Taxing Internal Capital Markets and the Repatriation of Risk

Anastasia Kartasheva* Andre Kraemer†

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Abstract

Taxing internal capital markets can curb profit shifting but may also affect risk sharing within multinational firms. We study how the 2017 Base Erosion and Anti-Abuse Tax changed where the risk is borne in the U.S. property-casualty insurance industry. We exploit the §953(d) election, which allows a foreign affiliate to be taxed as a U.S. corporation while remaining offshore, to examine which transfers persist under U.S. taxation. We estimate a policy-induced substitution ratio: the increase in cessions to electing affiliates was about one-third of the decline in non-electing cessions. Replacement was concentrated among groups with high catastrophe exposure, a pattern consistent with benefits from global diversification. The contraction in foreign-affiliate reinsurance increased underwriting risk retained on U.S. balance sheets, with groups retaining a larger share of current-year unpaid losses. We detect no statistically significant increase in required regulatory capital. The estimated decline in the credit charge on reinsurance recoverables approximately offset the increase in the premium-risk charge. These findings show how tax policy changes the location of insurance risk and highlight a potential limitation of regulatory capital measures in capturing changes in retained exposure.

JEL classification: G22, G32, F38, H26

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*Swiss Institute of International Economics and Applied Economic Research, School of Finance, University of St. Gallen, and Swiss Finance Institute. Email: anastasia.kartasheva@unisg.ch

†Swiss Institute of Banking and Finance, School of Finance, University of St. Gallen. Email: andre.kraemer@unisg.ch

1. Introduction

Multinational firms rely on internal capital markets to allocate resources across their global subsidiaries. A central debate in corporate finance concerns the extent to which these flows create economic value and facilitate profit shifting to low-tax jurisdictions (Desai et al., 2004; Gopalan et al., 2007; Stein, 1997; Tørsløv et al., 2023). These motives can have different policy implications. Restrictions on internal transactions may limit financing and risk-sharing benefits while reducing opportunities for profit shifting. Identifying their relative importance is difficult because a single transaction can serve both purposes. Reinsurance within multinational insurance groups provides a useful setting: one contract can transfer insurance risk and taxable income across borders. Contracts that transfer genuine risk can also generate tax benefits (Seessel, 2001).[1] Observed cessions and the receiving affiliate's location therefore do not, by themselves, reveal the relative importance of tax incentives and non-tax benefits (Altuntas et al., 2018).

We address this challenge by studying a policy-induced increase in the cost of cross-border affiliate payments. The 2017 Tax Cuts and Jobs Act (TCJA) introduced the Base Erosion and Anti-Abuse Tax (BEAT), a minimum tax that includes deductible payments from U.S. corporations to foreign related parties in its base. Domestic affiliate and arm's-length payments fall outside this provision's direct tax base, although these channels may respond through substitution. The reform allows us to examine the tax sensitivity of offshore affiliate reinsurance and whether groups preserve displaced flows through alternative arrangements.

The BEAT targeted base erosion by large multinationals generally rather than insurers specifically. It emerged late in the legislative process, appearing in the Senate proposal on November 9, 2017, and applied to tax years beginning after December 31, 2017 (U.S. Senate Committee on the Budget, 2017).[2] Our annual statutory data distinguish 2017, before implementation, from 2018, the first year of implementation, although anticipation during late 2017 remains possible.

We study the U.S. property-casualty insurance industry, which writes more than $1 trillion in annual premiums as of 2024. Affiliate reinsurance is an important channel through which

[1] For tax purposes, a contract is treated as reinsurance only if it shifts genuine insurance risk. §845 authorizes the IRS to restate reinsurance arrangements that are aimed primarily at shifting profits. Unless otherwise noted, all section references in this paper are to the Internal Revenue Code of 1986, as amended.

[2] The House bill had proposed a different instrument, an excise tax on deductible payments to foreign related parties, on November 2, 2017 (U.S. House of Representatives, 2017).

insurance groups allocate risk and capital (Powell et al., 2008). Cross-border affiliate contracts can provide access to offshore risk-bearing capacity while changing where the associated income is taxed. The setting also offers detailed statutory information on reinsurance counterparties, retained underwriting exposure, and regulatory capital charges.

One potential benefit of offshore affiliate reinsurance is diversification. Pooling imperfectly correlated exposures can reduce capital needs, depending on tail dependence and the applicable capital framework. For example, combining U.S. hurricane exposure with Japanese earthquake exposure may allow a group to deploy capital more efficiently than holding each exposure separately. Affiliate reinsurance can also provide internal capacity when external coverage is costly or difficult to obtain. The same arrangement can generate tax benefits: deductible premiums paid by a U.S. insurer may move the associated income to an affiliate facing a lower tax rate. More broadly, Clausing (2020) documents substantial foreign profits booked by U.S. multinationals in jurisdictions such as the Cayman Islands, Switzerland, and Bermuda. Tax benefits and non-tax benefits can coexist within a single contract, making it difficult to infer its underlying motives from observed flows.

We assign treatment using the BEAT's two statutory thresholds: average gross receipts of at least $500 million over the preceding three years and a base erosion percentage of at least 3%. These criteria capture group size and the share of deductions associated with qualifying foreign-related-party payments. We fix treatment using 2017 values, before implementation, rather than subsequent tax payments or reinsurance choices. Our sample contains 26 groups meeting both thresholds and 22 control groups, observed from 2013 to 2022, for a total of 480 group-year observations. Together, these groups held 55.2% of U.S. property-casualty industry assets in 2017. Statutory exposure does not imply that every treated group subsequently owed BEAT, since liability also depends on taxable income and firms' adjustments. Our analysis proceeds in four parts.

First, we examine the aggregate response of affiliate reinsurance. Treated groups reduced cessions to foreign affiliates by approximately 7 percentage points of gross premiums written relative to controls. The decline appears in 2018 and persists through the end of our sample in 2022. We do not observe a further deepening when the statutory rate rises in 2019. External reinsurance provides limited replacement: the estimated increase in external cessions amounts to less than one-fourth of the decline in foreign-affiliate cessions. This pattern indicates substantial

tax sensitivity in affiliate reinsurance and is consistent with domestic retention becoming relatively more attractive after the reform. It does not establish that the displaced contracts lacked non-tax benefits.

A natural identification concern is that treated groups are larger and more internationally integrated than controls. Such differences may generate differential trends or different responses to concurrent TCJA provisions. We therefore complement the between-group estimates with a design comparing exposed and other counterparty channels within group-year, including group-by-type and type-by-year fixed effects. This design absorbs group-wide shocks, although it still requires the absence of concurrent shocks that differentially affect the exposed channel. We also restrict the sample using propensity scores based on pre-reform characteristics. The contraction remains evident across these alternative comparisons.

Second, we examine replacement through foreign affiliates that elect to be taxed as U.S. corporations under §953(d). The election allows an affiliate to remain offshore while subjecting it to U.S. taxation. It therefore preserves access to potential non-tax benefits of an offshore location while changing tax treatment. Continued cessions through an electing affiliate indicate that the arrangement remains attractive under U.S. taxation. However, the choice can reflect diversification, regulatory benefits, existing affiliate structures, and the costs of alternative arrangements. It does not provide a deterministic classification of contracts as tax motivated or risk motivated.

We hand-collect election status for affiliates in Bermuda, the Cayman Islands, and Switzerland. These jurisdictions combine substantial reinsurance activity with potentially favorable tax treatment and received 88.8% of foreign-affiliate cessions in our sample in 2017. To our knowledge, this is the first study to track these elections systematically.

Insurance groups choose electing and non-electing cessions jointly as part of their reinsurance programs. A regression of one quantity on the other would therefore be difficult to interpret. Instead, we estimate the reform's effect on each channel among groups that met the statutory thresholds and held a pre-reform non-electing affiliate in the jurisdictions we observe. The ratio of these estimated effects gives a policy-induced substitution ratio. Although electing cessions fall outside the BEAT base, the reform can change demand for them through the group's joint allocation decision. We consequently interpret the ratio as scaling two responses to the same policy shock, rather than as a structural causal effect of non-electing cessions on electing cessions.

The estimated increase in electing cessions is about one-third of the decline in non-electing cessions. Most of the decline was therefore not offset through the electing channel. Some displaced flows migrated to arm's-length foreign reinsurers, so the remainder is not a measure of total exit from foreign reinsurance. Nor does it identify the proportion of pre-reform contracts that existed solely for tax reasons. The result instead quantifies how much of the estimated contraction in the non-electing channel was replaced through U.S.-taxed offshore affiliates.

Replacement is concentrated among treated groups with high catastrophe exposure, measured using the premium-weighted hazard intensity of the states in which each group writes. Highly exposed groups sharply reduced non-electing cessions while increasing electing cessions from near zero to approximately 7.6% of gross premiums written. The low-exposure groups showed little corresponding increase. The estimated replacement share increases with catastrophe exposure, although the exposure gradient is imprecisely estimated. This pattern is consistent with greater benefits from offshore risk sharing among catastrophe-exposed groups, but does not uniquely identify diversification as a mechanism.

We also find no detectable change in gross underwriting or overall portfolio composition. Groups instead retain a larger share of the business they write. The increase in retention is more pronounced in long-tail lines than in catastrophe lines. Long-tail business can offer substantial tax benefits because reserves remain invested for extended periods before claims are paid (Seessel, 2001). The smaller retention response in catastrophe lines, together with the election results, is consistent with differences in tax sensitivity and non-tax benefits across lines.

Third, we examine domestic underwriting retention and regulatory capital measures. For treated groups with a high pre-reform base erosion percentage, the net-to-gross ratio of unpaid losses from the current accident year increased by 11.8 percentage points. We detect no significant changes in unpaid losses from earlier accident years, which generally remain covered by contracts established before the reform. The results indicate that U.S. insurers retained a larger share of new underwriting exposure, while providing no direct measure of the resulting change in overall insolvency risk.

We detect no statistically significant increase in recomputed required capital. To understand this response, we decompose capital charges into the credit charge on reinsurance recoverables and the underwriting charges on reserves and premiums. Lower recoverables reduce the credit charge, while greater premium retention increases the premium-risk charge. The estimated decline in the

credit charge approximately offsets the increase in the premium-risk charge.

Some reduction in the credit charge is economically warranted because lower recoverables reduce exposure to reinsurer default. Our standardized calculation applies the pre-2018 recoverables charge across years; this charge varies with recoverables volume rather than counterparty ratings. The capital formula also aggregates risk charges through a square root, attenuating changes in individual components. A mechanical counterfactual that holds recoverables relative to gross premiums written at pre-reform levels, while retaining other observed components, likewise produces no statistically significant increase in required capital. These estimates help explain the measured capital response, but do not establish that capital requirements are inadequate. They highlight a potential limitation of aggregate regulatory capital measures in capturing changes in the location and composition of retained risk.

Fourth, we estimate the fiscal effect of the BEAT for the groups in our sample. Official statistics report direct minimum-tax receipts, which were less than the initial projections (Internal Revenue Service, 2021; Joint Committee on Taxation, 2018). These receipts capture only part of the potential revenue effect because firms can respond by retaining income domestically or redirecting cessions to U.S.-taxed affiliates. Under a counterfactual in which treated groups maintain their 2017 cession ratios, we estimate BEAT liability of $12.1 billion over 2018–2022. The estimated liability for the observed behavior is $1.5 billion. Applying assumed profit margins and corporate tax rates to the estimated onshored premium volumes yields an additional $4.2 billion in federal tax, for a total estimated fiscal effect of $5.7 billion. The indirect revenue from the on-shored base is thus almost three times the direct minimum-tax liability, and the total fiscal effect is almost four times as large. Revenue evaluations of minimum taxes that count only direct tax receipts may therefore substantially understate its revenue effect. These are model-based calculations rather than observed tax payments and depend on counterfactual cession ratios, margins, and tax assumptions. They illustrate how behavioral responses can shift revenue from the minimum-tax base into the regular corporate-tax base.

We contribute to three strands of literature. First, research on internal capital markets emphasizes their ability to relax financing constraints (Desai et al., 2004), allocate capital to divisions with better investment opportunities (Khanna & Tice, 2001; Stein, 1997), and support weaker affiliates (Gopalan et al., 2007). Other work examines their role in profit shifting (Huizinga & Laeven, 2008;

Tørsløv et al., 2023) and agency conflicts (Lamont, 1997; Rajan et al., 2000; Scharfstein & Stein, 2000).[3] Closest to our setting, Ge (2022) studies how internal transfers transmit financial shocks across insurance divisions. We instead examine a tax-induced increase in the cost of cross-border affiliate reinsurance. We document its tax sensitivity, quantify replacement through U.S.-taxed offshore affiliates, and trace changes in domestic underwriting retention.

Second, we contribute to research on taxation and reinsurance. Reinsurance can smooth earnings and reduce expected taxes under a convex tax schedule (Mayers & Smith, 1982, 1990). Adiel (1996) finds no supporting evidence for a proposed relationship between marginal tax rates and reinsurance demand, whereas Adams et al. (2008) documents a relationship between tax rates and reinsurance use among U.K. life insurers. Related work examines offshore regulatory arbitrage (Koijen & Yogo, 2016) and tax savings from affiliate reinsurance (Hepfer et al., 2020). We study the group's aggregated U.S. property-casualty operations, reflecting the group-level organization of tax and reinsurance decisions (Powell & Sommer, 2007). Our closest reference is Tang (2022), who examines the BEAT's effects on reinsurance use and insurance prices using a median split of pre-reform reinsurance usage. We assign exposure using statutory thresholds and examine replacement through electing affiliates, domestic underwriting retention, and regulatory capital measures.

Third, we contribute to the literature on the gap between economic substance and regulatory appearance in the U.S. insurance. Oh et al. (2025) studies how rate regulation creates cross-subsidies across states, Abramson et al. (2025) examines how guaranty-fund funding arrangements affect insurers' supply following peer failures, and Sastry et al. (2026) documents discrepancies between insurer insolvency risk, guarantee fees, and some credit ratings. We examine tax policy as a source of changes in risk allocation. Greater domestic underwriting retention is accompanied by no statistically detectable increase in recomputed required capital. Our decomposition identifies offsetting capital charges and the attenuating effect of aggregation, highlighting a potential limitation of regulatory capital measures in capturing changes in retained exposure.

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting and the BEAT. Section 3 presents the data and sample. Section 4 explains the research design.

[3]Tørsløv et al. (2023) estimate that U.S. multinationals shifted more than half of their foreign profits to tax havens in 2015. Related evidence shows that patent filings and intangible investment respond to cross-country tax differences (Dischinger & Riedel, 2011; Karkinsky & Riedel, 2012).

Section 5 examines the aggregate reinsurance response. Section 6 estimates replacement through U.S.-taxed offshore affiliates and its relationship with catastrophe exposure. Section 7 examines domestic underwriting retention and regulatory capital measures. Section 8 estimates fiscal effects, and Section 9 concludes.

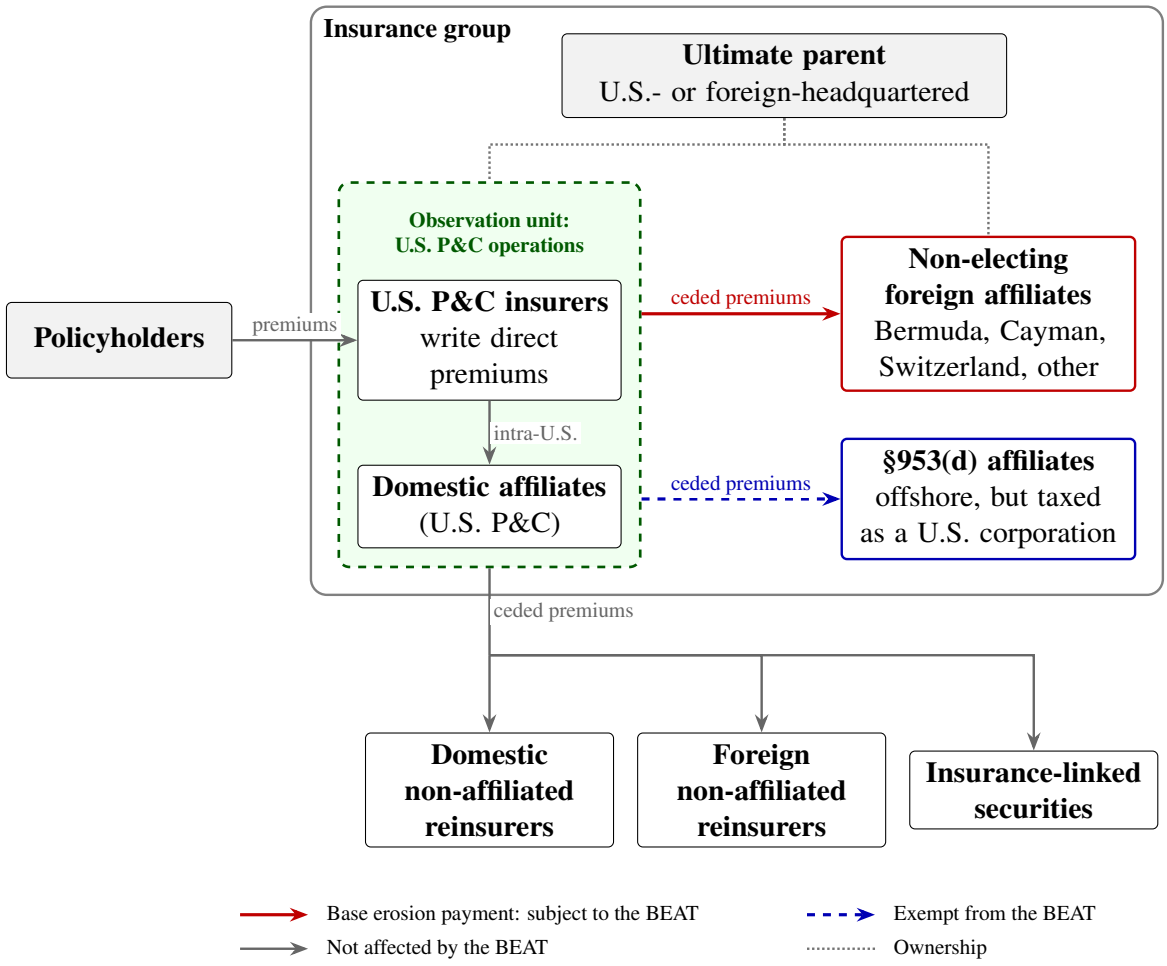
2. Institutional Background

Our setting is the U.S. property-casualty insurance market. The insurers that write business in this market belong to insurance groups, and each group determines the structure of its subsidiaries and the flows of premiums between them. Some of these groups are headquartered in the U.S., while others have a foreign ultimate parent, such as Chubb or Tokio Marine. In both cases, the U.S. insurance subsidiaries are subject to U.S. federal corporate income tax, and premiums they cede to affiliates are deductible. Where the ceded income is ultimately taxed, however, depends on where the group is headquartered. For a group with a U.S. parent, the offshore affiliates are generally controlled foreign corporations. Under the Subpart F rules, their insurance income is largely included in the U.S. parent's taxable income, so ceding premiums to an offshore affiliate does little to move income out of the U.S. tax base. For a group with a foreign parent, the offshore affiliates are not controlled by a U.S. shareholder and fall outside Subpart F. Ceding premiums from the U.S. subsidiary to an affiliate in Bermuda, the Cayman Islands, or Switzerland therefore moves the corresponding income out of U.S. taxation. Foreign-headquartered groups thus have a choice about where their income is taxed that U.S.-headquartered groups largely lack. This asymmetry is what the BEAT targets, and it shapes both how groups respond to the tax and which of them can use the §953(d) election discussed below. Figure 1 gives an overview of all reinsurance flows we observe and of our unit of observation: the group's aggregated U.S. P&C operations.

