2006, so its column is that year. †: ECO's count comes from the denomination's own church directory rather than an annual statistic (ECO was not founded until 2012), and that count is all that is known about it. ECO publishes no congregational statistics by polity. The received-from-PC(USA) row counts announced destinations in the departure census. Shares are of each denomination's congregations ever observed in its own records (942 EPC filers over 2006–2024, ECO's 454-church directory, 1,272 PCA filers over 2000–2016).

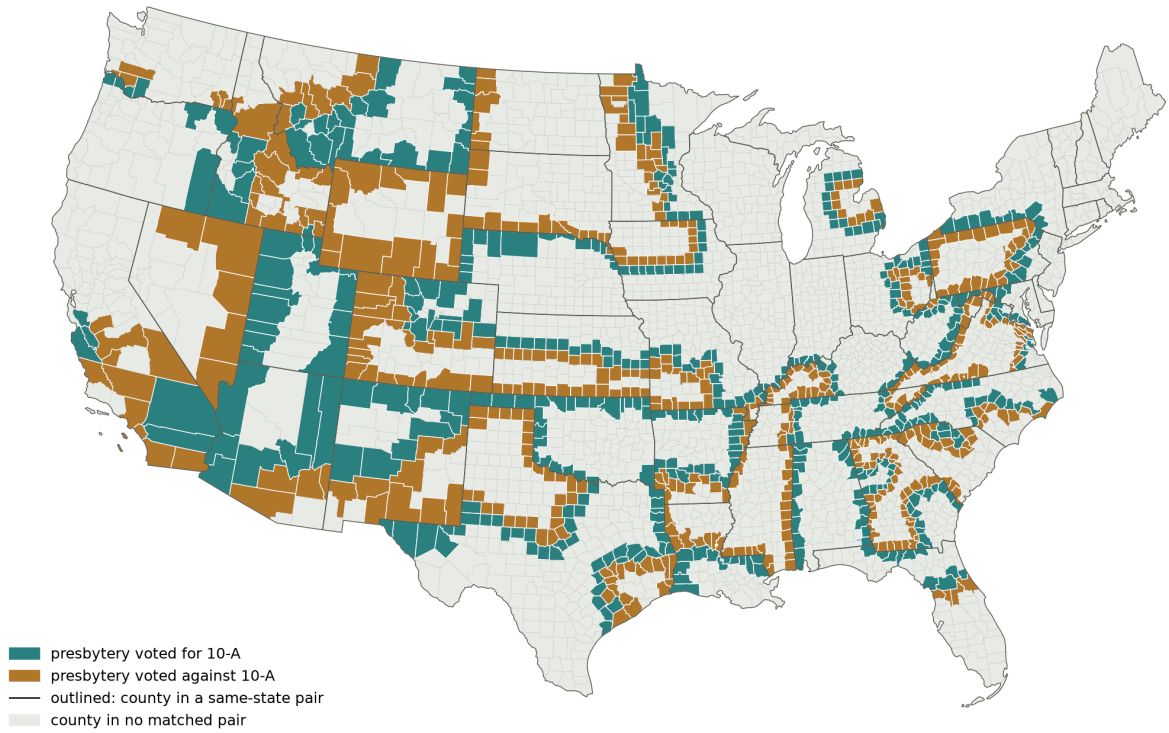


Figure 1: Counties in the matched presbytery-border comparison, shaded by how their presbytery voted on Amendment 10-A. Adjacent counties of different shades are the comparison: each pair straddles a presbytery boundary and so faces a different denominational environment on either side. Counties in same-state pairs are outlined, since the same-state restriction is the paper's main robustness cut. 994 matched pairs across 972 counties and 153 boundary segments; 475 counties sit in presbyteries that voted for 10-A and 497 in presbyteries that voted against, and 543 counties fall in a same-state pair.

	Voted no	Voted yes	Difference	p
Population	88,345	89,588	1,242	0.961
Median household income ($)	42,462	42,973	511	0.465
Poverty rate (%)	15.9	15.9	-0.0	0.923
Two-party Republican share, 2008	0.606	0.604	-0.001	0.866
Votes cast per resident, 2008	0.437	0.446	0.009	0.113
Net migration rate	0.0026	0.0016	-0.0010	0.193
PC(USA) members per 1,000 residents	7.37	5.96	-1.41	0.016
PC(USA) congregations	3.33	2.99	-0.34	0.437
EPC congregations	0.04	0.10	0.06	0.030
PCA adherents (2010)	91	103	12	0.715
Evangelical adherents per 1,000 (2010)	253.6	252.5	-1.1	0.918
Mainline adherents per 1,000 (2010)	110.5	101.1	-9.4	0.111
Catholic adherents per 1,000 (2010)	114.5	111.8	-2.7	0.748
Counties	497	475		

Table 3: Matched border counties in 2008, by how their presbytery voted on Amendment 10-A. Means, with the difference and an unequal-variance t-test. PCA adherents come from the decennial religion census and are reported at 2010, the earliest vintage in hand. ECO carries no row because it was not founded until 2012. Denominational counts are zero where a body has no presence, not missing.

and market institutions [Hungerman, 2005, Gruber and Hungerman, 2007, Andreoni et al., 2016, Ferrara and Testa, 2023]. This setting makes it possible to distinguish a loss to one denomination from a loss to the local religious economy. That distinction matters because denominational records mechanically treat a departing congregation as a loss, even when its members and giving continue in a successor body.

Finally, the county-level analysis speaks to work on religion and political behavior. Prior work links religious participation to turnout and preferences over redistribution or social insurance [Gerber et al., 2016, Scheve, 2006], while the broader polarization literature shows that partisan identity has become increasingly socially salient [Iyengar et al., 2019]. I treat the political results cautiously. A denominational schism is not a randomly assigned shock to local politics, and observed departures are themselves outcomes of local religious sorting. The empirical design therefore separates congregation-level sorting from county-level political change, uses real presbytery borders rather than generic denomination-share exposure, and treats pre-trend and sensitivity analysis as part of the main evidence [Roth, 2022, Rambachan and Roth, 2023].

2 Data and design

Presbyterian denominations count themselves carefully. Every congregation files an annual statistical report covering membership, receipts, and expenditures broken into local program, capital, per-capita apportionment and several mission categories, and the denomination keys

each one to a presbytery-assigned identifier that follows the congregation for as long as it exists. That identifier is what makes this project possible.

The PC(USA), EPC, and PCA each post these annual statistics in some public format dating back to 1983, 1981, and 2000, respectively. ECO is the exception to this rule among major Presbyterian denominations in the United States. As a matter of denominational polity to counter the property trust clause in the PC(USA), all property, including statistical records, belong entirely to the congregation and shared with the denomination only to the extent necessary to budget.[2]

I build a congregation-year panel for finance and membership: 12,982 PC(USA) congregations ever observed over 1983–2021, 942 EPC congregations over 2006–2024, and 1,272 PCA congregations over 2000–2016. (These are panel totals. Table 2 snapshots a single year, which is why its counts are smaller.)

Knowing who leaves the PC(USA) is harder than counting who is present, because the denomination's own records disagree with each other. I classify every congregation into one of seven outcomes (dismissed, split, informally split, dissolved, reconciled, stayer, or a likely unrecorded departure) from three independent sources: the PC(USA)'s native dismissal records, the General Assembly's published dismissal statistics, and a database of discernment votes and terms compiled by the Presbyterian Layman Committee through 2017. Where they disagree I apply an explicit source-priority rule, so a more fine-grained categorization is never silently coarsened by a less specific one. That yields 938 confirmed leavers (370 dismissed, 555 informal splits, 11 recorded splits, and 2 likely unrecorded departures), 7.2% of the 12,982 congregations ever observed.

A label is not the same as an event, and for the event studies in Section 7 I classify congregations by what their departure actually did rather than by what it was called. A congregation that left fully stops appearing in the PC(USA)'s books. A congregation that split leaves a remnant that keeps filing. That distinction cannot be read off the status label (of the 555 congregations recorded as informal splits, 264 appear in the receiving denomination's own statistics and only 86 leave a continuing remnant), and treating the label as though it carried the distinction would put departed congregations in a comparison group.

Following congregations after they leave requires their new denomination's records.[3] I match 409 PC(USA) identifiers to real EPC congregation records (104 confirmed by hand review, 272 matched automatically and accepted, 33 flagged and included regardless), and 408 of them carry usable post-departure finance data. A further 42 congregations were examined and found genuinely absent from EPC statistics, which I record as a finding rather than leaving them indistinguishable from cases nobody looked at. Matching now reaches 403 of the 413 congregations whose recorded destination is the EPC, so the matched set is very nearly the population rather than a selected slice of it.

[2]One percent of each congregation's budget goes toward national denomination activities.

[3]The two other destinations are not equivalent obstacles. ECO, which received 391 of the 936 leavers, publishes no congregational statistics by polity at all, so those congregations cannot be followed by any route short of collecting their own filings. The PCA does publish annual congregational statistics, keyed by an organization identifier and a statistical year, and is tractable in the way ECO is not, but only 12 PC(USA) congregations went there. I confirm nine of those cases against the PCA statistics and extract membership and finance for six, covering 2005–2025. The binding limitation on Sections 5 through 8 is therefore ECO, not the PCA.

The political design rests on presbytery borders. Presbyteries voted separately on Amendment 10-A in 2010–11, and I take every pair of presbyteries that physically touch and voted opposite ways, then match the real touching county pairs across each of those boundaries. That gives 994 matched county pairs across 153 boundary segments. The default specification throughout is border-pair × year fixed effects with county fixed effects, standard errors clustered on the matched pair, so identification comes from counties adjacent to one another across a denominational boundary rather than from comparisons between distant places.