2.1 The BEAT as a Tax on Internal Capital Markets

Multinational insurance groups rely on internal reinsurance markets to overcome significant external frictions. Functioning as a mechanism for the internal reallocation of capital, these markets allow groups to deploy resources to subsidiaries where they are most efficiently used (Powell et al., 2008). External reinsurance markets tend to suffer from information asymmetries and moral hazard

Figure 1: Reinsurance Flows and Unit of Observation



This figure illustrates the reinsurance flows of an insurance group operating in the U.S. property-casualty (P&C) market. The ultimate parent can be U.S.-headquartered or foreign-headquartered. Our unit of observation (green dashed box) is the group's U.S. P&C operations: we aggregate the statutory filings of all U.S. P&C subsidiaries to the group level. Policyholders pay premiums to the U.S. insurers, which cede part of these premiums to reinsurers. The red solid arrow marks cessions to non-electing foreign affiliates; these are base erosion payments and subject to the BEAT. The blue dashed arrow marks cessions to foreign affiliates that elected under §953(d) to be taxed as U.S. corporations; these are exempt from the BEAT. Gray arrows mark cessions to reinsurers that are not part of the insurance groups and are therefore not affected by the BEAT. We distinguish between domestic and foreign non-affiliated reinsurers, as well as insurance-linked securities vehicles. All flows are observed in Schedule F of the statutory filings.

(Jean-Baptiste & Santomero, 2000; Yan & Hong, 2015). Standard economic theory predicts these to be less of an issue in internal reinsurance markets. By operating under a unified structure with aligned interests, groups avoid the risk premiums embedded in third-party pricing and benefit from significantly lower transaction costs (Cragg et al., 2017; Park et al., 2021). Furthermore, these internal transfers have historically offered a tax advantage: by shifting premiums to low-tax jurisdictions, firms could minimize their global tax burden (see e.g., Cummins & Grace, 1994; Garven & Louberge, 1996; Mayers & Smith, 1990; Powell & Sommer, 2007).

The 2017 TCJA disrupted this equilibrium by introducing the BEAT. Designed to curb profit shifting through deductible payments to foreign related parties, the BEAT reclassifies cross-border affiliate reinsurance premiums as base erosion payments that must be added back in computing modified taxable income.[4] This effectively subjects these flows to a minimum tax, which phased in at 5% in 2018, rose to 10% for 2019–2025, and has reached a rate of 10.5% for 2026 and beyond.[5] By establishing a minimum tax on offshore transfers, the policy acts as an exogenous shock to the cost of internal capital. This forces affected insurance groups to adjust their internal capital allocations and move toward a new equilibrium. Their response reveals their true demand for internal risk pooling. Do they keep these transfers because the operational benefits, such as diversification and lower capital frictions, outweigh the new tax cost? Or do they abandon them because the flows were purely tax-motivated?

The BEAT was not designed for insurers specifically. Its stated purpose was to prevent large multinational corporations from reducing their U.S. tax liability through deductible payments to foreign related parties (Internal Revenue Service, 2019). According to the U.S. Senate Committee on the Budget (2017), the provision "aims to level the playing field between U.S. and foreign-owned multinational corporations in an administrable way" (p. 396). The committee was concerned about the ability of "profitable corporations with significant base erosion payments to avoid virtually all tax liability in the reformed corporate tax system" (p. 396). The base covers interest, royalties, service fees, and reinsurance premiums alike.

[4]Specifically, modified taxable income is multiplied by the BEAT rate. The resulting amount is compared with the insurance group's regular tax liability. The insurance group owes incremental BEAT only when the former exceeds the latter.

[5]See §59A(b)(1)(A).

2.2 The §953(d) Election: Decoupling Tax from Operation

The central empirical challenge is that observed reinsurance flows are a bundled product of genuine risk management and tax optimization. To separate these two motives, we use a special feature of the U.S. tax code: the §953(d) election. Under §953(d), a foreign insurance affiliate can elect to be treated as a domestic U.S. taxpayer for federal income tax purposes while maintaining its physical operations and regulatory domicile offshore (see e.g., Flum et al., 2016; Forni et al., 2025).

We focus on affiliated §953(d) entities located in Bermuda, Switzerland, and the Cayman Islands. The reason is their dual role in the global insurance business. On one hand, these jurisdictions have long served as prominent tax havens. Their statutory corporate tax rates are low or zero, which creates large tax-arbitrage opportunities for multinational groups (Seessel, 2001).[6] On the other hand, these jurisdictions are global reinsurance hubs for property-casualty insurance. They are the largest foreign destination for U.S. cessions and the central hub for global catastrophe capacity (Cragg et al., 2017). The three jurisdictions account for 88.8% of affiliated reinsurance cessions in our sample. Their prominence reflects a lighter and more flexible regulatory regime, which allows efficient capital management and risk pooling that is difficult to replicate in the U.S. domestic market (OECD, 2018). Catastrophe risk relies on global diversification, which domestic U.S. affiliates cannot easily replicate. As a result, these groups face large frictions when they try to reduce their international risk transfers.

The §953(d) election serves as a decoupling mechanism for our empirical analysis. An insurance group's internal capital market consists of domestic and foreign affiliates, and it operates alongside external markets: unaffiliated reinsurance and insurance-linked securities. The BEAT raises the price of reinsurance cessions to foreign affiliates only. These become base erosion payments, whereas those to domestic affiliates do not, and neither do payments to unaffiliated counterparties. Therefore, the BEAT changes the relative price of foreign affiliated reinsurance to the other available reinsurance markets. An insurance group facing the higher price can absorb it, move the risk to its domestic U.S. affiliates, move it outside the group, or remove the reinsurance transfers from the BEAT base by electing §953(d). The latter keeps the foreign affiliate offshore but taxes its

[6]The federal corporate tax rate in Switzerland is 8.5%. Bermuda and the Cayman Islands did not impose any corporate income tax during our sample period (2013–2022). See the OECD data set on statutory corporate income tax rates.

worldwide income at the U.S. rate.[7] We observe all four margins and test each in Sections 5 through 7.

We track §953(d) elections and the reinsurance flows directed to these entities after the BEAT. An insurance group does not necessarily have to establish a new electing affiliate; it can also route additional premiums to an existing §953(d) affiliate. Either route produces the same revealed preference: as the election leaves the foreign affiliate's domicile, capital structure and regulatory regime untouched while removing its tax advantage, a group that keeps ceding through an electing affiliate reveals that the *internal* offshore link is worth full U.S. taxation. The converse does not hold. A group that cut its non-electing cessions may have moved the coverage to an external reinsurer rather than abandoned it. What the contraction reveals is narrower: once the tax differential was gone, the internal affiliate link was no longer worth its cost relative to the group's other options.

The question that arises is which insurance groups held §953(d) affiliates before the BEAT. These are mainly U.S.-headquartered groups. The reason lies in U.S. tax law: §953(d)(1)(A) requires the electing entity to be a controlled foreign corporation, and under Subpart F the net income of such a corporation is added back to the U.S. parent. Affiliates eligible to elect therefore do not generally serve as a means to shift profits overseas in the first place (see Hepfer et al., 2025).[8] For foreign-headquartered groups such as Tokio Marine or Chubb, existing §953(d) entities before the BEAT mainly reflect previous M&A activities. Since §953(d) elections are irrevocable, §953(d) entities will keep their election after the transaction. These entities were nonetheless little used before the reform. Among treated groups holding at least one non-electing affiliate in Bermuda, the Cayman Islands, or Switzerland ($Treated^{\text{aff}}$), premiums ceded to §953(d) entities averaged 0.93% of gross premiums written over the pre-BEAT period, against 3.8% for the remaining groups in our sample.

For insurance groups holding §953(d) affiliates before the reform, the response was to redirect reinsurance flows to an entity that already existed rather than to establish one. For insurance groups not holding §953(d) affiliates before the reform, the response was to establish a §953(d) entity and redirect reinsurance flows to it directly. We do not distinguish redirection to an existing electing

[7] Additionally, the §953(d) election eliminates exposure to the Federal Excise Tax (FET) under §4371, which levies a 4% charge on direct insurance and 1% on reinsurance written with U.S. cedants.

[8] Classification as a controlled foreign corporation depends on whether a U.S. corporation controls the foreign affiliate. See §951(a), §953(c), §957, and Hepfer et al. (2020) for details.

affiliate, restructuring that enables an election, or a newly established entity. In each case the receiving entity's income is subject to U.S. federal tax, so the group accepts the tax cost to preserve an offshore risk-transfer relationship.

One might think that BEAT-affected groups have no incentive to keep ceding to §953(d) affiliates, since the BEAT rate sits well below the U.S. statutory rate, which dropped to 21% under the TCJA. At the same time, the BEAT rate sits well above the rates of low-tax jurisdictions such as Bermuda. This intuition is misleading. Tax-deductible cessions to foreign affiliates account for a median of 22.3% of gross premiums written by affected insurers over 2013–2017, so the BEAT applies to a substantially larger tax base. We illustrate that the typical treated insurer is better off making the election than paying the BEAT. We first set out a simple accounting framework that separates the tax rate from the tax base in each regime, and then evaluate it at treatment-group medians over 2013–2017.

Consider a global insurance group that consists of U.S. operations and a foreign affiliate. We evaluate the group's worldwide liability rather than the U.S. entity's alone, since the §953(d) election trades a lower foreign tax bill against a higher domestic one and is made at the group level. The U.S. operations write gross premiums P and cede a share a to the foreign affiliate, u to unaffiliated reinsurers, and d to a domestic affiliate. Let m denote the pre-tax margin on premiums retained inside the U.S., τ the effective U.S. tax rate, b the BEAT rate, and t_f the tax rate in the affiliate's jurisdiction, with $t_f < \tau$. The affiliate earns an underwriting margin μ_u per dollar of assumed premium and investment income μ_i per dollar. The latter arises from the reserves and flows of the assumed business. The U.S. operations file a consolidated tax return; therefore, cessions to a domestic affiliate are internal transfers that cancel out and do not change the group's U.S. tax base. Also, they never enter the BEAT base as they are not base erosion payments. The tax-relevant quantity is thus the premium retained inside the U.S., $N \equiv (1 - a - u)P$, regardless of how it is allocated across domestic entities.

Conditional on the U.S. operations of the group being subject to the BEAT, its worldwide tax

liability before the reform, under the BEAT, and under the BEAT with a §953(d) election is

$$\begin{aligned} T^{\text{pre}} &= \tau\, mN \;+\; t_f(\mu_u + \mu_i)\, aP, \\ T^{\text{BEAT}} &= b\,(mN + aP) \;+\; t_f(\mu_u + \mu_i)\, aP, \\ T^{953} &= \tau\,\big(mN + \mu_u\, aP + \mu_i\, aP\big). \end{aligned} \tag{1}$$

With the election, the foreign affiliate becomes a U.S. taxpayer on its worldwide income, so both its underwriting and its investment result are taxed at τ. No separate foreign term survives. This is the full cost of electing. In the other case, the U.S. operations would pay the BEAT on its income and the foreign affiliate's income would remain outside the BEAT base. Now, the group chooses the lower of the two post-reform tax liabilities.

Under the BEAT the full dollar is added back and taxed at b; under the election only the affiliate's margin $\mu_u + \mu_i$ is taxed, and only at the incremental rate $\tau - t_f$, since the affiliate already pays t_f abroad. Electing therefore saves $b - (\tau - t_f)(\mu_u + \mu_i)$ on each dollar ceded. Set against this base saving is a rate penalty on domestic income. When the minimum tax binds, the BEAT rate applies to the group's entire modified base, so domestic income is taxed at b rather than at τ. Writing $T^{953} < T^{\text{BEAT}}$ and collecting terms gives

$$\underbrace{(\tau - b)\, mN}_{\text{rate penalty}} \;<\; \underbrace{aP\,\big[\, b - (\tau - t_f)(\mu_u + \mu_i)\,\big]}_{\text{base saving}}. \tag{2}$$

The left-hand side is the cost of electing §953(d) and the right-hand side is the benefit. The cost is that the election gives up the low BEAT rate on domestic income, which the group would otherwise pay on all of mN. The benefit is that the election shrinks the taxable base on the ceded premium. It replaces a tax on the full dollar with a tax on the affiliate's margin alone. The group elects when the second outweighs the first. We can rewrite this inequality to

$$\kappa \;\equiv\; \frac{aP}{mN} \;>\; \kappa^{*} \;\equiv\; \frac{\tau - b}{b - (\tau - t_f)(\mu_u + \mu_i)}. \tag{3}$$

The left-hand side is the size of the add-back (base erosion payments) relative to the regular tax base. The right-hand side is the ratio of the rate penalty to the base saving. Two comparative statics

follow. The threshold κ^* rises in the affiliate's total margin, since a more profitable affiliate is more expensive to bring into the U.S. tax base. Additionally, it falls in t_f, since an affiliate already taxed abroad is cheaper to bring in. Appendix A derives Equations (2) and (3) and states the conditions under which the comparison is well defined. Of the two components of the affiliate's margin, μ_i is the one that varies systematically across lines of business. It scales with the reserves a dollar of premium generates, so it is large in long-tail business, where reserves compound offshore for years. Electing is therefore most costly where the offshore tax benefit is largest, which anticipates the repatriation pattern we document in Section 7.1.

At treatment-group medians the election dominates.[9] With $t_f = 0$ for Bermuda and the Cayman Islands, each dollar ceded costs 10 cents under the BEAT against 4.4 cents under the election, a saving of 5.6 cents. Each dollar of domestic income, by contrast, costs 10 cents under the BEAT against 17 cents under the election, a penalty of 7 cents. Since ceded premium is 2.65 times domestic pre-tax income, the saving applies to a much larger base: \$12.4 against \$5.9, leaving the election \$6.5 cheaper per \$1,000 written.[10] Formally, $\kappa = 2.65$ against a threshold of $\kappa^* = 1.26$, or 0.97 if the affiliate's investment income is set aside. For Switzerland, where $t_f = 8.5\%$, the two thresholds are 0.90 and 0.81. Equivalently, holding the other parameters at their medians, the election dominates for any group ceding more than roughly 10 to 12 percent of gross premiums to a foreign affiliate, and the median treated group cedes 22.3 percent.[11]

[9]Parameters are treatment-group medians over 2013–2017: $a = 22.3\%$, $u = 11.5\%$, $m = 12.7\%$ (EBT/NPW), and $\tau = 17.0\%$ (effective tax rate). The BEAT rate is the mid-term statutory rate $b = 10\%$. For the affiliate we use aggregate Bermudian figures for Class 4 and Class 3B reinsurers published by the Bermuda Monetary Authority (2018) for the pre-BEAT period: a combined ratio of 83.6% implies $\mu_u = 16.4\%$, and investment income of 9.7% of net premiums written gives $\mu_i = 9.7\%$. Note that m is already a total pre-tax margin and so includes investment income on the U.S. book. We ignore the Federal Excise Tax that the election also removes, which makes the election look more expensive than it is.