For outcomes I use presidential county returns from Amlani and Algara [2021] for 1868–2020 combined with the MIT Election Lab for 2024, Census county population across three vintage files for 1980–2024, and IRS county migration for 1991–2023 (county-to-county detail from 2007). Cross-sectional outcomes (CDC PLACES for health, the Opportunity Insights Social Capital Atlas, and the Stanford Education Data Archive) enter as single-vintage measures, which limits them to level comparisons rather than event studies.

Two things this design cannot do, both of which bound what follows. It cannot track congregations that left for anywhere other than EPC and PCA, so the post-departure results describe roughly half of the departures rather than all of them. And it cannot separate the effect of a presbytery's vote from whatever made the presbytery vote that way: 10-A stances were not assigned at random, and congregations may differ across boundaries for reasons the vote also reflects. I treat pre-trend and sensitivity analysis as part of the main evidence rather than as an appendix [Roth, 2022, Rambachan and Roth, 2023], and I report the specifications that fail those checks alongside the ones that pass.

3 Local effects of schism

I ask whether county political behavior moved differently after Amendment 10-A on the pro-10-A side of a presbytery boundary than on the anti-10-A side. I use 994 matched county pairs across 153 presbytery-boundary segments. My default specification includes county fixed effects and matched-pair-by-year fixed effects, so identification comes from nearby counties that share the same local comparison in a given election year but sit on opposite sides of an ecclesiastical boundary. Formally, for county c in matched pair $b(c)$, whose presbytery $p(c)$ voted on 10-A, I estimate

$$y_{ct} \;=\; \sum_{\substack{\tau=1980\\ \tau\neq 2008}}^{2024} \beta_\tau \, \mathbf{1}\{t=\tau\} \cdot \mathrm{Yes}_{p(c)} \;+\; \mu_c \;+\; \lambda_{b(c),t} \;+\; \varepsilon_{ct}, \tag{1}$$

where y_{ct} is the political outcome in county c in election year t, $\mathrm{Yes}_{p(c)}$ equals one if the county's presbytery approved 10-A, μ_c is a county fixed effect, and $\lambda_{b(c),t}$ is a matched-pair-by-year fixed effect, so each β_τ is identified from within-pair differences in year τ. The omitted year is 2008, the last presidential election before the General Assembly sent 10-A to the presbyteries. Because a county can sit in several matched pairs, the panel stacks pair memberships and I cluster standard errors on the matched pair. The pre-period coefficients $\beta_{1980}, \ldots, \beta_{2004}$ are the pre-trend evidence, and I test them jointly rather than eyeballing them.

Before asking about elections, I verify the first stage: the religious landscape itself diverges at these borders after 2011. I estimate equation (1) by Poisson pseudo-maximum likelihood on the count of congregations per 100,000 residents, by denomination (Figure 2). EPC congregations appear on the anti-10-A side after ratification, PC(USA) counts hold up slightly better on the pro-10-A side, and the PCA does not move at all. Figure 3 shows the matching membership margin: PC(USA) members per 1,000 residents rise on the pro-10-A side relative to the anti-10-A side, reaching about one member per 1,000 by 2021, while EPC membership grows where the presbytery voted no and PCA membership is flat everywhere.

Two things keep that relative PC(USA) gain from being misread. It is a relative statement inside an absolute collapse: the denomination shrank on both sides of every border, and the pro-10-A side simply shrank more slowly, as fewer of its congregations left. And it is not a denominator artifact. If pro-10-A counties were losing population, members per resident could rise with no one joining anything, but the migration results below show the opposite: pro-10-A counties retained people after ratification, so a growing denominator makes the per-resident membership gain an understatement, not an illusion. The political estimates that follow should be read against this first stage: the borders are not lines on a map, they mark places whose religious institutions actually moved.

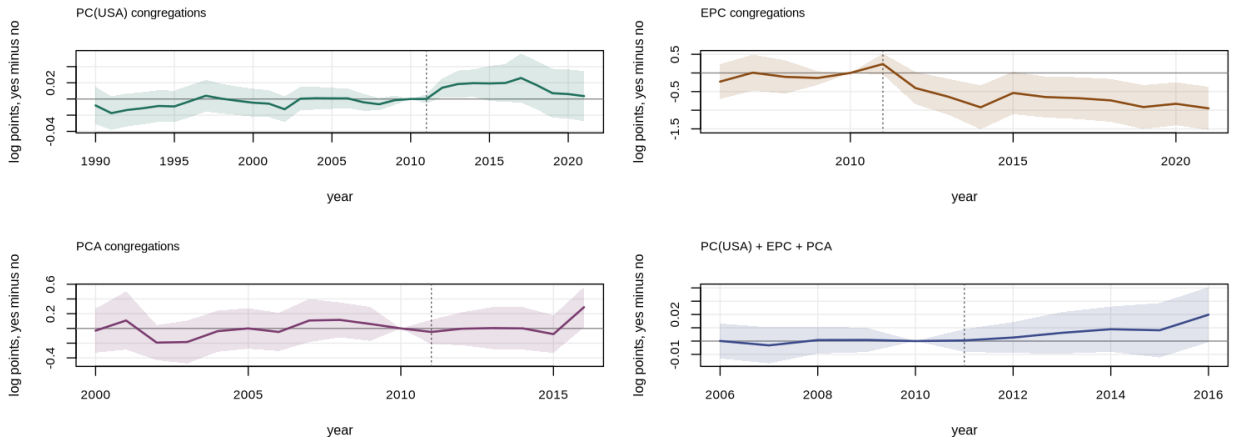


Figure 2: The first stage at the extensive margin: congregations per 100,000 residents by denomination, Poisson event studies across the matched borders (736–741 border counties per panel). A congregation closing, leaving, or arriving is the local institution appearing or disappearing.

The main result is a medium-run divergence in partisan and political composition. In 2012, the pro-10-A side is only slightly less Republican: the estimated effect on two-party Republican share is -0.0017 with a confidence interval that just includes zero. By 2016, however, the gap is about one percentage point, and it remains similar in 2020 and 2024: -0.010, -0.011, and -0.012, respectively. The effect on electoral extremity (the absolute distance of the two-party Republican share from 50/50) follows the same medium-run pattern. It is essentially zero in 2012 (-0.0000), then becomes about one percentage point lower on the pro-10-A side in 2016, 2020, and 2024. Thus the strongest political finding is not an immediate 2012 reaction to the denominational vote. It is a persistent post-schism differ-

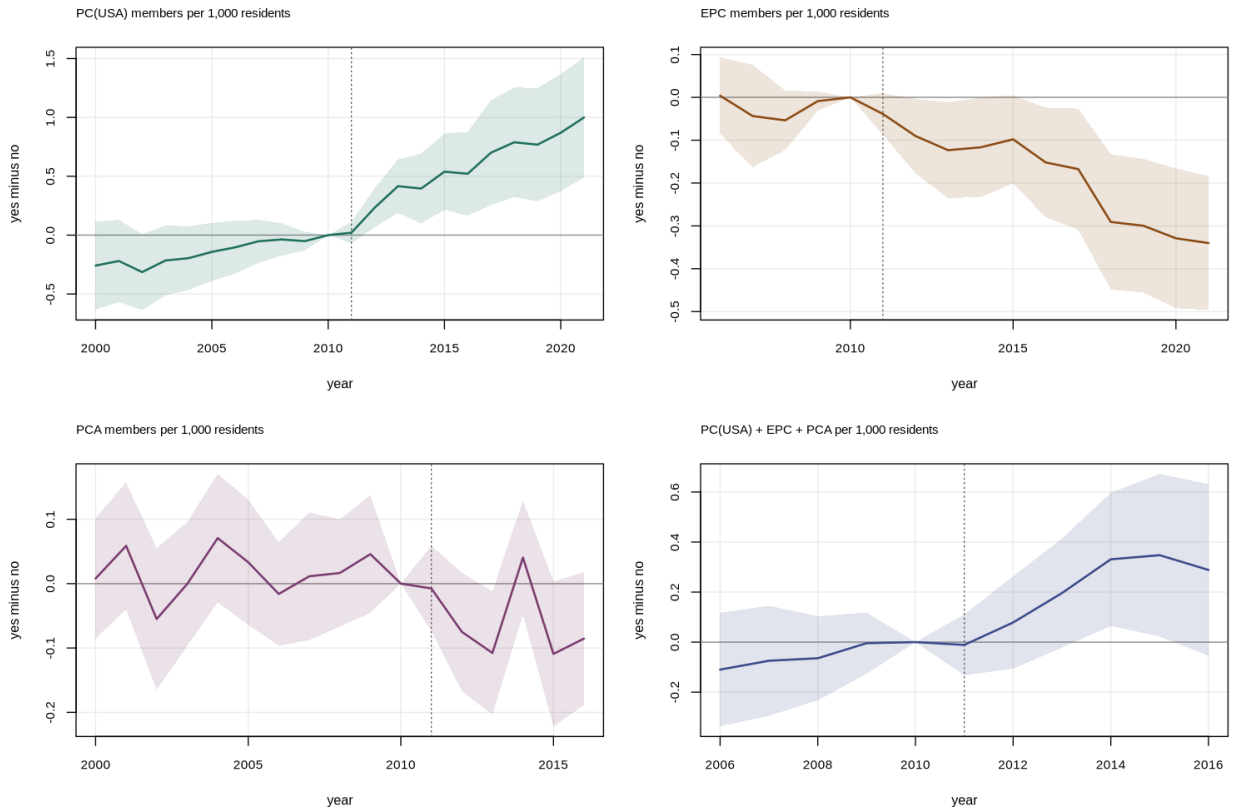


Figure 3: The first stage at the membership margin: members per 1,000 residents by denomination, reference year 2010, the last fully untreated year (10-A completed ratification in May 2011). Each series runs over its own records: PC(USA) 2000–2021, EPC 2006–2021, PCA 2000–2016, and the combined panel over 2006–2016, the years all three report. Between 725 and 736 border counties enter each panel.

ence in which pro-10-A places become less Republican and less electorally extreme relative to otherwise close anti-10-A comparison places.

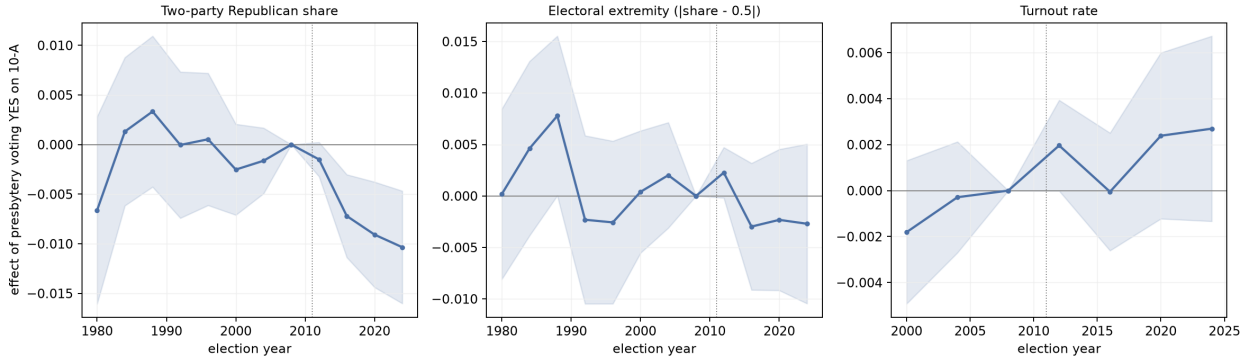


Figure 4: Presidential outcomes around Amendment 10-A, same-state matched pairs. Both counties of every pair share a state, so state-by-year shocks difference out by construction. This is the specification the design defends best, and the full-sample and cross-state versions are in Appendix Figure 11. Two-party Republican share, electoral extremity, and turnout rate, with 2008 omitted and 10-A's 2011 effective date marked. The turnout panel is the weak one, discussed below.

The turnout result is weaker and should be treated separately. Once turnout is normalized by county population rather than measured as raw votes, the immediate 2012 effect is negative and significant (−0.0020), but it fades by 2020–2024. The raw vote-count version is not a trustworthy outcome because county population growth differs across the boundary. It mechanically imports urban/rural growth differences into the event study.

Several checks discipline the political interpretation. Run through 2024 rather than only at the first post-election, they support the medium-run partisan result strongly. The 2016–2024 GOP-share divergence is essentially unchanged across every variant tried: restricting to same-state county pairs gives −0.0072, −0.0091, and −0.0103 in 2016, 2020, and 2024; restricting to cross-state pairs gives −0.0107, −0.0116, and −0.0122; and adding state-by-year fixed effects on top of the border-pair design gives −0.0073, −0.0097, and −0.0108 – all statistically significant, and all within a whisker of the baseline −0.0088, −0.0103, and −0.0112. The tighter specifications also sharpen the pre-trend argument rather than weakening it: in the same-state and state-year-FE variants, the pre-2008 coefficients drift mildly *positive* (the pro-10-A side trending more Republican before the schism), so a continuation of pre-existing trends would predict the wrong sign for the post-2011 divergence. Electoral extremity survives the same battery but shrinks by roughly a quarter under state-year fixed effects (−0.0097 versus −0.0079 at 2024), consistent with part of the extremity result being absorbed by state-level electoral dynamics. It should be read as a secondary result. Turnout does not survive: both the same-state restriction and state-year fixed effects eliminate it at every horizon, so the turnout finding is reported for completeness but carries no weight in the interpretation. Leave-one-boundary checks confirm no single boundary segment drives the medium-run result: dropping each of the 153 presbytery-boundary segments in turn never flips the sign of the 2016–2024 GOP or extremity estimates, and the 2024 GOP estimate stays within a narrow −0.0120 to −0.0102 band (the only sign flips anywhere in this exercise

are in extremity's 2012 coefficient, which is approximately zero in the full sample to begin with and flips in about half the omissions, as a zero should). Finally, HonestDiD sensitivity exercises indicate modest rather than spectacular robustness at the long horizon: the 2024 GOP result survives allowing parallel-trends violations up to roughly 40% of the largest observed pre-period deviation, and the 2024 extremity result up to roughly 20% [Rambachan and Roth, 2023]. The full robustness battery is presented in Appendix Table 6.

The political result is also concentrated where PC(USA) was actually present and, importantly, absent where it was not. Using baseline (2008) PC(USA) membership geocoded to counties, counties with no PC(USA) congregation at all show no GOP movement at any horizon (0.0051 in 2012, −0.0035 in 2016, 0.0011 in 2020 and −0.0009 in 2024, never negative and significant). Counties above the median PC(USA) member share show a monotonically growing decline, −0.0039 to −0.0155 across 2012–2024, with the 2024 estimate statistically significant and comparable to the full-sample effect, and the below-median counties sit in between (−0.0047 in 2016 to −0.0024 in 2024, noisy). The bins are small samples and the within-bin estimates are individually imprecise, so this is a gradient rather than three separately well-identified effects, but none-versus-above-median at 2024 is exactly the dose pattern an institutional-spillover story predicts, and exactly what a state-politics or generic-realignment confound would not produce. Figure 5 shows the effect by baseline PC(USA) presence.

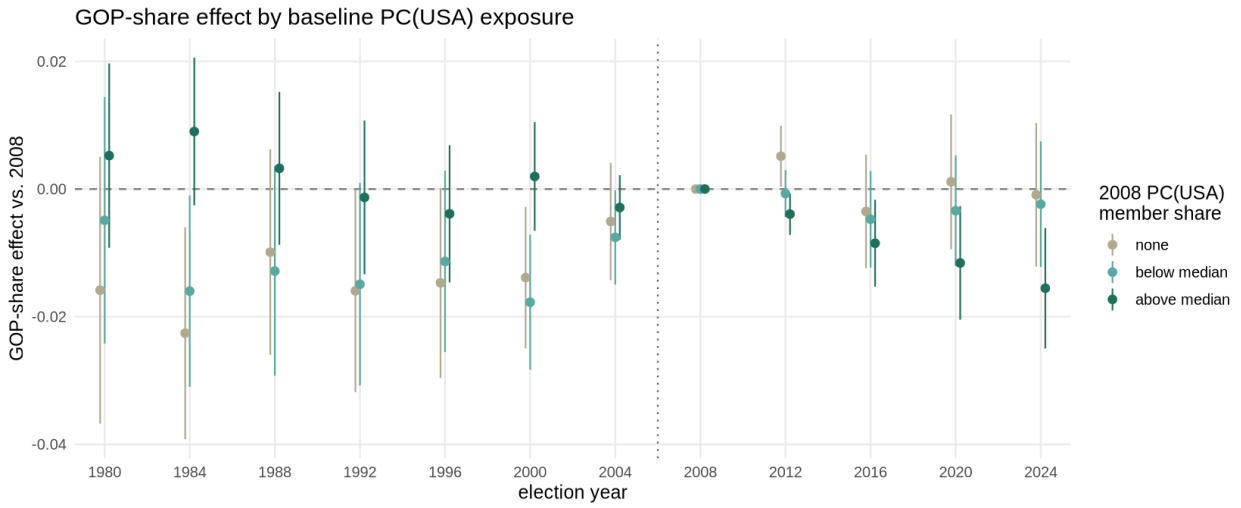


Figure 5: Heterogeneity by PC(USA) presence: border event-study effects on two-party Republican share by baseline PC(USA) member share (none, below the median among counties with any presence, above it), all election years relative to 2008. The dotted line separates the pre-period from the post-period.

A scale calculation makes the political interpretation more subtle, and it applies specifically to the medium-run effect. PC(USA) members average about 0.9% of county population in the border sample (median 0.6%). The small 2012 effect could in principle be a members-only story: −0.0017 divided by the mean member share implies a within-member vote-share shift of roughly −0.2, which is large but possible. The 2016–2024 effects cannot: −0.010 to −0.013 against the same member share implies an impossible within-member shift greater

than one in absolute value. If the medium-run estimate reflects a real causal effect, it is therefore a local institutional spillover (schism changing congregational networks, elite cues, social relationships, local mobilization, or community composition around the church) and not merely PC(USA) members changing their own votes. The alternative reading is not that the result is impossible, but that aggregate political estimates at this treated-share scale deserve care.

4 Mechanism

If the local political effect is real, what produces it? I test four mediators and none of them survives. Controlling for whether a presbytery has a written dismissal-discretion policy leaves the Republican-share and extremity coefficients essentially unchanged. Controlling for a presbytery's own departure rate, defined narrowly as confirmed leavers or broadly to include dissolved congregations, does not shrink the coefficient either, even though the underlying premise is real and well powered on the broad national sample.

Migration is the sharpest remaining candidate, and on the window where the data are clean it moves. The IRS county migration series spans three collection regimes (legacy files through 2004, a two-year publication hole, modern county files from 2007), so I estimate on the modern regime alone and anchor at 2010, the last flow measured entirely before the July 2010 General Assembly vote sent 10-A to the presbyteries. All three population-normalized rates then have flat pre-periods, the 2011 coefficient, the presbytery-voting season, is zero, and after ratification the pattern is retention (Figure 6): out-migration from pro-10-A counties falls by roughly 0.1 percentage points of the filing population and stays down through 2023, net migration rises correspondingly, and in-migration is flat. Fewer people leave the pro-10-A side after the schism. No more arrive. The raw exemption-count version of the same outcome fails its own pre-trend for the same population-growth reason as raw turnout above, which is itself a useful confirmation that the normalized measure, not the raw count, is the one to trust.