[10]The per-dollar figures follow from the structure of §59A. The group computes regular tax and it computes b times modified taxable income (regular taxable income *plus* the add-back), and pays the greater of the two. When the second exceeds the first, regular tax is displaced entirely, so every dollar in the modified base is taxed at b. Hence the BEAT liability of $\$30.7 = 0.10 \times (84.1 + 223)$ contains $0.10 \times 84.1 = \$8.4$ on domestic income and $0.10 \times 223 = \$22.3$ on the cession. Under the election, the add-back vanishes and the group returns to regular tax. Domestic income is taxed at τ, giving $0.170 \times 84.1 = \$14.3$, while the affiliate's profit of $\mu\, aP = 0.261 \times 223 = \$58.2$ enters the U.S. base at τ rather than t_f, giving $0.170 \times 58.2 = \$9.9$, for a total of \$24.2. Per dollar of domestic income the election therefore costs $0.170 - 0.100 = 7$ cents more, and per dollar ceded it costs $(\tau - t_f)\mu = 0.170 \times 0.261 = 4.4$ cents against $b = 10$ cents, a saving of 5.6 cents. Multiplying by the two bases gives a penalty of $0.07 \times 84.1 = \$5.9$ and a saving of $0.056 \times 223 = \$12.4$, so the election is \$6.5 cheaper, which is $\$30.7 - \$24.2$.

[11]Scaling to $P = \$1{,}000$: net premiums retained in the U.S. are $N = \$662$ and domestic pre-tax income is $mN = \$84.1$, so $T^{\text{pre}} = 0.170 \times 84.1 = \$14.3$. The BEAT adds back $aP = \$223$, giving $T^{\text{BEAT}} = 0.10 \times (84.1 + 223) = \$30.7$. Under election, affiliate underwriting profit is $0.164 \times 223 = \$36.6$ and investment income is $0.097 \times 223 = \$21.6$, so $T^{953} = 0.170 \times (84.1 + 36.6 + 21.6) = \$24.2$; excluding the investment result gives \$20.5.

The comparison uses the group's effective tax rate throughout, including on the income the affiliate brings into the U.S. base once it elects. A newly electing affiliate may face a rate closer to statutory, since it holds none of the carryforwards or tax-exempt positions that lower a domestic insurer's effective rate. Applying the statutory 21% to the affiliate's income while leaving domestic income at the observed effective rate raises κ^* to 1.55 and the critical cession share to 14.5%, still well below the observed 22.3%; group liability under election rises to $26.5, against $30.7 under the BEAT. The election dominates because of the base, not the rate. It trades a 7 percentage point higher rate for a taxable base roughly four times smaller.

Equation (1) also shows what the BEAT does *not* do. Reinsurance cessions to domestic affiliates enter none of the three expressions, because they neither change the U.S. base nor qualify as base erosion payments. The tax therefore gives BEAT-exposed groups no reason to move flows into the domestic internal market, and no reason to move them out. The framework thus predicts a null on the domestic affiliate channel, which is what we find in Section 5.

One caveat applies to this example. The §953(d) election subjects the affiliate's entire worldwide income to U.S. federal tax, not just the income from the U.S. risks it reinsures. Our calculation captures the election's cost accurately only when the affiliate's book consists primarily of U.S. business. For an affiliate that also reinsures substantial non-U.S. risk, electing would pull that foreign income into the U.S. tax base as well, raising the cost of election and narrowing the gap we describe.

2.3 The Statutory Thresholds

Our empirical strategy relies on the two-pronged statutory test defined in §59A(e), which creates discrete thresholds for tax liability. To be subject to the BEAT, an insurance group must meet two specific criteria simultaneously: (1) **The Gross Receipts Test**: The insurance group must have average annual gross receipts of at least $500 million over the three preceding tax years. (2) **The Base Erosion Percentage Test**: The group's base erosion payments (e.g., cross-border affiliate reinsurance premiums) divided by its total deductible payments must meet or exceed 3%.[12] In

In cession-share terms the threshold is $a^* = \kappa^* m(1-u)/(1+\kappa^* m)$, which equals 12.2% and 9.7% of gross premiums under the two values of κ^*.

[12]For insurers who meet these thresholds, the BEAT liability is calculated on a modified taxable income base that adds back base erosion payments.

essence, the two tests capture two distinct dimensions: the Gross Receipts Test measures firm size, while the Base Erosion Percentage Test measures the extent of offshore reinsurance cession. In Table A1 of the Appendix, we provide illustrative calculations of gross receipts and base erosion percentages. Using actual statutory thresholds, we improve the measurement compared to the median-split classifications used in Tang (2022).

2.4 The NAIC Risk-Based Capital Framework

U.S. insurers operate under the National Association of Insurance Commissioners (NAIC) Risk-Based Capital (RBC) framework. Unlike model-based regimes such as Solvency II and the Swiss Solvency Test, the NAIC system utilizes a factor-based approach, applying fixed risk charges to specific balance sheet items to calculate the Authorized Control Level (ACL). This number represents the minimum capital that insurers are required to hold for operation (Cummins & Phillips, 2009; NAIC, 2012). The formula sorts risk into six components: the risk of affiliated insurance subsidiaries (R0), asset risk on fixed-income investments (R1) and on equity investments (R2), credit risk, primarily on reinsurance recoverables (R3), reserve risk, the risk that booked reserves prove inadequate (R4), and premium risk, the risk that the coming year's premiums fail to cover the coming year's losses (R5). These components are not added directly. The framework combines them through a square root:

$$\text{ACL} = 0.5 \times \left(\text{R0} + \sqrt{\text{R1}^2 + \text{R2}^2 + \text{R3}^2 + \text{R4}^2 + \text{R5}^2} \right).$$

The RBC ratio is then total adjusted capital divided by the ACL.[13]

A central feature for our analysis is how the framework treats reinsurance recoverables. Until 2017, the RBC formula applied a flat charge of 10% to recoverables from unaffiliated and foreign affiliated reinsurers, split equally between R3 and R4. Recoverables from domestic affiliates were not charged. The factor did not vary with the reinsurer's credit rating. The charge therefore tracked the volume of recoverables rather than their credit quality.[14] Because of the split and the square-root

[13] Supervisory actions escalate as the ratio falls below prescribed thresholds (no action above 200%, regulatory control below 100%) (Grace et al., 1998; NAIC, 2012).

[14] This uniform treatment, which makes no distinction by counterparty rating, has been criticized on that ground (Odomirok et al., 2015).

aggregation, a dollar of recoverables adds considerably less than ten cents to the Authorized Control Level. Beginning in 2018, the NAIC replaced the flat factor with rating-based factors. To separate the effect of the BEAT from this regulatory change, we apply the pre-2018 rule to the credit charge in all years (Section 7.2). As the charge falls with the volume of recoverables, a firm that reduces its cessions also reduces its credit charge, while the risk it retains raises its underwriting charges. If the two effects offset each other, required capital can remain unchanged even though the firm keeps more underwriting risk on its own balance sheet. We test this in Section 7.2.

3. Data and Descriptive Statistics

3.1 Sample Construction and Summary Statistics

To analyze the impact of tax frictions on internal capital mobility, we construct a comprehensive panel dataset of insurance groups operating in the U.S. property-casualty market. The sample includes both U.S.-headquartered and foreign-headquartered groups. For each group, we aggregate the statutory filings of its U.S. property-casualty subsidiaries. We focus on the insurance group as the unit of analysis rather than individual insurance companies because capital allocation strategies are typically coordinated at the consolidated level (Cummins et al., 2021).[15] Additionally, the BEAT liability is determined using consolidated group-level metrics: both gross receipts and the base erosion percentage are calculated for the aggregate affiliated group, not for individual subsidiaries.[16]

Our primary data source is NAIC statutory filings accessed through the S&P Global Market Intelligence platform. These filings contain detailed annual information on direct and gross premiums written, counterparty-specific reinsurance transactions (Schedule F), information on insurance losses (Schedule P), as well as the ownership structure of the insurance group (Schedule Y Part 1A). The S&P Global Market Intelligence platform assigns subsidiaries to groups based on the most recent Schedule Y filing rather than the filing contemporaneous with each observation year. Taking these adjusted numbers would bias the results as we would not necessarily identify insurance groups that were actually subject to the BEAT in 2017.[17] We therefore reconstruct historical group composition

[15] Group members are often assigned specialized roles such as writing particular lines, serving specific regions, or acting as internal reinsurers to absorb catastrophe risk (Powell & Sommer, 2007).

[16] See §59A(e)(3). The consolidated computation prevents corporations from splitting up activities among affiliates to fall below the thresholds (Yang & Lauricella, 2020).

[17] For example, suppose insurance group A has a subsidiary C that, in every year from 2013 to 2023, exceeds both the

from annual Schedule Y filings directly, ensuring that treatment assignment reflects each group's actual structure in 2017.

We supplement this standard data with a unique dataset on the tax status of affiliates in Bermuda, the Cayman Islands, and Switzerland. The three jurisdictions account for 88.8% of affiliated reinsurance cessions in our sample. Because standard databases do not track tax elections, we manually collect the §953(d) status for every affiliate located in these countries. We use three strategies to identify the §953(d) status. First, for Bermudian affiliates, we extract election status from Bermuda Monetary Authority (BMA) financial statements.[18] A §953(d) election materially affects a Bermudian affiliate's statutory filing because it subjects the entity's worldwide income to U.S. taxation. Affiliates therefore disclose their election status explicitly in their Bermudian financial statement. Second, we check the notes to the financial statements in NAIC filings, which typically list all affiliated entities with which the filing entity files a consolidated U.S. tax return. For a foreign entity to be included in a consolidated U.S. tax return, it must be treated as a domestic corporation for U.S. tax purposes. The mechanism that achieves this for foreign insurance companies is the §953(d) election. We therefore classify any foreign affiliate identified in these consolidated tax return filings as a §953(d) entity.[19] Third, we verify the controlled foreign corporation (CFC) status of all foreign affiliates in our sample using historical Schedule Y filings. Because §953(d)(1)(A) requires the electing entity to be a CFC, any foreign affiliate that does not qualify as a CFC cannot have elected §953(d) status and is assigned a value of zero. This restriction also shapes how we interpret the substitution we document. Because a non-CFC affiliate cannot itself elect, the post-BEAT increase in §953(d) cessions need not reflect an election by the specific affiliate whose cessions declined. It may instead reflect redirection of flow to an affiliate that had already elected, an ownership restructuring that allows the affiliate to make the election, or the use of a newly

$500 million gross receipts threshold and the 3% base erosion percentage, making it subject to the BEAT. In 2024, group B acquires C from A. When extracting data from the platform in 2026, the platform aggregates all historical figures based on the 2025 Schedule Y, attributing C's full history to B. As a result, B appears to meet the BEAT thresholds in 2017 and is flagged as treated, while A falls below the thresholds and is assigned to the control group.

[18] https://www.bma.bm/public-filing-overview. Financial statements for Classes 3A, C, and D are available from 2016, and Class E from 2017. For earlier years, we rely on the NAIC consolidated tax return filings and the CFC screen described in strategies two and three, which are available for the full sample period and yield coverage of 98.8% of total premiums ceded to affiliates in the three countries in 2017.

[19] We also include affiliated reinsurance cessions to Swiss Re (Switzerland) as §953(d) cessions. In its 2019 consolidated annual report, Swiss Reinsurance Company Ltd (2020) confirms that it has restructured its internal capital market by creating a U.S. tax branch.

established entity. We observe cessions to §953(d) entities at the group level and do not distinguish between these routes. In each case, however, the foreign entity's income is subject to U.S. federal taxation, so the group accepts the tax cost in order to preserve an offshore risk-transfer relationship.

To the best of our knowledge, this is the first academic study to systematically track these elections. Each year, we identify whether a foreign affiliate falls under §953(d) and aggregate premiums ceded to affiliates with and without that status. Our data cover 98.8% of total premiums ceded by insurance groups in our sample to foreign affiliates in the three countries in 2017, providing comprehensive coverage of the relevant markets.

Starting with the universe of all NAIC-active property-casualty primary insurers and reinsurers, we exclude entities with non-positive direct premiums or those that do not engage in foreign or domestic affiliate reinsurance.[20] This affiliate requirement is central to constructing a comparable control group, and it does three things. First, it removes domestic-only firms that were mechanically unable to reduce cross-border cessions, so the treatment effect is not identified off firms that had no affiliate margin to adjust in the first place. Second, it restricts the sample to groups that operate an internal reinsurance network on both sides of the border, which is the setting our question is about. These are the firms that can shift risk across affiliates and allocate it to the entity best suited to hold it, so a change in the cost of one channel reveals something about how they use the network rather than about whether they have one. Third, maintaining affiliates of both types is a proxy for scale. Larger, more organizationally complex insurers are the ones that set up affiliate structures. The filter, therefore, narrows the size gap between treated and control groups. Within this filtered universe, we assign treatment using the statutory thresholds defined in the TCJA. We classify a group as treated ($Treated^{\text{stat}}$) if, in the pre-reform year of 2017, it meets both criteria of having average gross receipts of at least $500 million and a base erosion percentage of at least 3%. The control group consists of the remaining groups, that is, those failing at least one of the two tests. Controls are the full set of insurers that operate an affiliate reinsurance network but fall outside the statute. The comparison, therefore, holds the presence of an internal market fixed and varies only exposure to the tax. A central challenge is that BEAT-affected firms are inherently larger and more global than the average U.S. insurer. Therefore, we will add several control variables in our regressions and

[20] We classify an insurance group as having foreign (domestic) affiliates if it ceded premiums to a foreign (domestic) affiliate in 2017.

assess the overlap between the two groups explicitly through the propensity-score samples reported in Table 5. The final sample consists of 48 insurance groups (26 treated, 22 control) covering the period 2013 to 2022. We provide a list of all insurers in the treatment and control group, including their asset size, in Table A2 of the Appendix. This sample is highly representative of the aggregate U.S. insurance industry, covering 55.2% of total industry assets in 2017.

Table 1 provides the summary statistics for the final sample in the pre-treatment period. Internal reinsurance runs through domestic and foreign markets, and the domestic one is the larger. Insurers ceded on average 45% of gross premiums written to domestic affiliates, against 18% to foreign affiliates and approximately 7% to each unaffiliated market.[21] The two internal markets differ in what they accomplish. A domestic cession reallocates risk and capital across U.S. entities but leaves the U.S. group's consolidated position unchanged. A foreign cession moves risk and capital outside the U.S. and, therefore, changes the consolidated position of the U.S. group. On average, insurers ceded 18% of gross premiums written to foreign affiliates. Treated groups ceded on average 23% of gross premiums written to foreign affiliates, compared to 12% for control groups, a difference that is significant at the 5% level and reflects the higher offshore integration that generates BEAT exposure in the first place. On the domestic side they are statistically indistinguishable (43% versus 48%), as they are in both unaffiliated markets. As expected, treated groups are larger (mean log assets of 22.96 versus 21.75) and less concentrated in their line-of-business mix (LOB Concentration of 0.22 versus 0.36), both significant at the 5% level, consistent with a broader market scope. As there are systematic differences between both groups, we control for these variables in our regressions to account for any imbalance.

3.2 Measuring Capital Flows, Risk, and Solvency

To analyze the effects of the BEAT on internal capital markets, we construct several variables that measure different dimensions among insurance groups.

Reinsurance and Capital Flows. Our primary dependent variables are the ratios of reinsurance premiums ceded to specific counterparties, including foreign affiliates, foreign non-affiliates, domestic affiliates, and domestic non-affiliates, scaled by gross premiums written (GPW). We utilize

[21]The domestic ratio exceeds one for some group-years because intra-group pooling agreements cede and re-assume the same underlying premium across members.

Table 1: Summary Statistics

This table reports descriptive statistics for the main variables used in the analysis. Statistics are computed over 240 group-year observations covering 48 insurance groups (26 treated and 22 control) during the pre-reform period 2013–2017. All variables are calculated at the consolidated group level. Treated groups ($Treated^{\text{stat}}$) are identified ex ante based on 2017 statutory thresholds: average annual gross receipts $\geq$ \$500 million and a base erosion percentage $\geq 3\%$. The column *Diff.* reports the difference in group means between treated and control groups, with statistical significance assessed from a regression of each variable on the treatment indicator with standard errors clustered at the insurance group level. Continuous variables are winsorized at the 5th and 95th percentiles, separately for the treated and control group; reported minima and maxima are therefore post-winsorization bounds. Variable definitions are provided in Table A3.