The directed flows say where the retention comes from. I split each border county's observable county-to-county flows by whether the counterparty county has any PC(USA) congregation at baseline, and run the same event study on each piece (Figure 7). Arrivals from PC(USA) counties rise on the pro-10-A side, on a flat pre-period, peak in 2016–2017, and fade by 2023: a schism-era wave, not a permanent resettlement, and it comes specifically from the places where the denomination exists. Out-migration toward PC(USA) counties drifts down but is imprecise, and the flows involving counties with no PC(USA) presence either carry their own pre-trends or move little. One caution applies to all four panels: the IRS suppresses small county-to-county cells, so the split covers observable flows rather than exactly decomposing the county totals.

A null on net flows is weaker evidence than it looks, because a county's net flow can sit at zero while the people arriving differ from the people leaving, and it is the composition that a sorting story needs. I therefore also measure the average adjusted gross income per exemption of each county's in-flow and of its out-flow separately. Both have flat pre-periods, and neither diverges across the boundary after 2011. The people moving into pro-10-A counties after the schism do not look different, on the one dimension these data measure,

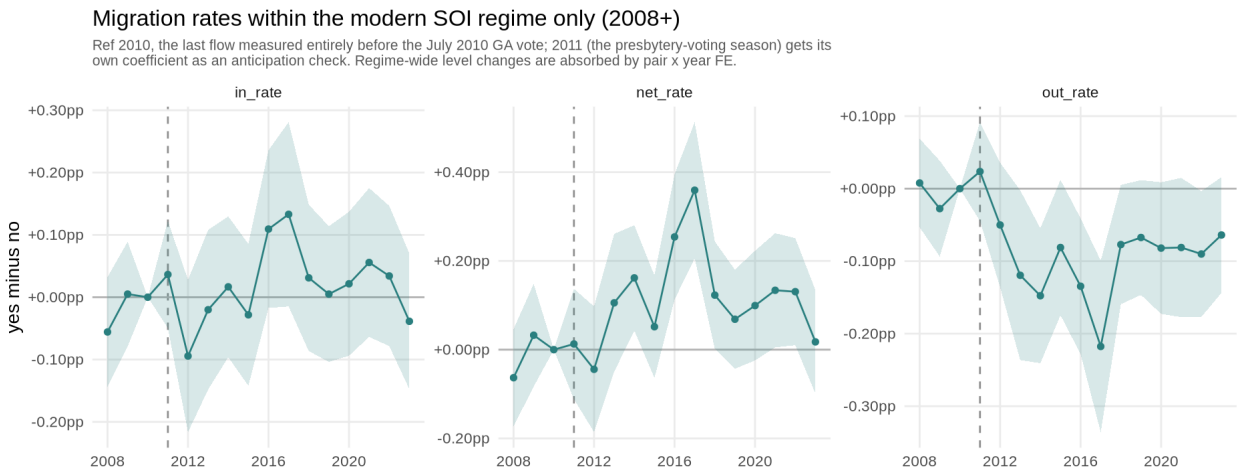


Figure 6: Migration rates across the matched borders, modern IRS regime only (2008–2023), reference year 2010: the last flow measured entirely before the General Assembly sent 10-A to the presbyteries. The 2011 flow spans the presbytery-voting season and shows no anticipation. The post-ratification pattern is retention: out-migration falls and stays down, in-migration does not move.

from the people moving into their anti-10-A neighbors. Income is a proxy for the political composition a sorting story actually cares about, and a coarse one, but it is the margin the data can see, and it does not move.

The third test is the one this geography most naturally invites and it does not work. Households crossing the presbytery line itself, the flow between the two counties of a matched pair, would be the cleanest evidence of sorting on the boundary, and the IRS publishes the county-to-county matrix that contains it. Two features of that matrix defeat the test. Its cells fail their own pre-trend badly, so the post-2011 pattern continues a divergence already underway before the schism. And the IRS suppresses small origin-destination cells on a threshold it tightened during the sample: about 45% of matched-pair flows are unobserved before 2013 and about 60% after, so cells go missing at exactly the point the treatment turns on. I report the attempt because the design looks compelling until those two facts are checked, and a reader who would otherwise ask for it should know it was run.

Net of this section, the political divergence is at least partly a migration story on the volume margin (after ratification, people stop leaving the pro-10-A side) while every composition margin I can measure (the income of movers, and the partisanship of the counties they come from and go to) stays flat, and the institutional frictions I can measure absorb none of the political effect. But the mediators enter as presbytery-level measures spread across every county in the presbytery, including the many with no PC(USA) congregation at all, while the effect itself is concentrated in the counties where the denomination was actually present, so a county-level channel would not load on them. Politically selective migration, local mobilization, and elite cue-taking remain untested rather than rejected.

The timing narrows the field a little further. Sorting through residential mobility accumulates gradually and should grow from one election to the next. The estimated effect is instead near zero in 2012 and close to full size by 2016, holding steady after. That step fits

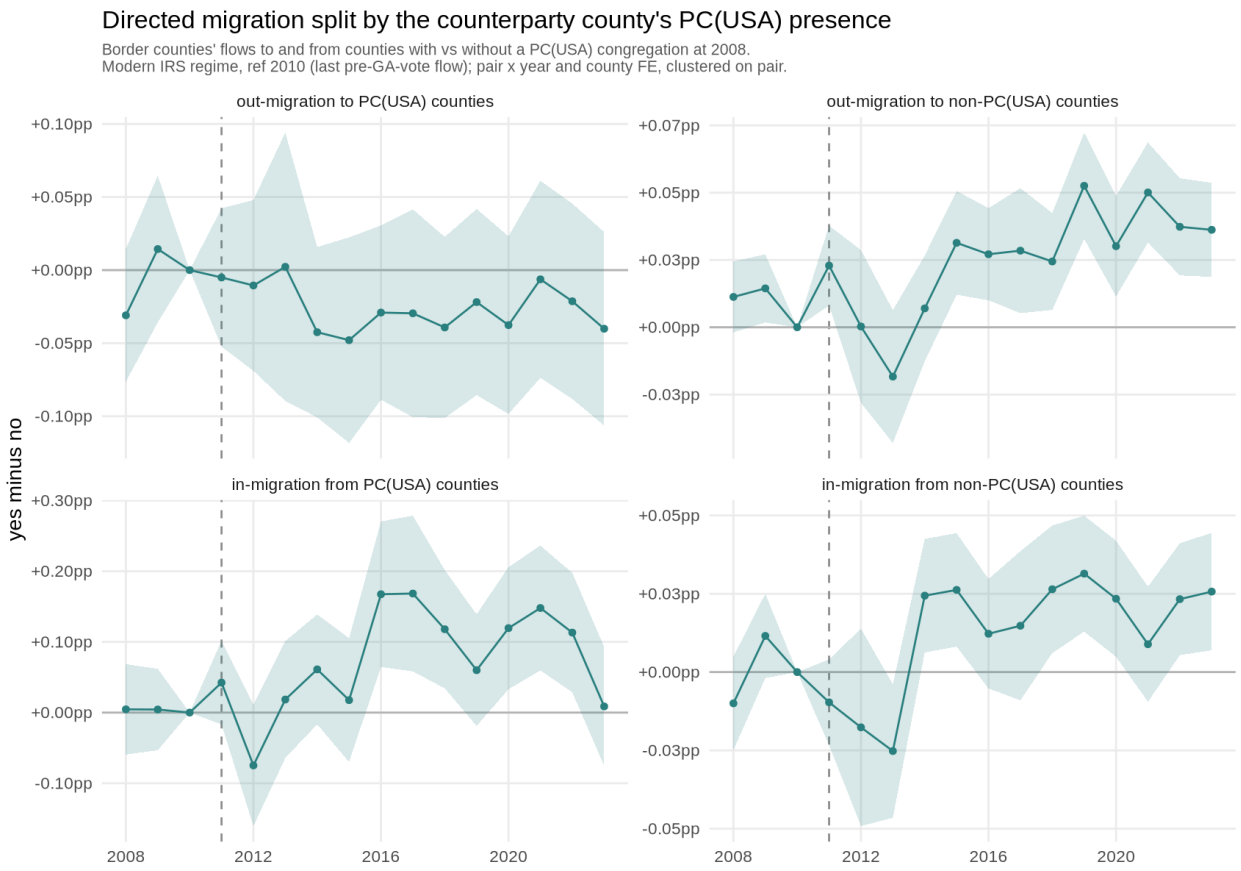


Figure 7: Directed migration split by the counterparty county's PC(USA) presence at 2008 (2,358 counterparty counties with a congregation, 759 without). Modern IRS regime, reference year 2010, matched-pair-by-year and county fixed effects, clustered on the pair. Suppressed small county-to-county cells drop out, so the panels cover observable flows rather than exactly decomposing the county totals.

a salience or cue-taking channel (the schism changing which religious institutions remain in a community and what their leaders signal) more comfortably than gradual replacement of the electorate. It does not establish one, and I leave the mechanism open.

5 What drives the decision to leave

I estimate this on the full census of 12,982 PC(USA) congregations ever observed in the panel, rather than on the subset tagged by the denomination's internal "Confessing Church" movement, which misses 56–60% of confirmed leavers depending on vintage. A congregation's own theological self-identification is the largest and most robust predictor of leaving: Confessing-tagged congregations leave at a 28–34 percentage point higher rate than otherwise identical congregations, with statistically significant in every specification I run.

The clearest and most novel result in this section concerns pastor gender. Built from a from-scratch match to a denominational minister database (95.2% match rate), a female pastor's main effect on leaving is modestly negative (-0.028 to -0.041) and statistically significant, but the interaction with Confessing-church status is large: -0.096 to -0.116 across the specifications that enter pastor gender alone ($p \leq 0.008$ throughout). Theologically conservative congregations with a female pastor leave at roughly half the rate of otherwise-similar congregations with a male pastor. The same pattern replicates independently at the level of lay leadership, now measured on the same rebuilt full census rather than on the older Confessing-only subsample: the female share of a congregation's *session* (its governing elders) has a main effect of -0.113 and a Confessing interaction of -0.538, both statistically significant. Read together, these two independently-constructed measures of the same underlying fact of whether women hold real governing authority in the congregation, both point the same direction, which is some protection against either being a data artifact of the other.

Two refinements sharpen what that shared fact is. First, the effect is specific to *governing* authority: the female share of the *diaconate*, a service rather than a ruling office in Presbyterian polity, predicts nothing at all: neither the main effect nor the Confessing interaction is significant. Second, when pastor gender and session composition are entered together, session composition absorbs most of the joint effect (session interaction -0.524, remains significant, while the pastor interaction falls to -0.062 and is not. The congregation's own governing body, not the pulpit alone, is where this shows up.

Proximity to an outside option matters, but not uniformly across destinations. A same-city EPC congregation predicts leaving with a statistically significant negative-on-mismatch interaction. The analogous ECO interaction is not significant and I do not read it as evidence of anything.

Separating formal membership from broader participation (adherents minus communicant members, following PC(USA)'s own definition) shows the two doing different work: net of membership, more non-member engagement predicts *less* leaving (-0.011, significant), while membership's own effect survives and is specifically pronounced among Confessing congregations (membership $\times$ Confessing interaction $+0.098$, significant) – consistent with larger, better-resourced Confessing congregations having the organizational capacity to actually execute a departure that smaller ones lack. Congregation age (from organized year,

recovered across each pin's full panel history rather than a single baseline row) has no robust independent effect on leaving.

The cleanest way to net out selection into *wanting* to leave would compare congregations that voted to leave and followed through against congregations that voted to leave and then stayed. That comparison cannot be made. Rebuilding the congregation classifications from the underlying dismissal records shows that 51 of the 55 congregations a legacy file had coded as "voted but stayed" in fact left, so there is no voted-but-stayed group of any size to compare against.

The weaker but honest substitute, reported in Section 5, is Confessing Church status, a congregation's own public theological self-identification, recorded years before any vote and independent of the withdrawn file, as the marker of demand for exit, comparing Confessing congregations that left against Confessing congregations that did not. That comparison does not net out selection as cleanly as a realized vote would have, since Confessing status is a coarser signal of intent than a congregational vote. It is reported as what it is.

	(1)	(2)	(3)	(4)	(5)
Confessing Church	0.311^{***}	0.324^{***}	-0.150^{**}	0.339^{***}	0.311^{***}
	(0.016)	(0.018)	(0.072)	(0.017)	(0.023)
Female pastor		-0.036^{***}			
		(0.005)			
Female pastor × Confessing		-0.112^{***}			
		(0.038)			
log membership	0.022^{***}	0.022^{***}	0.017^{***}		
	(0.002)	(0.002)	(0.003)		
log membership × Confessing			0.094^{***}		
			(0.015)		
log non-member participants			-0.008^{***}		
			(0.002)		
log non-member part. × Confessing			0.002		
			(0.010)		
Presbytery voted yes on 10-A					-0.038^{***}
					(0.006)
10-A yes × Confessing					0.007
					(0.032)
N	9365	9365	9365	9365	9365

Table 4: What predicts congregational exit from PC(USA)

OLS, robust standard errors in parentheses. Outcome: left the PC(USA) (dismissed, split, or informal split); dissolved congregations are not counted as leavers. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Every column is estimated on the same 9,365 congregations – the complete cases across every variable any column uses, out of 12,982 in the full census – so the columns are comparable to each other.

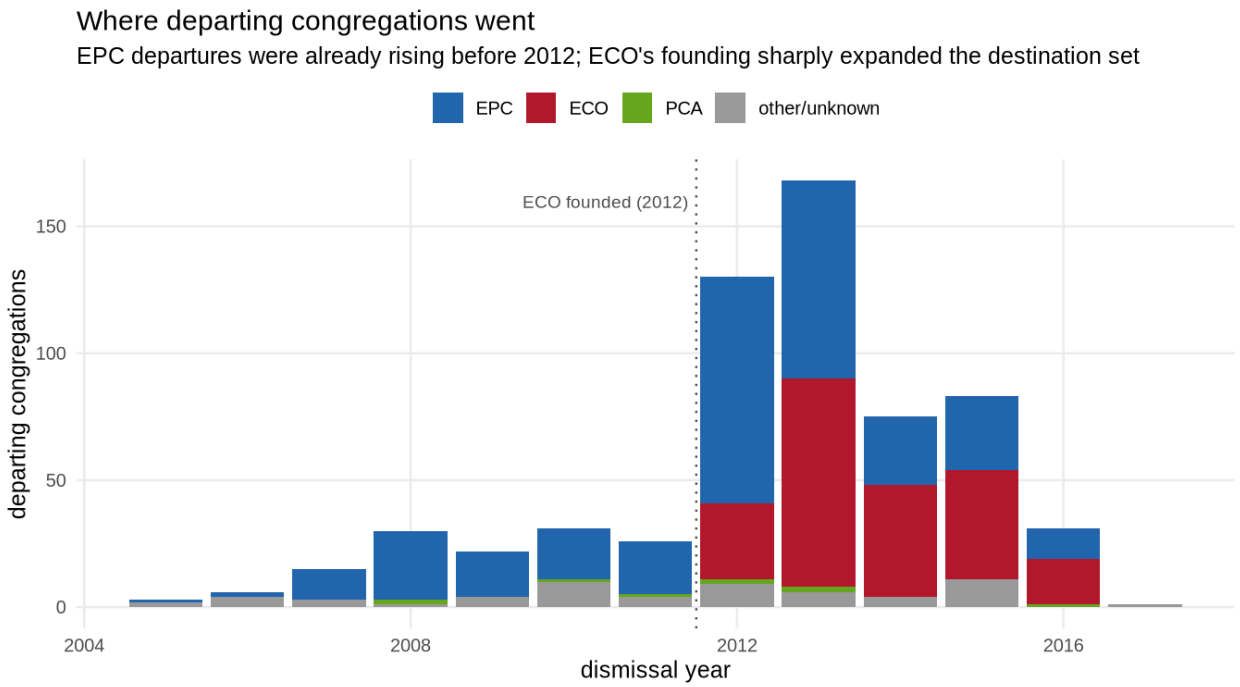


Figure 8: When congregations left and where they went: EPC, ECO, PCA and other destinations by year of departure.

6 Costs to congregation

I use pin-keyed payment amounts for 292 confirmed leavers, nearly double the sample available before this project's 2026 correction to the dismissal records, and re-derived from the source spreadsheet rather than from a legacy intermediate file. The price a departing congregation pays for its property scales almost exactly one-for-one with membership: an elasticity of 0.91–1.02, statistically significant in every specification I run, and the most stable estimate in the paper.[4] Neither pastor nor lay-leadership gender predicts price once I control for size: a real null, and one that holds before and after the correction that doubled the sample. The raw, uncontrolled group means flip sign across that correction. The regression conclusion does not.

Whether a presbytery merely *has* a written dismissal policy does not move price, but which default that policy sets does: relative to case-by-case defaults, negotiated defaults predict 0.71 log points higher payment and unspecified defaults 0.49, both statistically significant, though part of this is close to definitional (a congregation-retains default implies a payment near zero by construction). Broader engagement (non-member participants, net of membership) does not significantly predict price, though it is positive in sign: price tracks the formal membership roll specifically, consistent with membership being the relevant measure

[4] A congregation counts as having left if its recorded status is dismissed, split or informally split, which includes 116 congregations that split and left a remnant still filing with the PC(USA). Restricting to congregations that left in full moves the elasticity from 1.02 to 1.09 and the Confessing coefficient in the exit first stage from 0.289 to 0.255. Neither result turns on the choice, and I keep the broader definition because a congregation that split did decide to leave and did pay.

of a congregation's transferable financial/property capital.

	(1)	(2)	(3)	(4)	(5)
log membership	0.956***	0.959***	0.940***	0.921***	0.942***
	(0.112)	(0.114)	(0.114)	(0.114)	(0.109)
Female pastor		0.360			
		(0.308)			
log non-member participants			0.045	0.045	
			(0.047)	(0.046)	
log congregation age				−0.279**	
				(0.138)	
Presbytery policy default (vs. case-by-case):					
congregation retains					0.030
					(0.289)
negotiated					0.721***
					(0.227)
not specified					0.500**
					(0.244)
presbytery retains					1.978***
					(0.329)
N	205	205	205	205	205

Table 5: Realized dismissal payments: size is the price

OLS, robust standard errors in parentheses. Outcome: log realized dismissal payment, real 2015 USD, for leavers with a positive pin-keyed payment. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. Every column is estimated on the same 205 congregations – the complete cases across every variable any column uses, out of 292 with a positive payment – so the columns are comparable to each other. Policy categories: case by case 33, congregation retains 23, negotiated 87, not specified 60, presbytery retains 2. **presbytery retains has only 2 congregations and its coefficient should not be read as an estimate.**

7 Event study around leaving

Congregations that eventually left were not PC(USA)'s weakest. They were, if anything, its strongest. Across the entire pre-departure window (2000–2011), eventual leavers gave 20–30% more per member than congregations that stayed ($850–1,167 per member versus $686–903), a gap present from the very start of the panel, not one that opens up as departure approaches. Mission spending's share of the budget rises for leavers, stayers, and dissolved congregations alike over this period, most plausibly a mechanical artifact of per-capita apportionment claiming a growing share of shrinking overall budgets across the whole denomination rather than anything specific to the schism.