	Total Sample (N = 48)				Treatment (N = 26)				Control (N = 22)				Diff.
	Mean	St. Dev.	Min	Max	Mean	St. Dev.	Min	Max	Mean	St. Dev.	Min	Max	(T − C)
Reinsurance Foreign Affiliates	0.18	0.17	0	0.53	0.23	0.17	0	0.53	0.12	0.15	0	0.41	0.12**
Reinsurance Domestic Affiliates	0.45	0.39	0.01	1.45	0.43	0.37	0.03	1.45	0.48	0.41	0.01	1.33	-0.05
Reinsurance Foreign Non-Affiliates	0.07	0.07	0	0.28	0.07	0.06	0.01	0.24	0.08	0.09	0	0.28	-0.01
Reinsurance Domestic Non-Affiliates	0.07	0.07	0	0.32	0.07	0.06	0	0.22	0.07	0.08	0	0.32	-0.01
Reinsurance Total Non-Affiliates	0.15	0.13	0	0.57	0.14	0.1	0.01	0.38	0.16	0.16	0	0.57	-0.02
ILS / GPW	0	0.03	0	0.19	0.01	0.03	0	0.19	0	0	0	0.02	0.01**
Base Erosion Percentage	0.23	0.23	0	0.96	0.28	0.22	0	0.96	0.17	0.23	0	0.75	0.11*
Gross Premiums Written[a]	5.77	8.33	0.03	45.3	6.08	7.12	0.27	35.53	5.4	9.59	0.03	45.3	0.67
Direct Premiums Written[a]	4.93	7.59	0.01	38.84	5.02	7.01	0.01	33.83	4.83	8.26	0.03	38.84	0.18
Net Premiums Written[a]	4.1	7.4	-1.1	46.44	3.77	5.45	0.18	28.63	4.48	9.19	-1.1	46.44	-0.71
Size	22.41	1.72	18.73	25.96	22.96	0.96	21.49	25	21.75	2.14	18.73	25.96	1.21**
Return on Equity	0.09	0.08	-0.09	0.33	0.08	0.06	-0.09	0.19	0.11	0.09	-0.03	0.33	-0.03*
Leverage	0.33	0.09	0.18	0.57	0.31	0.08	0.21	0.57	0.35	0.1	0.18	0.53	-0.04
LOB Concentration	0.29	0.24	0.1	1	0.22	0.15	0.1	0.6	0.36	0.3	0.1	1	-0.14**
CatRisk	0.02	0.02	0	0.05	0.02	0.02	0	0.05	0.02	0.02	0	0.05	0.01
Gross Unpaid Losses	0.2	0.1	0.02	0.43	0.21	0.09	0.02	0.38	0.19	0.11	0.06	0.43	0.02
Ceded Unpaid Losses	0.09	0.08	0	0.26	0.1	0.08	0.01	0.25	0.08	0.08	0	0.26	0.02
Net Unpaid Losses	0.11	0.05	0.01	0.21	0.11	0.04	0.01	0.2	0.1	0.06	0.02	0.21	0.01
Credit Charge	0.03	0.03	0	0.1	0.03	0.02	0.01	0.08	0.03	0.03	0	0.1	0
Overdue Recoverables	0.12	0.15	0	0.53	0.1	0.12	0	0.4	0.15	0.17	0	0.53	-0.05
Reserve Charge	0.05	0.03	0	0.12	0.06	0.03	0.01	0.12	0.04	0.03	0	0.11	0.02**
R5 Charge	0.04	0.02	0.01	0.1	0.04	0.02	0.01	0.09	0.04	0.03	0.01	0.1	0
Underwriting Charge	0.1	0.04	0.03	0.19	0.1	0.04	0.03	0.19	0.09	0.04	0.04	0.17	0.02
ACL / Assets	0.06	0.02	0.02	0.11	0.06	0.02	0.02	0.1	0.06	0.03	0.02	0.11	0.01

Notes: [a] Amounts in USD billions.

GPW as the denominator to ensure that reinsurance activity is appropriately scaled to the size of the underwriting portfolio, particularly since our sample includes both primary insurers and reinsurers.

Treatment Intensity. While the statutory threshold for the BEAT is a base erosion percentage (BEP) of 3%, the economic incentive to restructure varies with the intensity of exposure. A higher base erosion percentage implies that a larger share of total deductions consists of cross-border affiliate payments, so the BEAT penalty bears on a greater fraction of the firm's tax base. To capture this heterogeneity, we split treated insurers into two groups at the median 2017 base erosion percentage. The resulting $LowBEP$ and $HighBEP$ groups have substantially different pre-reform BEAT exposure, with mean base erosion percentages of 10.3% and 50.0%, respectively. We exploit this variation to test whether the contraction in internal capital markets is more pronounced for firms facing a steeper tax penalty. Figure A1 of the Appendix illustrates the distribution of the base erosion percentage in 2017 for the treatment and the control group.

Catastrophe Risk. We use two distinct measures of catastrophe risk, which capture different objects and serve different purposes. $CatRisk$ measures what a group writes. It is the share of direct premiums written in catastrophe-prone business lines within catastrophe-prone states.[22] Catastrophe-prone states are defined based on their exposure to four distinct perils — earthquakes, hurricanes, tornadoes, and typhoons — following the S&P Global Market Intelligence platform classification. By requiring both conditions to hold simultaneously, $CatRisk$ captures only the subset of underwriting activity exposed to catastrophic loss potential. As it is constructed directly from the composition of the underwriting portfolio, we use it to test whether treated groups altered that composition after the reform. $CatExposure$ measures the hazard intensity of the states where an insurance group writes. The idea is that a group's need for global diversification depends on the realized catastrophe risk of the states in which it holds premiums, rather than on how its business lines are classified. We construct this measure based on SHELDUS data, which provides catastrophe-related losses on state-level.[23] We proceed in three steps. First, we compute state-level catastrophe intensity as mean annual inflation-adjusted property damage over 1996–2017. We

[22]Following the S&P Global Market Intelligence platform, catastrophe-prone business lines include: allied lines, commercial auto physical damage, commercial multiple peril (non-liability), earthquake, farmowners multiple peril, federal flood, fire, homeowners multiple peril, inland marine, multiple peril crop, private passenger auto physical damage, private crop, and private flood.

[23]SHELDUS (Spatial Hazard Events and Losses Database for the U.S.) covers 18 types of natural hazards for every U.S. state from 1960 to 2024. For each event, it records property and crop damage, injuries, and deaths. SHELDUS is run by the Arizona State University. See https://cemhs.asu.edu/sheldus.

only consider losses from catastrophe-related hazards, namely, hail, hurricanes, tornadoes, severe storms, wind, winter weather, wildfire, flooding, tsunami, and earthquake. Second, for each state we compute each insurance group's share of direct premiums written from NAIC Schedule T, average these shares over the pre-treatment period, and renormalize them to sum to one across states. Third, we take the premium-weighted average of state intensity and standardize it across all insurance groups in our sample.

Risk Retention and Regulatory Capital. To measure how much of the business it writes a group keeps on its own balance sheet, we use net premiums written scaled by GPW. To capture the stock of retained claim obligations, we use unpaid losses and loss adjustment expenses from Schedule P, Part 1, scaled by lagged total assets. We distinguish gross unpaid losses (the exposure written), ceded unpaid losses (the exposure transferred to reinsurers), and net unpaid losses (the exposure retained), each for the current accident year and for all accident years. We measure required capital using the Authorized Control Level (ACL) under the NAIC risk-based capital framework, scaled by lagged total assets. To trace how a change in reinsurance enters required capital, we decompose the ACL into the risk charges that respond to reinsurance: a credit charge on reinsurance recoverables, a reserve-risk charge, and a premium-risk charge. Comparing retained exposure with required capital allows us to test whether economic and reported solvency decouple. Section 7 describes the construction of these measures in detail.

Controls. We include a standard set of time-varying control variables consistent with the literature (see e.g., Adiel, 1996; Altuntas et al., 2018; Cole & McCullough, 2006; Cummins & Drechsler, 2008; Garven & Lamm-Tennant, 2003; Griffith & Liebenberg, 2021). These include $Size$ (log assets), $Leverage$, $Return\ on\ Equity$, $LOB\ Concentration$ (Herfindahl–Hirschman Index over business lines), and $CatRisk$. Detailed definitions for all variables are provided in Table A3.

4. Research Design and Identification

4.1 Statutory Assignment

We assign treatment from the statutory thresholds described in Section 2.3. Based on 2017 filings, we assign an insurance group to the treatment group ($Treated^{\text{stat}}$) if its average annual gross receipts over the three preceding years reach \$500 million and its base erosion percentage reaches 3%. The

treatment indicator therefore measures ex ante statutory exposure to the tax rather than realized legal liability in any post-reform year. Classifying groups ex ante addresses the concern that firms might have intentionally shrunk their gross receipts or shifted their reinsurance usage to fall just below the thresholds in 2018. At the same time, our ex ante assignment does not rule out that treated insurance groups may subsequently have fallen below at least one threshold. Belonging to the treatment group means that the insurance group was exposed to the possibility that the minimum tax binds rather than guaranteeing that it did. Our estimates are accordingly intent-to-treat effects of pre-reform statutory exposure. This is the appropriate estimand for our research question because realized liability is itself an outcome of the reinsurance decisions we study.

Our analysis answers two questions, and each requires its own definition of treatment. The first question is how the BEAT affected reinsurance allocation overall. The second question is how much of the displaced reinsurance was redirected to affiliates that elected to be taxed as U.S. corporations under §953(d). Answering the latter requires observing, for each group, both the cessions to non-electing affiliates and the cessions to electing affiliates. As we hand-collect the election status only for affiliates located in Bermuda, the Cayman Islands, and Switzerland, we can answer the second question only for a subset of the groups considered in the first.

Our primary indicator, $Treated^{\text{stat}}$, addresses the first question. It equals one for insurance groups that met both statutory BEAT thresholds in 2017: average annual gross receipts of at least \$500 million and a base erosion percentage of at least 3%. Every group that meets these criteria makes base erosion payments to foreign affiliates and is therefore exposed to the tax. The indicator relies only on the two legal criteria and does not condition on any other firm characteristic. We use it to estimate the overall effect of the BEAT on reinsurance allocation. Our second indicator, $Treated^{\text{aff}}$, addresses the second question. It equals one for statutorily treated groups that ceded premiums to at least one non-electing affiliate in Bermuda, the Cayman Islands, or Switzerland before the reform. We use it only for the decomposition in Section 6. The restriction follows from how we estimate the substitution ratio. We estimate it as the ratio of the BEAT's effect on cessions to electing affiliates to its effect on cessions to non-electing affiliates in these jurisdictions. Statutorily treated groups without non-electing affiliates in these jurisdictions paid no BEAT on their reinsurance flows to Bermuda, the Cayman Islands, or Switzerland and therefore had no incentive to restructure these flows.

As $Treated^{\text{aff}}$ identifies a narrower set of treated groups, a natural concern is that it may select firms whose overall response to the BEAT differs from that of the broader statutorily treated population. We address this concern by re-estimating the baseline specification in Table 4 using each treatment definition. The estimated contraction in foreign-affiliate reinsurance is -0.074 under $Treated^{\text{stat}}$ and -0.081 under $Treated^{\text{aff}}$. The similarity of these estimates indicates that the narrower group exhibits essentially the same aggregate response. The decomposition in Section 6 therefore does not appear to be driven by selecting a subset of treated firms that responded differently along this margin.

In addition, we split $Treated^{\text{stat}}$ groups at the median of their 2017 base erosion percentage into $LowBEP$ and $HighBEP$ groups, which we use throughout the analysis to capture variation in treatment intensity. Table 2 summarizes the three definitions.

Table 2: Treatment Definitions

This table shows the definitions of the treatment indicators used in the paper. All indicators are evaluated based on 2017 filings and at the consolidated group level. The column *Groups* reports treated and control counts for $Treated^{\text{stat}}$ and $Treated^{\text{aff}}$, and the split of treated groups for $HighBEP$ / $LowBEP$. Both $HighBEP$ and $LowBEP$ groups meet the statutory thresholds and are therefore subject to the BEAT; the split captures intensity of exposure within the treatment group.

Indicator	Definition (based on 2017 values)	Groups	Used in
$Treated^{\text{stat}}$	Average annual gross receipts over the three preceding years $\geq$ \$500 million *and* base erosion percentage $\geq 3\%$	26 / 22	Sections 5–7
$Treated^{\text{aff}}$	$Treated^{\text{stat}}$ *and* held at least one non-electing foreign affiliate in Bermuda, the Cayman Islands, or Switzerland before the reform	17 / 31	Section 6
$HighBEP$ / $LowBEP$	$Treated^{\text{stat}}$ groups above (below) the median 2017 base erosion percentage; mean base erosion percentage 50.0% and 10.3%, respectively	13 / 13	Sections 5–7

4.2 Empirical Specification

To estimate the impact of the BEAT on reinsurance demand, we employ a difference-in-differences (DiD) specification, whereby we treat the BEAT as an exogenous shock to internal capital markets. We estimate the following baseline specification:

$$Y_{i,t} = \alpha_i + \lambda_t + \beta \left(Treated_i^{\text{stat}} \times Post_t \right) + \gamma \mathbf{X}_{i,t} + \varepsilon_{i,t}, \tag{4}$$

where $Y_{i,t}$ represents the ratio of reinsurance premiums ceded to gross premiums written. We measure the reinsurance ratio for four different counterparties. These include foreign affiliates, foreign non-affiliates, domestic affiliates, and domestic non-affiliates. $Post_t$ is an indicator equal to one for years 2018–2022. The vector $\mathbf{X}_{i,t}$ includes time-varying control variables: $Size$, $Leverage$, $Return\ on\ Equity$, $LOB\ Concentration$, and $CatRisk$. α_i and λ_t represent group and year fixed effects, respectively, which absorb time-invariant heterogeneity and aggregate shocks. We cluster standard errors at the insurance group level. The coefficient of interest, β, measures the intent-to-treat effect of pre-reform statutory exposure. We report the specification without time-varying covariates as the baseline and the specification with covariates as a robustness check. The reason is that several covariates, notably return on equity, may themselves respond to the BEAT. Conditioning on them risks a bad-control problem.

The validity of our difference-in-differences design rests on the parallel trends assumption: that in the absence of the tax reform, BEAT-exposed and non-exposed groups would have exhibited similar capital allocation patterns. In practice, the parallel-trends assumption is evaluated by including leads of the treatment indicator in the regression and testing whether their coefficients are statistically different from zero (Cunningham, 2021). Following prior work (e.g., Cai, 2016; Carrizosa et al., 2023; Gavira-Durón et al., 2022; Samuel, 2023), we assess this assumption using an event-study specification that replaces the $Post$ indicator in the baseline model with year-specific interactions from 2013 to 2022:

$$Y_{i,t} = \alpha_i + \lambda_t + \left(\sum_{t=2013}^{2016} + \sum_{t=2018}^{2022} \right) \beta_t \left(Treated_i^{\text{stat}} \times Year_t \right) + \gamma \mathbf{X}_{i,t} + \varepsilon_{i,t}, \tag{5}$$

where $Year_t$ is an indicator variable equal to one if the observation falls in year t and zero otherwise. Following Cunningham (2021), we recenter the year of policy adoption at zero; in our setting, this corresponds to 2017, which serves as the reference period. Since treatment begins uniformly in 2018, the two-way fixed effects estimator is not subject to staggered adoption bias (see e.g., Baker et al., 2022; Borusyak et al., 2024; Sun & Abraham, 2021). The coefficients of interest, β_t, measure the annual difference in reinsurance demand between treated and control groups relative to 2017. Under the parallel trends assumption, all pre-BEAT coefficients (2013–2016) should be statistically indistinguishable from zero. We formally test this using a joint Wald test of the null that

all pre-BEAT interaction terms equal zero. We also conduct a placebo test using only the pre-BEAT period (2013–2016) with a placebo treatment year of 2016. This additional check examines whether our specification would spuriously detect an effect in a period without any policy change.

Treated groups are larger and more offshore-integrated than control groups, so they may follow a differential trend that the pre-reform test cannot rule out. As a robustness check that does not rely on the control group's trajectory, we therefore also estimate a within-group specification on a group $\times$ counterparty-type $\times$ year panel:

$$Y_{i,c,t} = \alpha_{i,c} + \lambda_{c,t} + \delta_{i,t} + \beta \left(Exposed_{c} \times Treated_{i}^{\text{aff}} \times Post_{t} \right) + \varepsilon_{i,c,t}, \tag{6}$$

where $Y_{i,c,t}$ represents the amount of premiums ceded to counterparty c. Again we scale reinsurance premiums with gross premiums written. The five counterparty types are foreign affiliates without a §953(d) election, foreign affiliates with an election, foreign affiliates where we could not identify an election, non-affiliates in general, and domestic affiliates. With 48 groups, five counterparty types and ten years, the panel contains 2,400 observations. $Treated_{i}^{\text{aff}}$ is an indicator variable equal to one if the insurance group is subject to the BEAT (gross receipts $\geq$ \$500 million and base erosion percentage $\geq 3\%$ in 2017), and held at least one non-electing foreign affiliate in Bermuda, the Cayman Islands, or Switzerland before the reform. $Post_t$ is an indicator equal to one for years 2018–2022. $\alpha_{i,c}$ are group-by-type fixed effects, $\lambda_{c,t}$ are type-by-year fixed effects, and $\delta_{i,t}$ are group-by-year fixed effects. The latter absorb everything that varies at the group-year level such as the insurer's size, profitability, catastrophe experience, macroeconomic exposure, or whether the group is U.S.- or foreign-headquartered. The coefficient of interest, β, is identified purely from how a treated group reallocated cessions across its own counterparties within a given year.

5. The Aggregate Response of Internal Capital Markets

Before quantifying the aggregate response, we examine the pre-reform behavior of treated and control groups. Figure 2 plots the coefficients of the between-group design with 95% confidence intervals from the dynamic difference-in-differences specification in Equation (5). We use 2017 as the base year. We provide the full regression results for the coefficients we use in Figure 2 in Table A4 of the Appendix. For the foreign affiliated market, the pre-reform coefficients are

individually insignificant and show a noticeable shift following the implementation of the BEAT in 2018. In each market, a joint Wald test does not reject the null that all pre-2017 interactions are zero.[24] We read the results as consistent with parallel trends rather than as direct evidence for them. Failing to reject the joint null is necessary but not sufficient. The foreign-affiliate point estimates converge monotonically toward zero between 2013 and 2016. This pattern is what one would expect if treated and control groups followed a differential linear trend, which is plausible here because treated groups are larger and more offshore-integrated than controls. We address this concern directly rather than rest on the joint test.