Following 409 departed congregations into their new denomination (104 confirmed by hand review, 272 matched automatically and accepted, 33 flagged for review and included regardless) using real, PDF-verified EPC finance records shows a sharp transition. Mission share drops abruptly at the point of departure (from roughly 0.17–0.20 under PC(USA) to

about 0.10 in the first year under EPC), recovers partially over five years to 0.11–0.16, but stays below both its own pre-departure level and a calendar-matched stayer baseline (which holds steady around 0.165–0.186 throughout). Giving jumps sharply at the transition, from a declining \$412K to \$218K under PC(USA) in the years immediately before departure, to \$558–716K under EPC immediately after, roughly three times the stayer baseline. Two objections to that transition can now be answered. The first is definitional: the outcome changes source at departure, from the PC(USA)'s recorded contributions to the EPC's tithes and offerings, so a jump might be an artifact of two denominations counting differently. It is not. For the 287 congregations observed on both sides within three years, the median ratio of the first EPC figure to the last PC(USA) figure is 1.02 (interquartile range 0.93–1.14), against 0.99 for a congregation that never left over the same gaps: the accounting contributes about four percent, the size of an ordinary year's drift. Membership is cleaner still, at 0.985. The second objection is compositional: that the jump reflects which congregations happen to be matchable rather than what departure does. Matching now reaches 403 of the 413 congregations whose recorded destination is the EPC, so the matched set is very nearly the population and there is little room left for selection into it.

Estimating the transition rather than describing it sharpens what survives. I classify every congregation by what its departure actually did (12,075 never tried, 791 left fully, and 116 split, leaving a remnant that kept reporting to the PC(USA)) and estimate Sun and Abraham [2021] event studies against never-treated congregations, so the denomination-wide decline is absorbed by the year effects rather than attributed to departure. The omitted period is $t-3$ rather than the customary $t-1$: a departure is anticipated, and a congregation heading for the door is already losing members while it argues about it, so normalising on the year before the vote hides the run-up inside the baseline. That choice matters. On $t-1$ the full-census specifications carry significant pre-trends. On $t-3$ they are flat, and their point estimates move toward zero.

What emerges is that leaving does not change a congregation's size and does change where its money goes. Membership is flat (an average effect of -0.02, not significant, against all never-treated congregations). Giving rises about ten percent and total expenditure by nineteen to twenty-five percent net of the accounting shift. Mission share, meaning the fraction of spending that leaves the congregation, falls three to four percentage points (-0.039), on a flat pre-period. Per-member figures tell the same story: expenditure per member rises, giving and income per member are flat once the source change is netted out. A congregation that leaves is no larger and no richer per member. It spends more and sends a smaller share of it outward.

One comparison is worth reporting because it is so easily misread. A congregation that splits keeps only its remnant in the PC(USA)'s book. Measured that way, leaving fully appears to outperform splitting by 44 log points of membership. Counting both halves of the split, the remnant plus the departing group's own EPC record, the gap is four log points and statistically indistinguishable from zero. The apparent effect was almost entirely a question of which half of a divided congregation the source lets one see, and it is a trap any study reading a single denomination's books will encounter.

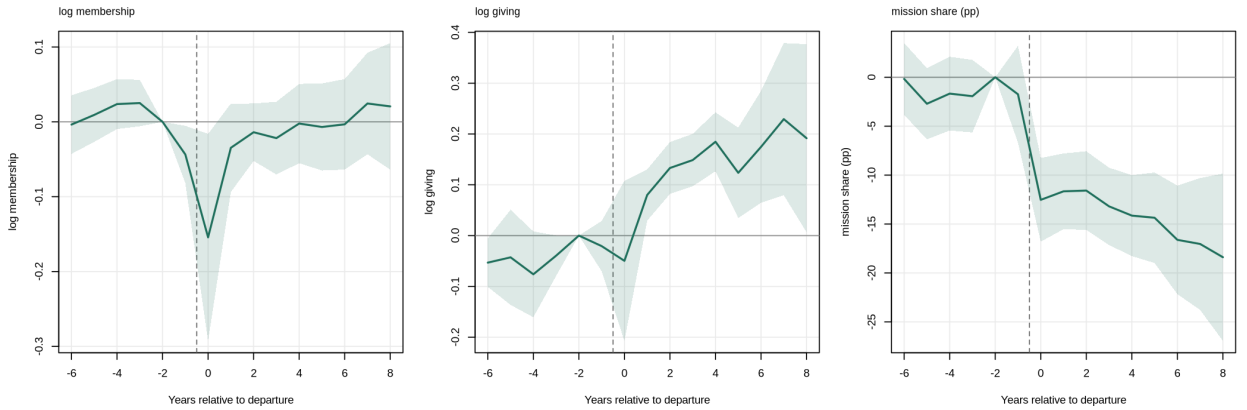


Figure 9: Membership, giving, and mission share around departure: twoway fixed-effects event studies against never-treated stayers. The omitted period is $t-2$ rather than the conventional $t-1$ because dismissal negotiations run two to three years, so $t-1$ is already treated, and event time is binned at $[-6,+8]$ because the unbinned tail is identified by the 2011 cohort alone. Membership is measured identically on both sides of the departure. Giving and mission share cross a definitional break at $t=0$ (PC(USA) accounts before, EPC accounts after), so their post-period trends carry the interpretation and the level jumps do not. Sun and Abraham and LP-DiD versions of all three are in Appendix Figure 13.

8 Alternative explanations: sorting, social capital, resource splitting

I address sorting and social capital above: sorting in Section 2, where it fails as a mediator of the political effect, and social capital's direct cross-sectional correlate in Section 1. What is new here is where a departed congregation's money actually goes.

A presbytery's raw, PC(USA)-recorded total giving declines mechanically with its own leaver rate: regressing the percent change in a presbytery's pre-2011-to-post-2011 recorded giving total (summed across every congregation that was ever PC(USA)-reporting, stayers and leavers alike, each counted only in the years it actually reported) on that presbytery's leaver rate gives a statistically significant coefficient of -0.394 (N=172), a strong, obvious result, since a congregation that leaves the denomination by construction stops appearing in PC(USA)'s own statistics. Adding back the giving of the 408 leavers (43.6% of the 936 total) matched to a real post-departure EPC record, on the same 2000–2017 window as the PC(USA) side and imputing the EPC source's own missing years, cuts that relationship by more than half and leaves it statistically indistinguishable from zero: -0.166 (same N). Without the window restriction the coefficient falls further, to -0.046, also not significant. 116 of 175 presbyteries contain at least one matched, giving-recovered leaver, and 8% of the expected in-window congregation-years are imputed at the congregation's own mean.

That accounting correction should not be read as showing the departures cost the denomination nothing. It shows that the money is still being given, not that it is still being sent outward. Mission share falls three to four percentage points after departure and does not recover within five years, on specifications with flat pre-periods, so the outward-facing

portion of congregational spending genuinely contracts even as the totals hold up. The PC(USA) loses less money than its own books suggest and more mission activity than the accounting reveals.

Figure 10 sets the four outcomes against the departure rate. Two things follow, and they should be kept separate. First, a substantial share of what looks like destroyed local giving capacity is not destroyed but relocated: the recorded decline is meaningfully smaller once the money is followed across the denominational boundary, using nothing but real EPC filings. Second, the recovery is partial, and this design cannot say whether the remainder is a genuine loss or simply untracked. Only EPC destinations are recovered here. ECO, PCA, ACNA, and independent destinations are not observed at all, and they account for roughly three-quarters of confirmed leavers. If those congregations continued giving at rates comparable to the EPC-bound ones, the true recovery would be substantially larger than what is measured. The honest statement is therefore that a real and sizable fraction of the apparent local loss is a transfer rather than a destruction, with the remainder unresolved rather than shown to be destroyed.

The receiving denomination's own books show the same transfer from the other side, and without reference to PC(USA) records. I split the EPC's reported membership by whether a congregation matches one of the departures I follow. In 2006, congregations that had come from the PC(USA) accounted for 2.9% of EPC reported membership; by 2013 they accounted for half, and by 2021 for 53.7%. Over the same window the EPC's reported total roughly doubles (Appendix Figure 14), a factor of 1.91, and almost none of that is growth. Congregations already in the denomination, or unmatched to any departure, contracted by 9%, and the balanced panel of 178 congregations filing in both 2006–2008 and 2019–2021 grew 9% in fifteen years, which is close to flat. A denomination that looks from its headline totals to have doubled instead absorbed a body of congregations roughly the size of itself while its own congregations went nowhere. Read against the accounting correction above, the two sides agree: the schism moved members and money between denominations rather than destroying them.

Two qualifications keep that number honest. The unmatched group is not a clean incumbent series (it holds the roughly two dozen departing congregations I could not match, along with genuine EPC church plants), and EPC reporting coverage widens sharply across the window, from 177 congregations filing in 2006 to 563 in 2021, so the unmatched total moves with who files as well as with who grows. The balanced panel is the coverage-proof version of the same statement and it tells the same story. This is the identical extensive-margin problem that disqualifies the county aggregates in Section 9, and it is why I report the balanced figure alongside the headline one.

What is observed about the unrecovered destinations is worth stating, because it is not nothing. The event census records 572 PC(USA) departures to the EPC and 351 to ECO: A Covenant Order of Evangelical Presbyterians, with the remaining destinations (the Evangelical Covenant Church, the PCA, the Cumberland and Associate Reformed Presbyterians, and a handful of non-Presbyterian bodies) accounting for fewer than forty between them. ECO is therefore not a residual category but the second destination, taking roughly two departures for every three that went to the EPC, and its 2012 founding is visible in the timing of departures in Figure 8. Survival can be established for those congregations even where finances cannot: 154 of the ECO-bound leavers are confirmed active in ECO's own

congregation directory, which lists 454 congregations in total.

That ECO congregations are not carried into the financial analysis is a data-availability constraint and not a modelling choice. ECO publishes a current directory but no per-congregation statistical or financial series, and no equivalent of the EPC's General Assembly tables exists to be extracted; nor does the directory carry the year a congregation joined, so even the arrival timing that identifies EPC-bound congregations is unavailable. Enquiries to ECO's national office established that the denomination does not hold congregational statistics centrally by presbytery. The consequence for the estimates is one-directional and stated in the preceding paragraph: giving that moved to ECO is invisible here, so the recovery reported is a lower bound on the share of apparent local loss that is in fact transfer.

The PCA route is now closed, and it closes small. Its congregational statistics are published annually with an organization identifier and a statistical year, and I have assembled them: 12,176 congregation-years across 1,272 congregations for 2000–2016. But only 12 PC(USA) congregations went to the PCA at all, so recovering their giving cannot move the accounting above. The panel is useful for a different purpose, reported in Section 9: it is the one destination body with a long enough pre-period to test whether a conservative Presbyterian denomination grew differentially on either side of a PC(USA) presbytery boundary.

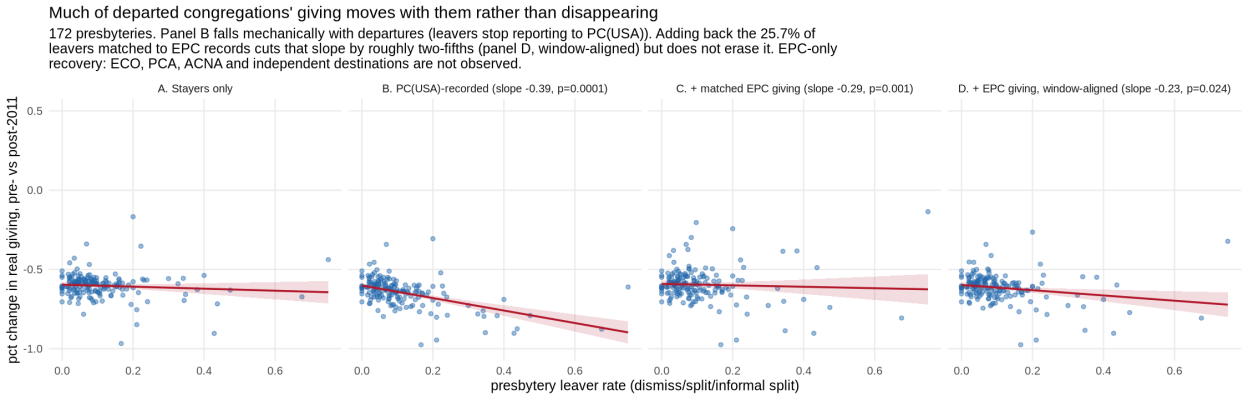


Figure 10: Presbytery resource splitting: four outcomes against the departure rate.

9 Discussion

I treat schism as an institutional process rather than as a single cultural shock, and that is my main contribution. The political results show that the 10-A divide maps onto later local partisan and electoral-extremity change, but PC(USA) is too small in these counties for a pure member-vote explanation. The congregation results show what the institutional process looked like: disagreement created demand for exit; leadership and outside options shaped whether that demand became a realized departure; property rules and bargaining translated departure into payments; and successor records show that giving capacity often moved with congregations rather than disappearing.

The argument is therefore not that a denominational vote mechanically changed county

politics in one election. It is that religious governance channels social conflict into organizational sorting, and that this sorting changes both the religious economy and, modestly, the local political environment. That framing also explains the nulls and weak results. Turnout fades after 2012; out-migration falls and stays down while the composition of movers does not change; education does not move; social-support outcomes may well be too aggregate to register a denomination-specific shock. Those boundaries are part of the paper's contribution: they distinguish a measurable institutional reallocation from a sweeping claim about county-level economic transformation.

Five limitations bound what I can claim, and I state them in the order they would change a reader's mind.

The first is that I follow congregations into one destination. Of the 936 confirmed leavers, 428 went to the EPC and I match 403 of them; 391 went to ECO, which publishes no congregational statistics by polity and so cannot be followed at all without collecting individual congregations' own filings. Everything in Sections 5 through 8 that depends on post-departure outcomes therefore describes the EPC half of the schism. Whether the ECO half behaves the same way is genuinely unknown, and it is not a small question: ECO was founded in 2012 explicitly as a destination for departing PC(USA) congregations, so the two groups may have selected on different things. The PCA is a smaller version of the same problem in reverse: its statistics are published and usable, but only 12 congregations went there.

The second is that a presbytery's Amendment 10-A vote is not randomly assigned. Presbyteries voted the way their congregations and commissioners already leaned, and congregations sort across presbytery boundaries for reasons correlated with that lean, so the border design cannot, in principle, hold constant a difference that the boundary itself was drawn around. I test that concern rather than concede it. Using full-address geocodes rather than zip-code centroids, I measure each congregation's distance to the shared boundary of every pair of touching presbyteries that voted differently, and re-estimate holding the congregation fixed. The differential pre-trend in membership over 1983–2007 is $+0.003$ log points per year within ten kilometers of the line (354 congregations) and per year within ten kilometers of the line (354 congregations) and -0.001 within fifty (1,883 congregations), neither significant. It is not distinguishable from zero at any bandwidth, and (the part that matters) it neither shrinks nor grows as I narrow the window, which is what sorting on trends would produce. Parallel trends holds where the design rests on it.

What does not survive that check is the county aggregate, and the distinction is worth stating plainly because it cuts against a natural way to use these data. County PC(USA) membership carries a differential trend running the length of the panel. Decomposing it, the pre-2000 portion is the denominator (voted-yes counties are more urban and their population grew faster, so members per resident falls with no change in anyone's behavior), and the portion after 2000 is the extensive margin, which congregations exist and file a report in a given county. Neither is surviving congregations on one side shrinking faster than their neighbors, surviving congregations on one side shrinking faster than their neighbors, because the congregation-level estimates above show they did not. I therefore run the membership designs at the congregation level and read county aggregates as description. A reader who wants a county-level religiosity first stage will not find a credible one here, and I do not report one.

The third is external validity. This is one denomination, of about 1.2 million members at the time of the schism, in a country where most religious adherents belong to bodies organized very differently. The PC(USA)'s presbyterian polity is what makes the design possible: property is held in trust, exit requires a vote by a governing body, and that body's own composition varies. A congregational or episcopal tradition would produce neither the same variation nor the same records. I would expect the institutional logic to travel and the magnitudes not to.

The fourth is measurement on the political side. I use total county population as the turnout denominator rather than the voting-age or citizen-voting-age population, because a consistent county-year series of the latter is not available for the full window; the turnout results should be read as changes in votes cast per resident. On the membership side the congregational statistics change source in 2014, inside the post-treatment window, so I split the post period at the break: the estimates either side agree at every boundary bandwidth, with no significant interaction, and diverge only when I pool congregations at all distances, which is not the specification I rely on. The health, social-capital and education outcomes enter as single-vintage cross-sections, so they support level comparisons across the border but not event studies.

The fifth is power, and it is why several of the nulls are quiet rather than informative. A denomination this size is small relative to a county's poverty rate or its safety-net caseload, and I would not expect to detect a PC(USA)-specific effect on those outcomes even if one existed. Where I report a null on such an outcome I mean that the data cannot distinguish a modest effect from zero, not that I have bounded the effect near zero.

A Appendix: additional figures and tables

	2012	2016	2020	2024
Panel A: two-party Republican share				
Baseline (border-pair×year + county FE)	-0.0017^{**}	-0.0088^{***}	-0.0103^{***}	-0.0112^{***}
	(0.0008)	(0.0015)	(0.0020)	(0.0021)
Same-state county pairs only	-0.0015^{*}	-0.0072^{***}	-0.0091^{***}	-0.0103^{***}
	(0.0009)	(0.0021)	(0.0027)	(0.0029)
Cross-state county pairs only	-0.0019	-0.0107^{***}	-0.0116^{***}	-0.0122^{***}
	(0.0013)	(0.0023)	(0.0028)	(0.0031)
+ state × year FE	-0.0010	-0.0073^{***}	-0.0097^{***}	-0.0108^{***}
	(0.0009)	(0.0020)	(0.0026)	(0.0028)
Leave-one-boundary sign flips (of 153)	0	0	0	0
Panel B: electoral extremity ($\lvert$GOP share $-$ 0.5$\rvert$)				
Baseline (border-pair×year + county FE)	-0.0000	-0.0077^{***}	-0.0074^{***}	-0.0079^{***}
	(0.0010)	(0.0022)	(0.0024)	(0.0027)
Same-state county pairs only	$+0.0023^{*}$	-0.0030	-0.0023	-0.0027
	(0.0013)	(0.0031)	(0.0035)	(0.0040)
Cross-state county pairs only	-0.0026^{*}	-0.0130^{***}	-0.0131^{***}	-0.0139^{***}
	(0.0015)	(0.0030)	(0.0033)	(0.0037)
+ state × year FE	$+0.0025^{**}$	-0.0029	-0.0031	-0.0034
	(0.0012)	(0.0029)	(0.0033)	(0.0037)
Leave-one-boundary sign flips (of 153)	80	0	0	0
HonestDiD breakdown $\bar{M}$ (2024 target)		GOP share: 2.0	Extremity: 2.0	