Our primary defense against a differential pre-trend is a within-group design that does not depend on the control groups' trajectory at all. We move to a group $\times$ counterparty-type $\times$ year panel with group-by-year, type-by-year, and group-by-type fixed effects. As the group-by-year effects absorb every group-level shock and trend, the treatment effect is identified from the reallocation of cessions *across counterparty types within the same group and year*. Each group serves as its own control, and any differential trend operating at the group level is canceled out. Columns (3) and (4) of Table 3 report this design. The pre-reform coefficients are close to zero and individually insignificant, so the pattern that motivated the concern in the between-group event study does not survive the within-group transformation. The estimated break in 2018 is of comparable magnitude to the between-group design. In the within-group design, the foreign-affiliate cession ratio drops by approximately 9.8 percentage points in 2018 ($t = -2.45$), compared with 11.2 percentage points ($t = -2.81$) in the between-group design, and remains stable through 2022.[25]

As a further check, we confirm that the estimated post-reform contraction survives an explicit linear-trend counterfactual that removes the differential pre-reform trend between treated and control groups. Specifically, we estimate the differential linear trend between treated and control groups on pre-reform data alone (2013–2016). We then remove this trend from the treated-group outcome, and re-estimate the event study. As the trend is itself estimated, we base inference on a two-step cluster bootstrap-t procedure that re-runs both steps in each replication. Table A5 in the Appendix reports the results. For the foreign affiliated market, the adjusted pre-reform coefficients

[24]The p-values for the joint Wald test are as follows: foreign affiliates ($p = 0.57$); domestic affiliates ($p = 0.43$); non-affiliates ($p = 0.71$).

[25]A Wald test does not reject the equality of $Treated^{\text{stat}} \times 2018$ and $Treated^{\text{stat}} \times 2019$ ($\chi^2_1 = 0.14$, $p = 0.71$) in the between-group design using control variables.

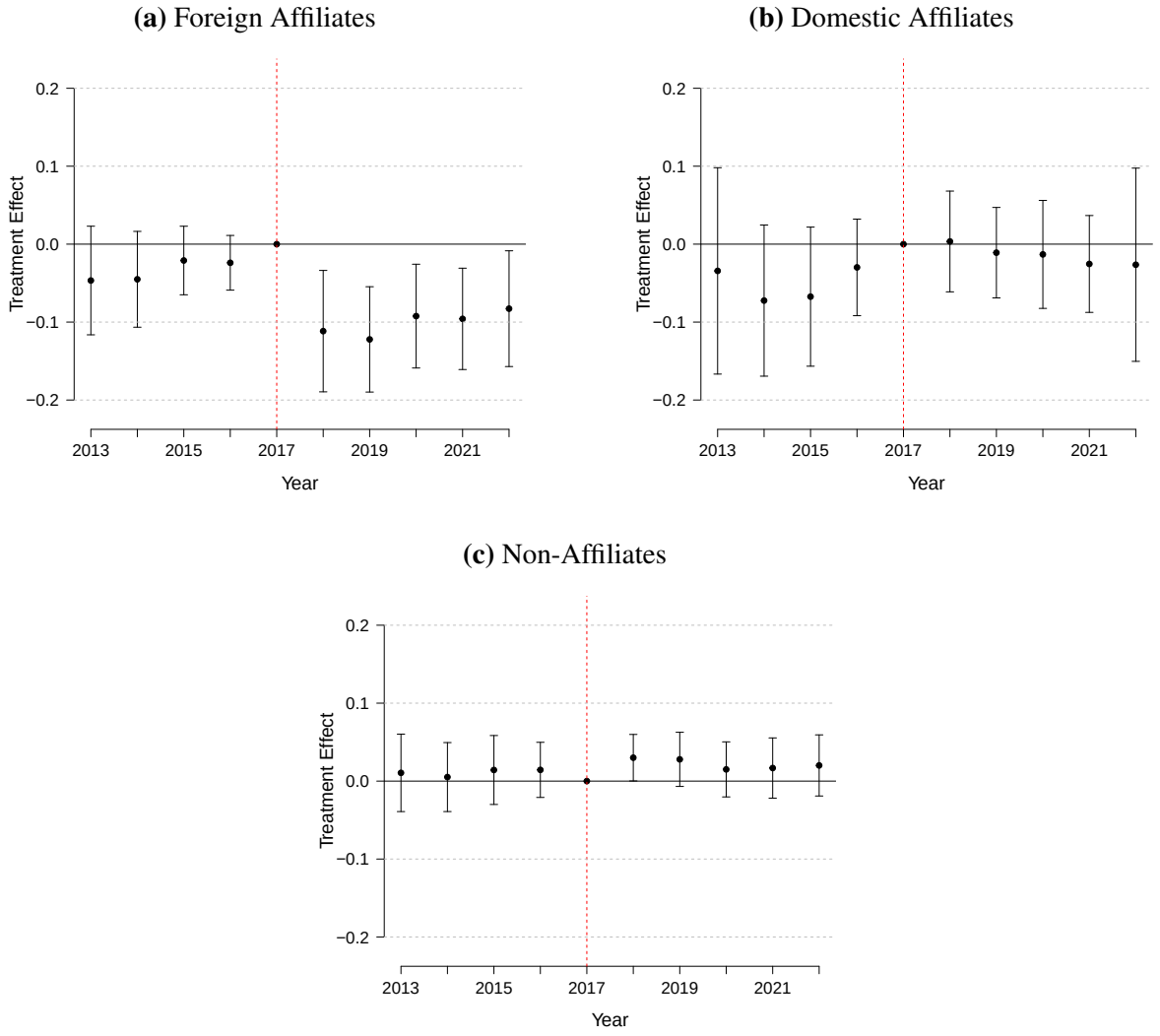
Table 3: Parallel Trends: Between and Within Groups

This table reports year-specific treatment effects of the Base Erosion and Anti-Abuse Tax (BEAT) on reinsurance ceded to foreign affiliates. In Columns (1) and (2) the dependent variable is the ratio of premiums ceded to foreign affiliates to gross premiums written (GPW), estimated on a group $\times$ year panel with group and year fixed effects. In Columns (3) and (4) the dependent variable is premiums ceded to a given counterparty type, scaled by GPW, in a group $\times$ counterparty-type $\times$ year panel. The fixed effects in these columns absorb all variation across groups within a year, so the effect is identified from the reallocation of cessions across counterparty types within the same group and year rather than from comparisons between treated and control groups. Row labels denote the treatment indicator interacted with year indicators; 2017 is the omitted reference year, so each coefficient measures the difference relative to the year before the reform. $Treated^{\text{stat}}$ equals one if the insurance group met both statutory BEAT thresholds in 2017: average annual gross receipts of at least \$500 million and a base erosion percentage of at least 3%. Column (4) uses $Treated^{\text{aff}}$, which additionally requires a pre-reform non-electing foreign affiliate in Bermuda, the Cayman Islands, or Switzerland. Control variables in Column (2) are $Size$, $Leverage$, $LOB\ Concentration$, $CatRisk$, and $Return\ on\ Equity$. Variable definitions are provided in Table A3.

	Between group		Within group	
	(1)	(2)	(3)	(4)
Treated $\times$ 2013	-0.059^{*}	-0.047	-0.008	-0.036
	(0.032)	(0.036)	(0.030)	(0.038)
Treated $\times$ 2014	-0.055^{*}	-0.045	-0.000	-0.035
	(0.030)	(0.031)	(0.028)	(0.037)
Treated $\times$ 2015	-0.028	-0.021	0.005	-0.032
	(0.021)	(0.022)	(0.018)	(0.020)
Treated $\times$ 2016	-0.029	-0.024	0.002	-0.024
	(0.018)	(0.018)	(0.015)	(0.017)
Treated $\times$ 2018	-0.111^{***}	-0.112^{***}	-0.098^{**}	-0.169^{***}
	(0.040)	(0.040)	(0.040)	(0.045)
Treated $\times$ 2019	-0.119^{***}	-0.122^{***}	-0.101^{**}	-0.188^{***}
	(0.037)	(0.034)	(0.042)	(0.047)
Treated $\times$ 2020	-0.089^{**}	-0.092^{***}	-0.101^{**}	-0.185^{***}
	(0.036)	(0.034)	(0.042)	(0.046)
Treated $\times$ 2021	-0.097^{***}	-0.096^{***}	-0.105^{**}	-0.183^{***}
	(0.036)	(0.033)	(0.042)	(0.048)
Treated $\times$ 2022	-0.079^{*}	-0.083^{**}	-0.108^{**}	-0.166^{***}
	(0.041)	(0.038)	(0.043)	(0.049)
Observations	480	480	2,400	2,400
Adjusted R^2	0.727	0.738	0.904	0.906
Treatment variable used	$\text{Treated}^{\text{stat}}$	$\text{Treated}^{\text{stat}}$	$\text{Treated}^{\text{stat}}$	$\text{Treated}^{\text{aff}}$
Control variables	no	yes	—	—
Group fixed effects	yes	yes	—	—
Time fixed effects	yes	yes	—	—
Group $\times$ type fixed effects	—	—	yes	yes
Type $\times$ year fixed effects	—	—	yes	yes
Group $\times$ year fixed effects	—	—	yes	yes

Notes: All continuous variables are winsorized at the 5th and 95th percentiles, with cutoffs computed separately for treated and control groups. For the within-group design, we determine cutoffs separately for each treatment-status $\times$ counterparty-type cell. Standard errors are in parentheses and clustered at the insurance group level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Figure 2: Identification and the Parallel Trends Assumption



This figure shows point estimates and 95% confidence intervals from the dynamic difference-in-differences specification following Equation (5). The regression results can be found in Table A4 of the Appendix. We omit the year 2017 as reference year. $Treated^{\text{stat}}$ is an indicator variable equal to one if the insurance group met the statutory BEAT thresholds in 2017. These include average annual gross receipts of at least $500 million and a base erosion percentage of at least 3%. The dependent variables are the ratios of reinsurance premiums ceded to three counterparty types, scaled by gross premiums written (GPW): foreign affiliates in Panel A, domestic affiliates in Panel B, and non-affiliated reinsurers in Panel C. Standard errors are clustered at the insurance group level.

are flat and close to zero. In contrast, the post-reform coefficients remain large and statistically significant, with the exception of the final year, which is of the same magnitude but only marginally insignificant. Comparing the magnitude of the de-trended coefficients with the original coefficients in the post-reform period, they became larger. This is because the fitted pre-trend runs opposite to the treatment effect, so our baseline estimates are if anything conservative. For the other two

markets the estimated pre-trends are small and statistically indistinguishable from zero, with a single marginal exception, and the post-reform coefficients are insignificant. We do not view the detrended version as a validation of parallel trends; rather, it shows that the estimated decline is robust to removing a differential linear trend fitted on the pre-reform period. As a final falsification, we conduct a placebo test restricted to the pre-BEAT period (2013–2016) that assigns a fictitious treatment in 2016. Table A6 in the Appendix reports the results. The absence of significant placebo effects is consistent with the parallel-trends assumption.

Table 4 presents the results for the impact of the tax shock on internal reinsurance streams to foreign affiliates. Columns (1)–(4) estimate the average treatment effect. Column (1) presents a parsimonious specification with only treatment and post-period indicators, while Column (2) adds group and year fixed effects. Column (3) includes the full set of insurer-level controls and fixed effects. Column (4) replaces the statutory indicator with $Treated^{\text{aff}}$, which restricts the treated group to insurers holding a pre-reform non-electing foreign affiliate. The interaction is negative and statistically significant throughout. In Column (2), treated groups reduce their foreign-affiliate cession ratio by 6.5 percentage points relative to controls. There could be concerns that relying on time-varying controls for identification biases results. These could themselves respond to the BEAT, so conditioning on them risks absorbing part of the treatment effect. However, we note that in this table and the following ones the estimate is of similar magnitude with and without these controls. This indicates that the contraction and the following results are not driven by contemporaneous shifts in firm size or profitability. Column (5) examines heterogeneity by splitting BEAT-exposed insurers into $LowBEP$ and $HighBEP$ based on their 2017 base erosion percentage. The $LowBEP$ group shows no statistically significant response, consistent with the notion that firms with minimal exposure faced negligible costs and thus had little incentive to restructure. The $HighBEP$ group shows large, significant reductions of 13.7 percentage points. One could argue that the coefficients in Column (5) are mechanical, since the two groups cede different levels of reinsurance to foreign affiliates before treatment. To rule this out, we also use simple growth rates of the dependent variable. Here too, the response to the BEAT is concentrated in groups with high exposure to foreign affiliated reinsurance. They reduced their reinsurance cessions by 31.6% relative to the control group in 2018.[26]

[26]To keep Table 4 readable, we show only the 2018 interaction terms.

Table 4: Tax Sensitivity of Foreign Affiliate Reinsurance

This table reports regression results based on Equation (4), estimating the effect of the Base Erosion and Anti-Abuse Tax (BEAT) on internal capital transfers to foreign affiliates. The dependent variable is the ratio of reinsurance premiums ceded to foreign affiliates to gross premiums written (GPW). $Treated^{\text{stat}}$ is an indicator variable equal to one if the insurance group met the statutory BEAT thresholds in 2017. These include average annual gross receipts of at least \$500 million and a base erosion percentage of at least 3%. $Post$ is an indicator variable for the years 2018–2022. Column (1) reports the DiD coefficient without fixed effects or covariates. Column (2) adds group and year fixed effects and Column (3) adds insurer-level controls. Column (4) uses the treatment definition $Treated^{\text{aff}}$, which requires non-electing foreign affiliates. Column (5) interacts treatment with median-split groups of the 2017 base erosion percentage to capture treatment intensity. Column (6) uses the simple growth rate of reinsurance to foreign affiliates as the dependent variable and runs it on every interaction term between median-split group and year. Control variables are $Size$, $Leverage$, $LOB\ Concentration$, $CatRisk$, and $Return\ on\ Equity$. Variable definitions are provided in Table A3.

	Reinsurance Foreign Affiliates					
	level	*level*	*level*	*level*	*level*	*simple growth*
	(1)	(2)	(3)	(4)	(5)	(6)
Treated^{stat} × Post	−0.065**	−0.065*	−0.074**			
	(0.033)	(0.033)	(0.031)			
Treated^{aff} × Post				−0.081**		
				(0.035)		
LowBEP × Post					−0.010	
					(0.027)	
HighBEP × Post					−0.137***	
					(0.038)	
LowBEP × 2018						−0.171
						(0.178)
HighBEP × 2018						−0.316*
						(0.164)
Treated^{stat}	0.115***					
	(0.044)					
Post	−0.049**					
	(0.023)					
Constant	0.115***					
	(0.031)					
Control variables	no	no	yes	yes	yes	yes
Group fixed effects	no	yes	yes	yes	yes	yes
Time fixed effects	no	yes	yes	yes	yes	yes
Observations	480	480	480	480	480	432
Adjusted R^2	0.145	0.728	0.740	0.741	0.764	0.018

Notes: All continuous variables are winsorized at the 5th and 95th percentiles, with cutoffs computed separately for treated and control groups. Standard errors are in parentheses and clustered at the insurance group level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

A natural concern is that treated and control groups represent different populations, since the BEAT targets large, internationally integrated insurers. Size matters for our setting in particular. A larger insurance group has a greater capacity to diversify internally and derives more value from its internal capital market. Treated groups might therefore respond differently to a tax on internal reinsurance simply because they are larger, not because of the tax itself. We address this concern with a set of robustness checks built on the propensity score, which allow us to compare treated groups with control groups of similar size. As treatment is time-invariant and assigned ex ante, we estimate the score on the 2017 cross-section by Firth-penalized maximum likelihood, using pre-period averages of $Size$, $LOB\ Concentration$, $CatRisk$, and $Leverage$. In this specification only $Size$ is a significant determinant of treatment, and the covariates do not jointly separate the two groups. Adding pre-reform foreign-affiliate cessions makes both that variable and $Size$ statistically significant, but this is mechanical rather than informative: the base erosion percentage is defined as foreign-affiliate cessions over total deductible payments, so the added covariate is a rescaling of the assignment rule itself. Eight groups fall outside the region of common support under the score excluding pre-reform cessions, and twenty under the score including them. Table A7 in the Appendix reports the score estimates, and Figure 3 shows the overlap under both specifications. We then re-estimate the baseline across a series of restricted samples and alternative designs. Restricting the sample to the region of common support ensures that the estimated effect is not driven by comparing the largest, most diversified treated groups with small control groups that lack comparable diversification capacity.

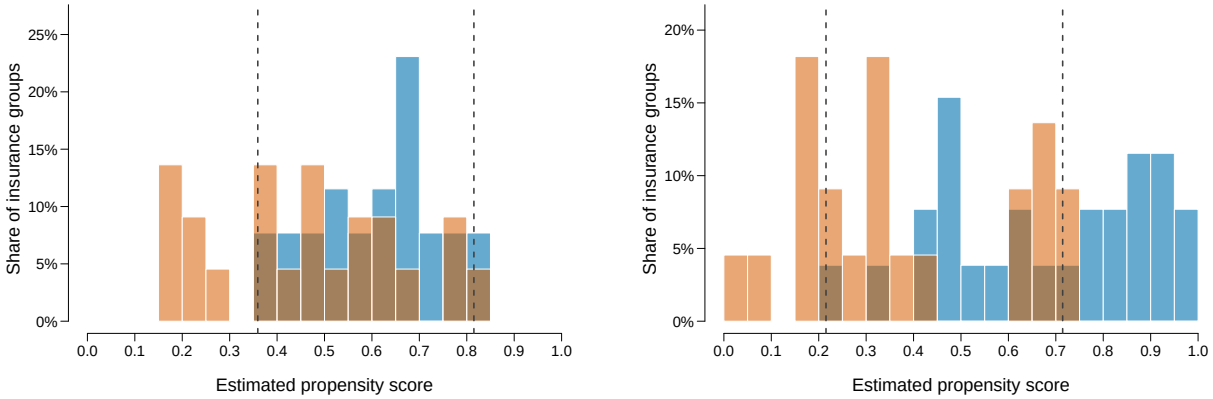
Table 5 reports these estimates, with the three treatment indicators shown in separate panels. Column (1) reproduces the baseline from Table 4 for comparison. Column (2) adds back the insurers we excluded from the final sample for holding no foreign or domestic affiliates. This extends the sample to the full U.S. property-casualty industry. Columns (3)–(5) use the propensity-score samples. Column (3) trims to common support under the score excluding cessions, Column (4) under the score including cessions, and Column (5) uses the latter score but retains all treated groups, dropping only the controls that lie outside the bounds. Column (6) estimates the within-group design.

The estimate is stable across every design except Column (4). Restricting to insurers of comparable propensity in Column (3) leaves the magnitude essentially unchanged. In this specification

Figure 3: Common Support Figure

(a) Score excluding pre-reform cessions

(b) Score including pre-reform cessions



This figure plots the distribution of estimated propensity scores for treated and control insurance groups. Scores are taken from the Firth-penalized logit models of Table A7 in the Appendix: Panel A uses Column (1), which conditions on *Size*, *LOB Concentration*, *CatRisk*, and *Leverage*; Panel B uses Column (2), which additionally conditions on pre-reform foreign affiliate cessions. Blue bars are treated groups and orange bars are control groups; overlapping regions appear in the darker shade where the two distributions coincide. Bars show the share of groups within each treatment status, so each distribution sums to one and the two are comparable despite unequal group sizes. Bin width is 0.05. $Treated^{\text{stat}}$ is an indicator equal to one if the insurance group met both statutory BEAT thresholds in 2017: average annual gross receipts of at least $500 million and a base erosion percentage of at least 3%. The dashed vertical lines mark the bounds of the region of common support, defined as the interval in which both treated and control groups have observations; groups with scores outside these bounds are excluded from the restricted samples in Columns (3) and (4) of Table 5. The bounds are [0.359, 0.815] in Panel A, which drops 8 of 48 groups, and [0.215, 0.714] in Panel B, which drops 20. The wider separation in Panel B is mechanical: pre-reform cessions are a rescaling of the base erosion percentage that assigns treatment, so conditioning on them pushes the two distributions apart and trims the most heavily exposed treated groups.

we compare similarly sized groups, so our baseline contraction in foreign affiliated reinsurance is not driven by comparing insurers of different size. Column (4) yields an insignificant estimate, but for a reason that strengthens the result rather than weakens it: the score including cessions trims the most heavily exposed treated groups (Figure 3, Panel B), which are the $HighBEP$ groups that drive the response. Restoring them in Column (5), while still dropping the incomparable controls, restores the effect. Column (6) removes group-year variation entirely and, if anything, produces a larger estimate. The coefficient of -0.102 ($t = -2.64$) says that, within the same group and year, treated insurers reduced cessions to non-electing foreign affiliates by 10.2 percentage points relative to their other counterparties, such as external reinsurers. Because this design uses each group as its own control, it also absorbs any group-level shock to overall reinsurance demand, which is the channel left open by the between-group pre-trends.

If reinsurance demand is driven more by risk diversification than by tax arbitrage, the source

Table 5: The Contraction in BEAT-Exposed Cessions across Samples and Designs

This table assesses whether the estimated contraction in foreign affiliate reinsurance is driven by systematic differences between treated and control groups. In Columns (1)–(5) the dependent variable is the ratio of reinsurance premiums ceded to foreign affiliates to gross premiums written (GPW); in Column (6) it is premiums ceded to a given counterparty type, scaled by GPW, in a group × counterparty-type × year panel. Row labels denote the treatment indicator, defined in Table 2; the interaction row reports the coefficient estimated in each column. In Column (6), *Exposed* equals one for cessions to foreign affiliates without a §953(d) election and zero for the other counterparty types. Panel A uses the statutory indicator $Treated^{\text{stat}}$; Panel B splits treated groups at the median 2017 base erosion percentage to capture intensity of exposure; Panel C uses $Treated^{\text{aff}}$, which additionally requires a pre-reform non-electing foreign affiliate in Bermuda, the Cayman Islands, or Switzerland. $Post$ is an indicator variable for the years 2018–2022. Column (1) reproduces the baseline estimate from Table 4 for comparison. Column (2) expands the sample to the entire U.S. property-casualty industry, irrespective of whether a group has domestic or foreign affiliates. Columns (3) and (4) restrict to the region of common support under propensity scores estimated excluding and including pre-reform foreign affiliate cessions, respectively. Column (5) restricts to groups which are either treated or lie within the common support bounds. For Columns (2)–(5) we additionally report cluster bootstrap-t p-values in brackets and base significance stars on them; the procedure uses 1,000 replications, in each of which we resample insurance groups with replacement, re-estimate the model, and compute the studentized statistic for the coefficient of interest, comparing the original t-statistic with the bootstrap distribution of these statistics. Control variables in Columns (1)–(5) are $Size$, $Leverage$, $LOB\ Concentration$, $CatRisk$, and $Return\ on\ Equity$. Variable definitions are provided in Table A3.

	(1) Full sample	(2) Population	(3) Common support *excl. cessions*	(4) Common support *incl. cessions*	(5) Common support *incl. cessions and treated*	(6) Within group
Interaction	*Treated* × *Post*	*Treated* × *Post*	*Treated* × *Post*	*Treated* × *Post*	*Treated* × *Post*	*Exposed* × *Treated* × *Post*
Panel A: statutory treatment						
$Treated^{\text{stat}}$	−0.074**	−0.117***	−0.069*	−0.022	−0.080**	−0.102**
	(0.031)	(0.021)	(0.033)	(0.033)	(0.034)	(0.039)
			[0.064]	[0.569]	[0.034]	
Panel B: by treatment intensity						
LowBEP	−0.010	−0.042***	−0.003	0.000	−0.016	0.002
	(0.027)	(0.016)	(0.029)	(0.030)	(0.031)	(0.029)
			[0.922]	[0.997]	[0.656]	
HighBEP	−0.137***	−0.176***	−0.128**	−0.093	−0.141***	−0.206***
	(0.038)	(0.033)	(0.039)	(0.052)	(0.041)	(0.049)
			[0.012]	[0.284]	[0.004]	
Panel C: affiliate-restricted treatment						
$Treated^{\text{aff}}$	−0.081**	−0.130***	−0.072*	−0.011	−0.081**	−0.153***
	(0.035)	(0.030)	(0.035)	(0.046)	(0.036)	(0.044)
			[0.060]	[0.859]	[0.046]	
Insurance groups	48	268	40	28	41	48
Observations	480	2,680	400	280	410	2,400
Control variables	yes	yes	yes	yes	yes	—
Group fixed effects	yes	yes	yes	yes	yes	—
Time fixed effects	yes	yes	yes	yes	yes	—
Group × type fixed effects	—	—	—	—	—	yes
Type × year fixed effects	—	—	—	—	—	yes
Group × year fixed effects	—	—	—	—	—	yes

Notes: All continuous variables are winsorized at the 5th and 95th percentiles, with cutoffs computed separately for treated and control groups. For the within-group design, we determine cutoffs separately for each treatment-status × counterparty-type cell. Clustered standard errors are in parentheses and bootstrap-t p-values are in brackets. *, **, and *** are based on the bootstrap-t p-values and indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

of capital, internal or external, should be less relevant than the need for risk transfer itself. We would therefore expect the decline in affiliate capacity to be fully offset by an increase in alternative coverage. We test for substitution along two channels: internal domestic reinsurance, measured by cessions to domestic affiliates, and external reinsurance, measured by cessions to non-affiliated reinsurers. Table 6 reports the estimates. Neither channel shows a significant average response. The interaction is indistinguishable from zero for domestic affiliates (Columns 1–2) and for non-affiliated reinsurers in aggregate (Columns 3–4). We do not read these nulls as closing either channel. Both channels are economically non-trivial and imprecisely estimated, so the data remain consistent with some groups shifting risk to either channel even though we cannot detect it on average. Splitting non-affiliated reinsurers into domestic (Columns 5–6) and foreign (Columns 7–9) shows two forces that offset in the aggregate. Treated groups raised cessions to foreign non-affiliated reinsurers by 2.6 percentage points ($t = 2.11$) of GPW. At the same time, they lowered cessions to domestic non-affiliated reinsurers, where the estimate is -0.018 ($t = -1.46$) in Column (6). Since the two coefficients move in opposite directions, the aggregate non-affiliate effect in Column (4) is close to zero.

To further characterize this external shift, we decompose non-affiliated reinsurance demand into an intensive and an extensive margin. The intensive margin is the volume ceded, scaled by GPW; the extensive margin is the number of distinct reinsurance counterparties. Table 7 reports both, with the intensive margin in Panel A and the extensive margin in Panel B. For foreign non-affiliated reinsurers the increase is entirely intensive. This means that cessions rise by 2.8 percentage points of GPW (Panel A, Column 5), while the number of foreign counterparties does not rise (Panel B). As volume grows without a corresponding increase in the count of relationships, the additional risk flowed through reinsurers the groups already used before the BEAT was enacted. The domestic market moves the other way. The intensive margin is flat, whereas the extensive margin shows weak evidence of a reduction (Panel B, Column 7). Treated groups appear to have terminated domestic reinsurance relationships rather than to have reduced the volume ceded to those they retained. One reading of this pattern is a response of reinsurers following the BEAT. A domestic reinsurer that itself cedes to foreign affiliates would increase its base erosion percentage and thus potentially trigger the BEAT or increase its BEAT liability. Dealing with the foreign unaffiliated reinsurer directly avoids that indirect exposure to the BEAT. This would show up in fewer domestic

Table 6: Spillover to other Reinsurance Markets

This table reports regression results based on Equation (4), estimating the effect of the Base Erosion and Anti-Abuse Tax (BEAT) on potential capital reallocation within the U.S. The dependent variable is the ratio of reinsurance premiums ceded to a given counterparty to gross premiums written (GPW): domestic affiliates in Columns (1) and (2), all non-affiliated reinsurers in Columns (3) and (4), and the domestic and foreign components of that total in Columns (5)–(6) and (7)–(9), respectively. $Treated^{\text{stat}}$ equals one if the insurance group met both statutory BEAT thresholds in 2017: average annual gross receipts of at least $500 million and a base erosion percentage of at least 3%. *Post* is an indicator variable for the years 2018–2022. Column (9) splits treated groups at the median 2017 base erosion percentage to test whether any reallocation is concentrated among the most heavily exposed groups. Control variables are *Size*, *Leverage*, *LOB Concentration*, *CatRisk*, and *Return on Equity*. Variable definitions are provided in Table A3.

	Domestic Affiliates		**Total Non-Affiliates**		**Domestic Non-Affiliates**		**Foreign Non-Affiliates**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treated^{stat} × Post	0.015	0.025	0.005	0.013	−0.022*	−0.018	0.023*	0.026**	
	(0.052)	(0.047)	(0.019)	(0.018)	(0.012)	(0.013)	(0.013)	(0.012)	
LowBEP × Post									0.025
									(0.018)
HighBEP × Post									0.026*
									(0.013)
Treated^{stat}	−0.051		−0.021		−0.005		−0.006		
	(0.111)		(0.037)		(0.018)		(0.022)		
Post	0.056		0.040***		0.035***		0.009		
	(0.036)		(0.012)		(0.010)		(0.008)		
Constant	0.480***		0.163***		0.075***		0.076***		
	(0.086)		(0.033)		(0.015)		(0.019)		
Control variables	no	yes	no	yes	no	yes	no	yes	yes
Group fixed effects	no	yes	no	yes	no	yes	no	yes	yes
Time fixed effects	no	yes	no	yes	no	yes	no	yes	yes
Observations	480	480	480	480	480	480	480	480	480
Adjusted R^2	0.003	0.887	0.022	0.879	0.031	0.808	0.021	0.850	0.849

Notes: All continuous variables are winsorized at the 5th and 95th percentiles, with cutoffs computed separately for treated and control groups. Standard errors are in parentheses and clustered at the insurance group level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

relationships alongside intensified foreign ones.

We cannot rule out some external substitution. However, comparing the point estimate of 0.013 in Column (4) from Table 6 to our estimated decline in foreign affiliated reinsurance in Table 4 of 0.074 reveals a substantial substitution gap. Treated firms replaced less than one fourth of the lost internal capacity with external reinsurance. If internal and external reinsurance were available at the same price, the offset would have been one for one. However, it was not. This gap suggests that demand for affiliate reinsurance was highly sensitive to its tax treatment. Once the tax advantage was removed, most of these flows were not worth buying elsewhere. It does not tell us that they covered no real risk. Insurance groups had a third option beyond ceding internally or buying external coverage, which is keeping the risk on their domestic balance sheet. An unaffiliated reinsurer charges a margin an affiliate does not, so when the tax advantage disappears, retention can be the cheaper choice even for risk exposure. A contract can transfer real risk and still be bought mainly because the tax treatment made the transfer worthwhile. Section 7.1 shows this is what happened: net retention rose, and so did unpaid losses on the balance sheet. Risk that carried no claims could not have produced that increase. What the gap establishes on its own, then, is that these flows depended on the tax differential to survive, not what they were for. Separating the motives requires the §953(d) evidence in Section 6.1 and the catastrophe split in Section 6.2.

A potential concern is that BEAT-affected groups responded to the tax by replacing traditional affiliated reinsurance with cessions to insurance-linked securities (ILS) vehicles. We assess this channel and find it unlikely to explain our results. First, the ILS market, though growing, remains small relative to the broader reinsurance market. Total outstanding ILS volume in our sample grew from $1.6 billion in 2016 to $12.8 billion in 2022.[27] By comparison, total premiums ceded in the U.S. reinsurance market reached about $578 billion in 2022. Outstanding ILS volume is a stock and ceded premiums is an annual flow. The two magnitudes are so far apart that ILS can absorb only a small share of overall risk transfer. Second, ILS usage is highly concentrated: only 12 of our insurance groups report any ILS cessions over the sample period.

We test the channel directly in Table 8, where we regress outstanding ILS volume, scaled by gross premiums written, on the treatment interaction over the full sample. The average effect is

[27]The S&P Global Market Intelligence platform began collecting ILS data in 2016, which is why our ILS sample starts then.

Table 7: Non-Affiliated Reinsurance: Intensive and Extensive Margin

Panel A: Intensive Margin

This table asks whether the contraction in foreign affiliate cessions was offset by a larger *volume* ceded to non-affiliated reinsurers. The dependent variable is the ratio of premiums ceded to a given counterparty type to gross premiums written (GPW): all non-affiliated reinsurers in Columns (1)–(3), foreign non-affiliates in Columns (4)–(6), and domestic non-affiliates in Columns (7)–(9). $Treated^{\text{stat}}$ equals one if the insurance group met both statutory BEAT thresholds in 2017: average annual gross receipts of at least $500 million and a base erosion percentage of at least 3%. $Post$ is an indicator for the years 2018–2022. Within each type of counterparty, the first column reports a parsimonious specification with no controls or fixed effects, in which the level terms are separately identified; the second adds group and year fixed effects together with the full set of insurer-level controls; the third splits treated groups at the median 2017 base erosion percentage into $LowBEP$ and $HighBEP$ to test whether any effect is concentrated among the most heavily exposed groups. Control variables are $Size$, $Leverage$, $LOB\ Concentration$, $CatRisk$, and $Return\ on\ Equity$. Variable definitions are provided in Table A3.

	Total Non-Affiliates			**Foreign Non-Affiliates**			**Domestic Non-Affiliates**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treated^{stat} × Post	0.011	0.018		0.026**	0.028**		−0.014	−0.010	
	(0.017)	(0.017)		(0.011)	(0.011)		(0.011)	(0.010)	
LowBEP × Post			0.017			0.025			−0.007
			(0.022)			(0.016)			(0.008)
HighBEP × Post			0.020			0.030**			−0.014
			(0.021)			(0.011)			(0.015)
Treated^{stat}	−0.018			−0.006			−0.005		
	(0.035)			(0.021)			(0.018)		
Post	0.009			−0.010			0.020**		
	(0.010)			(0.007)			(0.008)		
Constant	0.158***			0.075***			0.074***		
	(0.031)			(0.018)			(0.015)		
Control variables	no	yes	yes	no	yes	yes	no	yes	yes
Group fixed effects	no	yes	yes	no	yes	yes	no	yes	yes
Time fixed effects	no	yes	yes	no	yes	yes	no	yes	yes
Observations	480	480	480	480	480	480	480	480	480
Adjusted R^2	0.00001	0.864	0.864	0.006	0.848	0.848	0.010	0.819	0.818

Notes: All continuous variables are winsorized at the 5th and 95th percentiles, with cutoffs computed separately for treated and control groups. Standard errors are in parentheses and clustered at the insurance group level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 7: Non-Affiliated Reinsurance: Intensive and Extensive Margin – Continued

Panel B: Extensive Margin

This table asks whether the contraction in foreign affiliate cessions changed the *number* of non-affiliated reinsurance relationships insurance groups maintain. The dependent variable is $\log(1+N)$, where N is the number of distinct non-affiliated reinsurance counterparties of a given type: all non-affiliated reinsurers in Columns (1)–(3), foreign non-affiliates in Columns (4)–(6), and domestic non-affiliates in Columns (7)–(9). $Treated^{\text{stat}}$ equals one if the insurance group met both statutory BEAT thresholds in 2017: average annual gross receipts of at least $500 million and a base erosion percentage of at least 3%. $Post$ is an indicator for the years 2018–2022. Within each type of counterparty, the first column reports a parsimonious specification with no controls or fixed effects, in which the level terms are separately identified; the second adds group and year fixed effects together with the full set of insurer-level controls; the third splits treated groups at the median 2017 base erosion percentage into $LowBEP$ and $HighBEP$ to test whether any effect is concentrated among the most heavily exposed groups. Control variables are $Size$, $Leverage$, $LOB\ Concentration$, $CatRisk$, and $Return\ on\ Equity$. Variable definitions are provided in Table A3.