Table 6: Robustness of the 10-A border political effects

Standard errors clustered on the county pair in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

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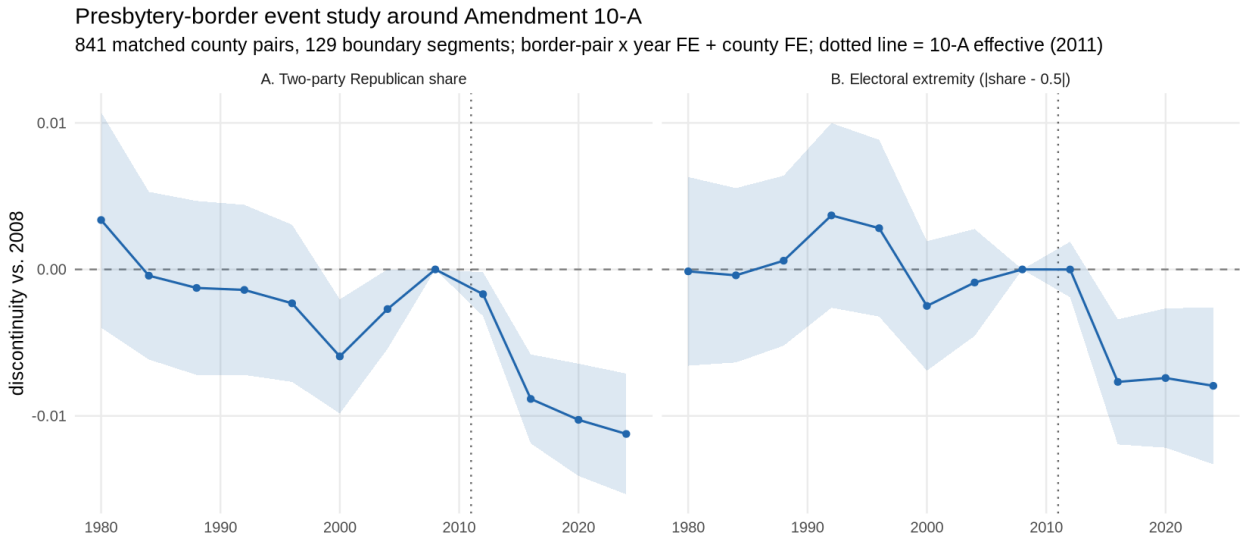


Figure 11: Full-sample version of Figure 4: all 994 matched county pairs, including cross-state pairs. Two-party Republican share, 2008 omitted, 10-A's 2011 effective date marked.

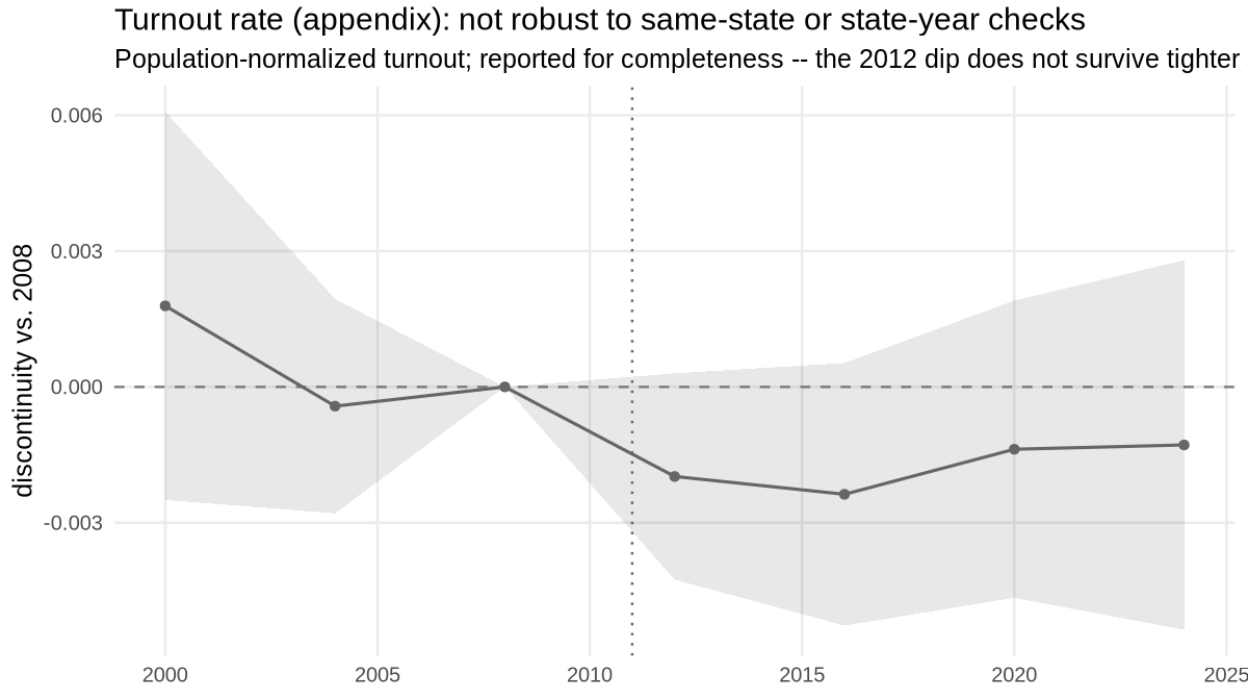


Figure 12: Full-sample version of the turnout panel of Figure 4.

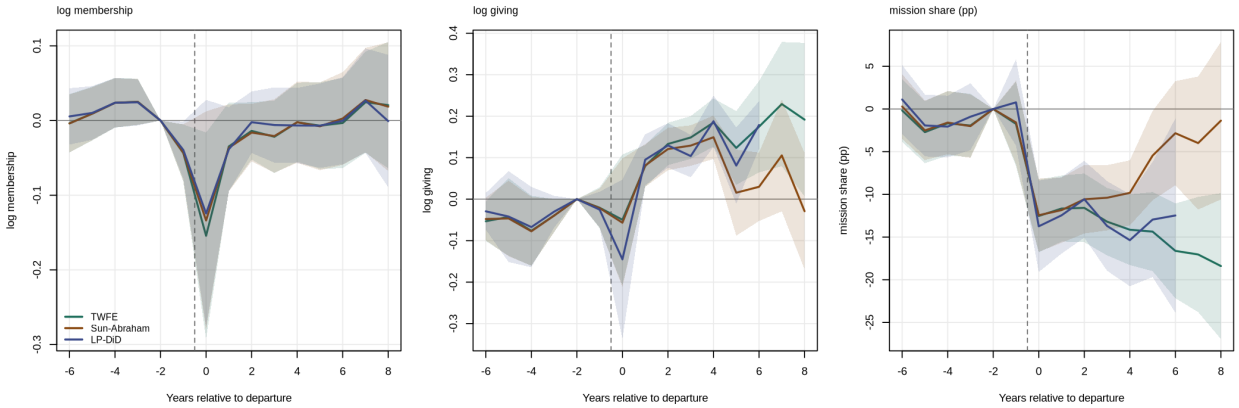


Figure 13: Estimator comparison for the within-congregation event studies of Figure 9: TWFE, Sun and Abraham (2021) interaction-weighted, and LP-DiD long differences on the same axes for each outcome. The three estimators agree closely on every outcome: membership dips at the transition and recovers, giving rises 0.1 to 0.2 log points, and mission share drops 10 to 15 percentage points against never-treated stayers. The smaller mission-share declines in the exit-versus-remnant specifications come from a different contrast, not from estimator disagreement.

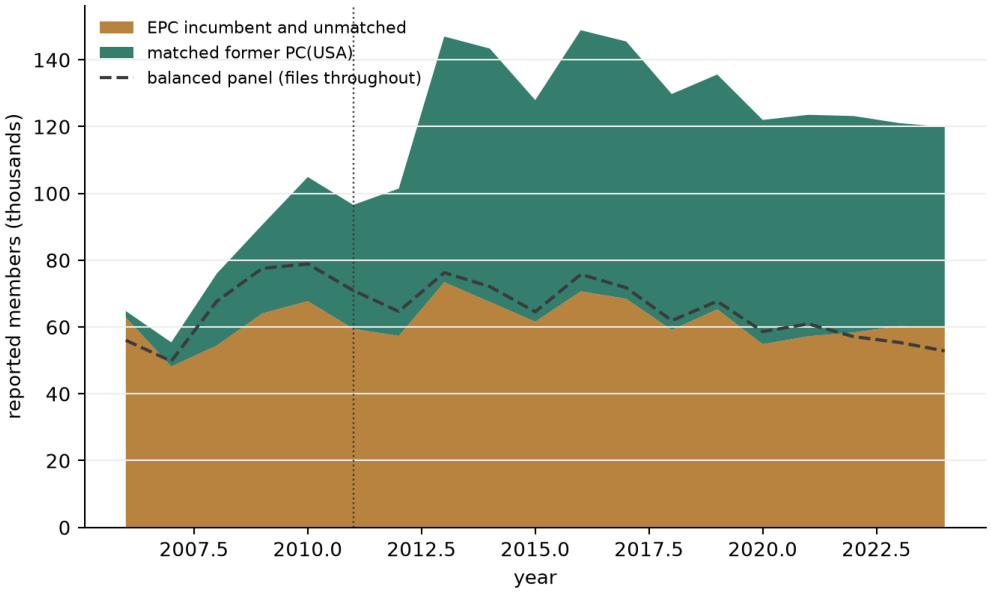


Figure 14: EPC reported membership, 2006–2021, split by whether the congregation matches a confirmed PC(USA) departure. The dashed line is the balanced panel of 178 congregations filing in both 2006–2008 and 2019–2021.

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