	Total Non-Affiliates			**Foreign Non-Affiliates**			**Domestic Non-Affiliates**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\text{Treated}^{\text{stat}} \times \text{Post}$	0.023	−0.013		−0.234	−0.068		−0.230**	−0.133	
	(0.106)	(0.059)		(0.148)	(0.093)		(0.113)	(0.104)	
LowBEP × Post			−0.009			−0.076			−0.095
			(0.070)			(0.098)			(0.116)
HighBEP × Post			−0.018			−0.060			−0.170
			(0.071)			(0.108)			(0.110)
$\text{Treated}^{\text{stat}}$	0.491*			0.844**			0.482*		
	(0.263)			(0.331)			(0.260)		
Post	0.164*			0.424***			0.386***		
	(0.096)			(0.138)			(0.105)		
Constant	4.376***			3.650***			3.033***		
	(0.231)			(0.299)			(0.205)		
Control variables	no	yes	yes	no	yes	yes	no	yes	yes
Group fixed effects	no	yes	yes	no	yes	yes	no	yes	yes
Time fixed effects	no	yes	yes	no	yes	yes	no	yes	yes
Observations	471	471	471	480	480	480	480	480	480
Adjusted R^2	0.084	0.964	0.963	0.120	0.940	0.940	0.063	0.926	0.926

Notes: All continuous variables are winsorized at the 5th and 95th percentiles, with cutoffs computed separately for treated and control groups. Standard errors are in parentheses and clustered at the insurance group level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

economically small and at most marginally significant: 0.013 without controls and 0.015 with controls (Columns 1–2), so treated groups increased ILS exposure by at most about 1.5 percentage points of GPW after 2017. More tellingly, splitting treated groups by exposure (Column 3) shows the point estimate is driven by $HighBEP$ groups (0.026) but is not statistically significant. This is the opposite of what defines our main results, where the response is sharp and significant precisely among $HighBEP$ groups. If ILS were absorbing the reinsurance that the BEAT drove out of foreign affiliates, the increase should concentrate in the most exposed groups. However, it does not. We therefore find no evidence that ILS substitution accounts for the contraction we document.

Table 8: Robustness: Substitution toward Insurance-Linked Securities

This table tests whether BEAT-affected groups shifted risk toward insurance-linked securities (ILS) vehicles. The dependent variable is outstanding ILS volume scaled by gross premiums written (GPW), estimated on the full sample. Column (1) reports a specification without controls, Column (2) adds the full set of firm-level controls, and Column (3) splits treated groups at the median 2017 base erosion percentage into $LowBEP$ and $HighBEP$. $Treated^{\text{stat}}$ equals one if the insurance group met both statutory BEAT thresholds in 2017: average annual gross receipts of at least $500 million and a base erosion percentage of at least 3%. $Post$ indicates the years 2018–2022. All specifications include group and year fixed effects. Control variables are $Size$, $Leverage$, $LOB\ Concentration$, $CatRisk$, and $Return\ on\ Equity$. Variable definitions are provided in Table A3.

	ILS/GPW		
	(1)	(2)	(3)
Treated^{stat} × Post	0.013	0.015*	
	(0.008)	(0.009)	
LowBEP × Post			0.004
			(0.003)
HighBEP × Post			0.026
			(0.016)
Control variables	no	yes	yes
Group fixed effects	yes	yes	yes
Time fixed effects	yes	yes	yes
Observations	480	480	480
Adjusted R^2	0.460	0.463	0.476

Notes: All continuous variables are winsorized at the 5th and 95th percentiles, with cutoffs computed separately for treated and control groups. Standard errors are in parentheses and clustered at the insurance group level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

6. Unbundling Risk Management and Tax Optimization

There are multiple motives for an insurer to purchase reinsurance. These include risk sharing, regulatory arbitrage, and tax optimization. Risk sharing means that an insurance group can send risk to an affiliate whose book is weakly correlated with the risk being ceded. Pooling weakly correlated risks creates diversification benefits. These benefits lower the capital the insurer must hold and thus raise capital efficiency within the group. Even without cross-border risk sharing needs, an insurance group could send its U.S. premiums offshore. The reason is that the same risk can attract a lower capital charge under the foreign affiliate's regulatory regime than under the U.S. NAIC framework (Koijen & Yogo, 2016).[28] In this case, the insurance group incurs less capital costs since it must hold less capital for the same amount of risk. Finally, reinsurance can be used for profit shifting. Before the BEAT, reinsurance cessions reduced taxable income in the U.S. and raised taxable income in jurisdictions with a lower statutory tax rate.

To separate the tax motive from the others, we exploit a unique feature of the U.S. tax code: the §953(d) election. The election removes the tax advantage while leaving the affiliate's domicile, capital structure, and regulatory regime untouched. It therefore divides the three motives into two groups rather than three: flows that cease reveal that the transaction was not viable once the tax benefit was withdrawn, while flows that persist reveal a non-tax operational motive strong enough to justify paying full U.S. tax. That surviving category still bundles diversification with offshore capital relief, and we return to this distinction in Section 6.2 when we analyze cross-sectional results based on insurers' catastrophe risk exposure.

6.1 Estimating the Substitution Ratio of Internal Capital

The §953(d) election provides a mechanism to unbundle the tax status of an affiliate from its operational location. This institutional feature allows us to decompose the BEAT-induced decline in affiliate cessions into two parts, according to what insurers did when the tax advantage was withdrawn. We classify flows that cease entirely following the tax shock as tax-motivated capital. These transactions represent capital that was located offshore primarily to exploit the tax differential; once the tax advantage is removed by the BEAT, the economic rationale for the transaction evaporates,

[28] See Tang (2024) for regulatory arbitrage across U.S. states.

and the capital exits the internal market. Conversely, flows that continued through an electing affiliate were worth enough, on non-tax grounds, to justify paying full U.S. tax on the affiliate's worldwide income. These transactions represent sticky capital where the operational surplus of the internal market, such as specialized risk pooling or regulatory arbitrage, exceeds the cost of U.S. taxation.

To distinguish between genuine risk-sharing and tax-optimization motives, we hand-collect data on the §953(d) status of foreign affiliates. Specifically, we focus on affiliates located in Bermuda, the Cayman Islands, and Switzerland. The reason is that these jurisdictions are the major countries that simultaneously serve as global reinsurance hubs and tax havens. In 2017, insurance groups in our sample ceded more than $39.7 billion to affiliates in these countries, which corresponds to 88.8% of their total reinsurance cessions to foreign affiliates.[29] An overview of the ten largest reinsurance countries for affiliated reinsurance can be found in Table A8 of the Appendix. At the same time, these countries are among the largest tax havens in the world (Clausing, 2020), where U.S. multinationals report most of their foreign profits.[30]

To measure the share of reinsurance driven by risk-sharing versus tax-optimization motives, we estimate σ as

$$\sigma = \frac{\Delta Premiums^{\text{953d}}}{\Delta Premiums^{\text{Non953d}}}. \tag{7}$$

Specifically, we ask: for each percentage point decline in premiums ceded to non-electing foreign affiliates, how many percentage points are offset by an increase in premiums to §953(d) affiliates? We define the substitution ratio σ as the responsiveness of reinsurance via §953(d) entities to changes in reinsurance via non-electing entities.

The substitution ratio assumes that the decline in non-electing foreign affiliate cessions reflects a genuine restructuring of the reinsurance program. However, the denominator could instead decline mechanically, which would bias our estimate. Suppose treated firms shifted their underwriting toward lines that require less global diversification. They would then cede less offshore simply

[29] Bermuda: $23.5 billion (52.5%), Cayman Islands: $1.0 billion (2.3%), Switzerland: $15.2 billion (34.0%).

[30] Specifically, using 2017 IRS Form 8975 data, Clausing (2020) finds that U.S. multinationals booked $58.5 billion of profit in the Cayman Islands, $49.4 billion in Switzerland, and $32.5 billion in Bermuda, corresponding to 9.2%, 7.7%, and 5.1% of total U.S. foreign profits, respectively. Among all studied jurisdictions, this places the Cayman Islands first, Switzerland third, and Bermuda sixth in booked foreign profit. The effective tax rates for these countries—0.1%, 7.1%, and 2.7%, respectively—are all significantly below the 10% threshold used by Clausing (2020) to define tax haven status.

because they are writing less reinsurance-intensive business, not because they are unwinding tax-driven treaties. Under this scenario, part of the observed decline in non-§953(d) cessions would be a composition effect rather than a behavioral response to the tax. This would inflate our estimate of the tax-motivated share.

We rule out this channel using the portfolio reallocation tests in Table 9. For each insurance group and year, we compute the share of direct premiums written in each line of business. We then construct four measures of portfolio adjustment. Portfolio Distance L1 is the sum of absolute deviations in line-of-business shares from each firm's fixed 2017 portfolio. It captures the total magnitude of reallocation across all lines. Portfolio Distance L2 is the Euclidean distance between the current share vector and the firm's fixed 2017 portfolio. It places greater weight on large deviations in any single line. Both distance measures use 2017 as the fixed reference point and equal zero by construction in that year. The Herfindahl–Hirschman Index measures overall portfolio concentration across lines. CatRisk measures the share of direct premiums written in catastrophe-exposed lines within catastrophe-prone states. Across all four outcomes and both specifications, the coefficient on $Treated^{\text{stat}} \times Post$ is statistically indistinguishable from zero. We find no evidence that treated firms changed their underwriting composition, their portfolio concentration, or their exposure to catastrophe-prone lines after the BEAT. The decline in non-§953(d) cessions thus appears to reflect a genuine restructuring of the reinsurance program, not an insurance portfolio adjustment driven by changes in the underlying business mix. We cannot rule out small adjustments, but nothing in these estimates points to reallocation on a scale that would account for a 15 percentage point contraction in reinsurance cessions.

Additionally, we test whether insurance groups adjusted not only the composition of their underwriting portfolio but also their overall underwriting effort. That is, whether their gross underwriting changed after the BEAT. Table 10 reports year-specific interactions for the simple growth rates of Gross Premiums Written (GPW) in Columns (1)–(2), Direct Premiums Written (DPW) in Columns (3)–(4), and Total Premiums Assumed in Columns (5)–(6). Even-numbered columns add the lagged growth rate of incurred losses to separate underwriting changes from mechanical price movements after loss shocks. For primary underwriting (GPW and DPW), the interactions are indistinguishable from zero through 2020 and turn mildly positive in 2021. There is no year in which treated groups wrote significantly *less* primary business; if anything, primary

Table 9: Portfolio Reallocation

This table reports difference-in-differences (DiD) estimates of the effect of the Base Erosion and Anti-Abuse Tax (BEAT) on the underwriting portfolio composition. For each insurer-year, we compute the share of direct premiums written allocated to each line of business. The outcome variables capture four distinct dimensions of portfolio adjustment. Portfolio Distance (L1), in Columns (1) and (2), is the sum of absolute deviations in line-of-business shares from each firm's 2017 portfolio across all lines. It captures the total magnitude of portfolio reallocation relative to the pre-BEAT structure. Portfolio Distance (L2), in Columns (3) and (4), is the Euclidean distance between the current share vector and the firm's 2017 portfolio. It places greater weight on large deviations in any single line. The Herfindahl–Hirschman Index (HHI), in Columns (5) and (6), measures the overall concentration of the portfolio across all business lines. The catastrophe risk share, in Columns (7) and (8), is the share of direct premiums written in catastrophe-exposed lines within catastrophe-prone states. It captures whether treated firms reduced their exposure to lines that require global diversification. $Treated^{\text{stat}}$ is an indicator variable equal to one if the insurance group met the statutory BEAT thresholds in 2017. These include average annual gross receipts of at least $500 million and a base erosion percentage of at least 3%. $Post$ is an indicator variable for the years 2018–2022. Odd-numbered columns report parsimonious specifications without controls; even-numbered columns add the full set of firm-level controls including $Size$, $Leverage$, $LOB\ Concentration$, $CatRisk$, and $Return\ on\ Equity$. All specifications include group and year fixed effects. Variable definitions are provided in Table A3.

	Portfolio Distance (L1)		**Portfolio Distance (L2)**		**LOB Concentration (HHI)**		**CatRisk**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated^{stat} × Post	0.017	0.005	0.006	0.0003	−0.016	−0.012	−0.0003	−0.001
	(0.029)	(0.023)	(0.014)	(0.012)	(0.013)	(0.011)	(0.001)	(0.001)
CatRisk		−1.648		−1.352		−2.717**		
		(2.985)		(1.461)		(1.308)		
LOB Concentration		0.212		0.058				−0.015
		(0.251)		(0.134)				(0.009)
Control variables	no	yes	no	yes	no	yes	no	yes
Group fixed effects	yes	yes	yes	yes	yes	yes	yes	yes
Time fixed effects	yes	yes	yes	yes	yes	yes	yes	yes
Observations	480	480	480	480	480	480	480	480
Adjusted R^2	0.667	0.690	0.644	0.667	0.973	0.975	0.969	0.970

Notes: All continuous variables are winsorized at the 5th and 95th percentiles, with cutoffs computed separately for treated and control groups. Standard errors are in parentheses and clustered at the insurance group level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

volume goes up late in the window. Assumed premiums are much noisier, with large standard errors throughout and a single marginally significant decline in 2022. We therefore find no evidence that treated groups pulled back on originating insurance business in response to the higher cost of internal capital; the primary-market estimates, which matter most for our interpretation, never point to a contraction. As retained primary volume did not fall while cessions did, the pattern is one of higher net retention, which we take up in Section 7.1.

Table 10: The Impact of the BEAT on Underwriting Volume

This table investigates whether insurance groups adjusted their underwriting activity in response to the increased cost of internal capital. The dependent variables are the simple growth rates of: (i) Gross Premiums Written (GPW), (ii) Direct Premiums Written (DPW), and (iii) Total Premiums Assumed. $Treated^{\text{stat}}$ is an indicator variable equal to one if the insurance group met the statutory BEAT thresholds in 2017. These include average annual gross receipts of at least $500 million and a base erosion percentage of at least 3%. Even-numbered columns include the lagged growth rate of losses incurred, Δ $LossesIncurred$, to isolate underwriting changes from mechanical price adjustments following loss events. Control variables are $Size$, $Leverage$, $LOB\ Concentration$, $CatRisk$, and $Return\ on\ Equity$. Variable definitions are provided in Table A3.

	Gross Premiums Written *simple growth*		**Direct Premiums Written** *simple growth*		**Total Premiums Assumed** *simple growth*	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated^{stat} × 2018	0.006	−0.003	−0.022	−0.029	0.010	0.019
	(0.035)	(0.036)	(0.033)	(0.034)	(0.175)	(0.164)
Treated^{stat} × 2019	0.022	0.012	0.010	0.001	0.178	0.181
	(0.029)	(0.029)	(0.026)	(0.025)	(0.154)	(0.154)
Treated^{stat} × 2020	0.046	0.037	0.046	0.038	−0.038	−0.022
	(0.034)	(0.032)	(0.035)	(0.034)	(0.159)	(0.140)
Treated^{stat} × 2021	0.060*	0.050	0.076**	0.068*	0.255	0.266
	(0.032)	(0.033)	(0.036)	(0.035)	(0.162)	(0.161)
Treated^{stat} × 2022	0.046	0.037	0.053	0.046	−0.259*	−0.246*
	(0.033)	(0.034)	(0.032)	(0.032)	(0.132)	(0.131)
Δ Losses Incurred		0.008		0.006		0.097
		(0.029)		(0.027)		(0.125)
Control variables	yes	yes	yes	yes	yes	yes
Group fixed effects	yes	yes	yes	yes	yes	yes
Time fixed effects	yes	yes	yes	yes	yes	yes
Observations	432	384	432	384	432	384
Adjusted R^2	0.070	0.053	0.134	0.127	0.037	0.063

Notes: All continuous variables are winsorized at the 5th and 95th percentiles, with cutoffs computed separately for treated and control groups. Standard errors are in parentheses and clustered at the insurance group level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

The naive OLS regression (without using the BEAT as identification strategy) to estimate the

substitution ratio would be

$$953d_{i,t} = \gamma_i + \delta_t + \sigma Non953d_{i,t} + \varepsilon_{i,t}, \tag{8}$$

where γ_i denotes group fixed effects and δ_t year fixed effects. The OLS estimates of σ would be biased by simultaneity. A group chooses its cessions to non-electing and to §953(d) affiliates jointly, as part of the same reinsurance program, so neither variable is determined before the other.

To estimate σ, we employ a Two-Stage Least Squares (2SLS) approach, where we use the implementation of the BEAT as an exogenous shock to internal capital market transfers to non-§953(d) affiliates. We instrument for the endogenous decline in non-electing cessions using the interaction $Post \times Treated^{\text{aff}}$. $Post$ is an indicator variable equal to one for years 2018–2022. $Treated^{\text{aff}}$ is an indicator variable equal to one if the insurance group (i) was subject to BEAT in 2017 and (ii) maintained at least one non-electing foreign affiliate in Bermuda, the Cayman Islands, or Switzerland in the pre-reform period. Formally, we test:

$$\begin{aligned} \text{First Stage:} \quad & Non953d_{i,t} = \alpha_i + \lambda_t + \pi \left(Post_t \times Treated_i^{\text{aff}} \right) + \beta \mathbf{X}_{i,t} + \eta_{i,t} \\ \text{Second Stage:} \quad & 953d_{i,t} = \gamma_i + \delta_t + \sigma \widehat{Non953d}_{i,t} + \mu \mathbf{X}_{i,t} + \varepsilon_{i,t}, \end{aligned} \tag{9}$$

where α_i and γ_i represent group fixed effects that absorb time-invariant heterogeneity in reinsurance usage. We include time-fixed effects (λ_t and δ_t) to absorb common time trends that affect all insurers uniformly. We also include the standard set of controls in $\mathbf{X}_{i,t}$ to account for structural differences in our treatment and control group. The coefficient of interest is σ, which we interpret as a policy-induced substitution ratio rather than as a structural elasticity of one cession quantity on the other. A value of $\sigma = -1$ indicates perfect substitution (full offset): every dollar reduction in premiums ceded to non-electing affiliates is fully offset by an equivalent increase in cessions to §953(d) affiliates. A value of $\sigma = 0$ would mean that none of it was rerouted through the §953(d) channel.

Before turning to the conditions on the instrument, we state precisely what σ measures. An insurance group chooses its non-electing and §953(d) cessions jointly, and the BEAT raises the relative price of the non-electing channel. As a result, the BEAT can shift §953(d) demand directly through the allocation decision, not only through the realized quantity of non-electing cessions.

We therefore do not claim the strict exclusion restriction required for a quantity-on-quantity causal interpretation. Instead, we read σ as a *policy-induced substitution ratio*: it is the ratio of the reform's effect on §953(d) cessions (the reduced form) to its effect on non-electing cessions (the first stage), and it scales one behavioral response to the BEAT by the other. Under this reading the design requires two things: that the instrument moves the non-electing channel (relevance), and that $Post \times Treated^{\text{aff}}$ is exogenous, so that the reduced form and first stage both capture responses to the reform rather than to a confound. Relevance is empirically testable and evaluated in the first stage: it holds if BEAT-affected groups significantly reduced their non-§953(d) cession ratios after the 2018 implementation. Exogeneity of the instrument is supported on three grounds. First, the BEAT is a federal tax change that applies to all qualifying groups simultaneously and discontinuously in 2018. Its timing and design were fixed by the TCJA legislative process rather than chosen by firms, so $Post \times Treated^{\text{aff}}$ is plausibly unrelated to unobserved determinants of reinsurance structure. Second, §953(d) entities are by construction exempt from the tax, so the reform does not act on the electing channel directly through the tax base; whatever movement we see in §953(d) cessions is a behavioral response to the higher cost of the non-electing channel. Third, a confound capable of biasing the ratio would have to generate a simultaneous decrease in non-electing cessions and increase in §953(d) cessions concentrated in BEAT-exposed groups. We are aware of no such shock—catastrophe cycles and COVID-related disruptions do not produce this specific pattern. It is worth noting that we do not read σ as a decomposition of the entire pre-reform offshore book. It is a marginal ratio, defined on the flows the policy moved rather than on the stock that preceded it.

The BEAT was implemented together with a reduction in the U.S. corporate tax rate from 35% to 21%. One might argue that the rate cut, rather than the BEAT, affects groups' decision to substitute from non-§953(d) entities to §953(d) entities. A lower domestic rate narrows the tax arbitrage spread and could independently make §953(d) arrangements relatively more attractive. We view this channel as unlikely to account for our estimates for two reasons. First, the corporate rate reduction applies equally to all firms in the treatment and control group, regardless of their BEAT exposure. Our instrument isolates BEAT-exposed groups with a pre-existing non-electing affiliate, and the §953(d) increase is concentrated there. The control group does not show a meaningful response. Second, the impact of the U.S. corporate tax rate reduction is too small to explain the substitution we observe. Consider the following calculation. The BEAT penalty was 10% in the

medium term on premiums ceded, which is a gross flow measure. The rate-cut saving, by contrast, accrues on net income. The median combined ratio for a Bermudian reinsurer was 83.6% between 2013 and 2017,[31] implying a net profit margin on premiums of roughly 16.4%. The rate-cut saving on a dollar of premiums ceded is therefore approximately $(35\% - 21\%) \times 16.4\% \approx 2.30\%$ of premiums, compared to a BEAT cost of 10%—a ratio of roughly 4.3 to 1. Furthermore, 2.30% is an upper bound: premiums ceded to the Cayman Islands represent a small fraction of those ceded to Bermuda, and Switzerland faces a substantially higher corporate tax rate, making tax arbitrage smaller there.[32] The BEAT therefore dominates the rate-cut effect both in magnitude and in its ability to account for the documented substitution pattern. We do not translate either wedge into a predicted cession response, since that mapping would require an elasticity we do not estimate.

Insurance groups have exposure to other taxes as well, which might explain the response we observe. One of them is the Federal Excise Tax (FET) under §4371. This tax levies a 1% charge on reinsurance received from U.S. cedants. The concern is that FET avoidance, rather than the contraction in non-electing flows induced by the BEAT, drives our results. We find this channel unpersuasive for two reasons. First, the FET rate was not modified by the TCJA. For FET avoidance to explain our results, firms' incentives to eliminate this charge would need to have changed discontinuously in 2018, which is not the case. Second, and more fundamentally, both treated and control firms face FET exposure by maintaining foreign affiliates. If FET avoidance were independently driving §953(d) elections, we would expect to observe elevated elections among control firms as well. The reduced form estimate in Table 11 shows precisely the opposite. The increase in §953(d) cessions is concentrated entirely among BEAT-exposed firms. There is no discernible response in the control group. This differential is difficult to reconcile with a potential FET story. We therefore treat the FET channel as at most a secondary benefit. Firms received this benefit while restructuring for BEAT-related reasons. It was not an independent driver of the §953(d) elections we document. Finally, similar to the rate-cut channel, the FET is too small in magnitude (1% on premiums versus 10% BEAT) to explain the magnitude of our estimates.[33]

[31]The numbers refer to Class 4 and Class 3B reinsurers, which represent the largest Bermudian reinsurers. See Bermuda Monetary Authority (2018).

[32]The federal corporate tax rate in Switzerland is 8.5%. See the OECD data set on statutory corporate income tax rates.

[33]We also rule out that the concurrent introduction of the Global Intangible Low-Taxed Income (GILTI) provision could explain our results. GILTI imposes a minimum tax on the income of Controlled Foreign Corporations (CFCs), defined under §951A(a). A potential concern is that GILTI independently incentivized §953(d) elections among

Table 11 presents the results. Column (1) reports the OLS estimate of the relationship between non-§953(d) and §953(d) cessions as stated in Equation (8). The coefficient of -0.256 ($t = -1.97$) indicates that firms with higher non-§953(d) cessions tend to have lower §953(d) cessions, consistent with the two channels being substitutes rather than complements. Column (2) presents the reduced form, regressing §953(d) cessions directly on the instrument. The coefficient of 0.054 is positive and significant at the 5% level. This means that BEAT-exposed firms with a pre-existing non-§953(d) affiliate increased their §953(d) cessions by 5.4 percentage points of gross premiums written after 2017 relative to the control group. Columns (3) and (4) report the first stage of Equation (9), regressing non-§953(d) cessions on the instrument without and with control variables, respectively. The instrument is highly significant in both specifications. In economic terms, BEAT-exposed firms reduced their non-§953(d) cessions by 15 percentage points of gross premiums written relative to control firms after 2017. The first-stage F-statistics indicate strong instruments. We report the Kleibergen and Paap (2006) F-statistic rather than the conventional first-stage F-statistic, as the latter is only valid under homoskedastic errors.[34] The Kleibergen–Paap F-statistics are 15.56 in column (3) and 15.94 in column (4). We report these as descriptive measures of first-stage strength.[35] To address remaining weak-instrument concerns, we instead report the 95% confidence interval for the estimated substitution ratio σ, based on the cluster-robust Anderson and Rubin test (see Andrews et al., 2019). We note that the intervals are bounded and lie strictly within $(-1, 0)$, which supports our inference. Columns (5) and (6) present the second stage estimates, which identify the substitution ratio σ. To account for the uncertainty arising from the estimation of the first stage, we base inference in the second stage additionally on a cluster bootstrap-t procedure (Cameron & Miller, 2015).[36] We report adjusted p-values in brackets.

BEAT-affected firms. This concern is, however, logically inconsistent with the structure of the tax arbitrage we study. Had the non-electing foreign affiliates been CFCs, Subpart F would have already rendered the offshore structure tax-inefficient by accounting for their insurance income in their U.S. parent's taxable income under §953(a) (Hepfer et al., 2025). The non-electing affiliates in our sample are non-CFCs. Since GILTI requires CFC status under §951A(a), it simply does not apply to them, and its introduction creates no independent incentive to elect §953(d) status.

[34]Since our setting includes group and time fixed effects with standard errors clustered at the insurance group level, the Kleibergen–Paap statistic is the appropriate measure of instrument strength as it is robust to heteroskedasticity and clustering.

[35]We do not compare them to the Stock and Yogo (2005) critical values. Those values are derived within the homoskedastic Cragg and Donald (1993) framework and thus provide no comparable test size.

[36]In each of 1,000 replications, we resample insurance groups with replacement, re-estimate the full two-stage model, and compute the studentized statistic for the coefficient of interest. We then obtain p-values by comparing the original t-statistic with the bootstrap distribution of these studentized statistics.

The coefficient on the instrumented variable, $\widehat{Non953d}$, is -0.355 without controls and -0.360 with controls. Under the cluster bootstrap-t, both estimates are significant at the 10% level. Taken together with the first stage magnitude of 15 percentage points, this implies a §953(d) increase of roughly 5.4 percentage points, matching the reduced form in Column (2). The IV estimate is larger in magnitude than OLS (-0.360 versus -0.256). This ordering is consistent with attenuation bias from endogenous co-movement between the two cession streams that the instrument corrects, with omitted variables that bias OLS toward zero, or with the IV recovering the response of firms whose cessions were most sensitive to the BEAT. We do not attempt to distinguish these explanations. A Wu–Hausman test rejects the exogeneity of non-electing cessions at the 1% level across all specifications, which indicates that OLS and IV differ by more than sampling variation would suggest; conditional on the validity of the instrument discussed above, this favors the IV specification.

Our estimated σ of -0.36 means that for each percentage point of BEAT-induced decline in non-electing cessions, about 0.36 percentage points reappeared as §953(d) cessions. Equivalently, roughly 36% of the flows the BEAT drove out of the non-electing channel were valuable enough on non-tax grounds to be preserved at the cost of full U.S. taxation. The remaining 64% were not redirected to an electing affiliate. We do not make any statement about where that 64% went. As shown earlier, we cannot rule out partial substitution toward third-party foreign reinsurers, and some of the flows may have migrated there rather than collapsing entirely. However, no other reinsurance market restores the tax arbitrage that the non-electing structure had provided: an electing affiliate pays full U.S. tax, a domestic affiliate is onshore, and an unaffiliated reinsurer charges an arm's-length margin. We therefore read the 64% as primarily tax-driven capital. These are flows whose viability depended on the tax differential, and which the BEAT made uneconomic once it removed that differential. This reading is consistent with the only partial external substitution we document, and it does not require the flows to have carried no insurance risk. As Section 7.1 shows, treated groups retained more risk and their net unpaid losses rose, so the discontinued cessions did cover genuine exposure. We classify them as tax-motivated because the net present value of the transfer turned negative once the tax benefit was removed.

Table 11: The Substitution Ratio of Internal Capital

This table examines whether insurance groups substitute toward §953(d)-electing foreign affiliates in response to declining cessions to BEAT-exposed foreign affiliates. Specifically, it reports baseline estimates where the dependent variable in the OLS, reduced form, and second-stage specifications is the §953(d) cession ratio, defined as the share of premiums ceded to Bermuda, Switzerland, and the Cayman Islands affiliates operating under §953(d) elections. Column (1) reports OLS estimates of the effect of the non-§953(d) cession ratio on the §953(d) cession ratio. Column (2) reports the reduced-form estimate, directly regressing the §953(d) cession ratio on the instrument. Columns (3) and (4) report the first-stage regressions, where the dependent variable is the non-§953(d) cession ratio. Columns (5) and (6) report the two-stage least squares estimates, in which the non-§953(d) cession ratio is instrumented by its predicted value from the corresponding first stage. The instrument, $Treated^{\text{aff}} \times Post$, is the interaction of an indicator for the post-BEAT period (2018 onward) with an indicator for BEAT-affected insurance groups holding non-§953(d) affiliates before 2018. Control variables are $Size$, $Leverage$, $LOB\ Concentration$, $CatRisk$, and $Return\ on\ Equity$. Variable definitions are provided in Table A3. All specifications include group and year fixed effects. Standard errors are clustered at the group level. For the second-stage IV estimates in columns (5) and (6), we additionally report cluster bootstrap-t p-values in brackets and base significance stars on them. The bootstrap-t procedure uses 1,000 replications: in each replication we resample insurance groups with replacement, re-estimate the full two-stage model, and compute the studentized statistic for the coefficient of interest; p-values compare the original t-statistic with the bootstrap distribution of these statistics. For the first-stage regressions, we report the Kleibergen–Paap rk Wald F-statistic. For the second-stage estimates, we additionally report the 95% Anderson–Rubin confidence interval for the substitution ratio σ, obtained by inverting the Anderson–Rubin test with standard errors clustered at the group level.

	OLS	**Reduced Form**	**IV: 1st stage**		**IV: 2nd stage**	
	Dep. var: 953d	*Dep. var: 953d*	*Dep. var: Non953d*		*Dep. var: 953d*	
	(1)	(2)	(3)	(4)	(5)	(6)
Non953d	-0.256^{*}					
	(0.130)					
$\widehat{\text{Non953d}}$					-0.355^{*}	-0.360^{*}
					(0.151)	(0.149)
					[0.076]	[0.073]
Treated^{aff} × Post		0.054^{**}	-0.150^{***}	-0.151^{***}		
		(0.026)	(0.038)	(0.038)		
Control variables	Yes	Yes	No	Yes	No	Yes
Group fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Kleibergen-Paap F-stat	—	—	15.56	15.94	—	—
95% AR confidence set	—	—	—	—	[-0.70, -0.03]	[-0.69, -0.03]
Observations	480	480	480	480	480	480
Adjusted R^2	0.684	0.654	0.757	0.756	0.653	0.654

Notes: All continuous variables are winsorized at the 5th and 95th percentiles, with cutoffs computed separately for treated and control groups. Standard errors are in parentheses and bootstrap-t p-values are in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

6.2 Limits to Internal Capital Market Adjustments

The aggregate substitution ratio of $\sigma \approx -0.36$ indicates that a small but critical fraction of internal capital flows persisted despite removed tax benefits. This subsection asks which insurance groups account for that surviving third. If the sticky flows reflect a genuine operational need for global diversification rather than residual tax arbitrage, they should be concentrated among the groups whose books most require it. We therefore let the substitution ratio vary with a group's catastrophe exposure. The prediction is that groups that underwrite more insurance with exposure to natural catastrophes redirect a larger share of the lost capacity to the tax-neutral §953(d) channel, accepting full U.S. taxation to preserve global risk pooling. Formally, σ should be increasing in catastrophe exposure. We proxy the need for diversification with $CatExposure$, the premium-weighted hazard intensity of the states in which a group writes. The variable captures a group's realized exposure to catastrophic loss independent of how its business lines are classified. We standardize $CatExposure$ across insurance groups in our sample to have a mean of zero and a standard deviation of one. Throughout we use the affiliate-based indicator $Treated^{\text{aff}}$ and the same sample as the substitution analysis, so the estimates below are directly comparable to the aggregate σ. We estimate the following triple DiD model:

$$\begin{aligned} Y_{i,t} = \alpha_i + \lambda_t + \beta_1 \left(Treated_i^{\text{aff}} \times Post_t \times CatExposure_i \right) + \beta_2 \left(Treated_i^{\text{aff}} \times Post_t \right) \\ + \beta_3 \left(Post_t \times CatExposure_i \right) + \gamma \mathbf{X}_{i,t} + \varepsilon_{i,t}, \end{aligned} \tag{10}$$

where α_i and λ_t are group and year fixed effects. $Treated_i^{\text{aff}}$ equals one if the insurance group met the statutory BEAT thresholds in 2017 and held a pre-reform non-electing foreign affiliate in Bermuda, the Cayman Islands, or Switzerland. $Post_t$ equals one for 2018–2022. $\mathbf{X}_{i,t}$ is the standard vector of time-varying controls. The coefficient of interest is β_1. Since $CatExposure$ is standardized, β_1 measures how the post-reform response of treated groups changes with a one-standard-deviation increase in catastrophe exposure. We estimate Equation (10) separately for the two channels of the substitution ratio: non-electing cessions in the denominator and §953(d) cessions in the numerator.

Table 12 previews the mechanism with a simple sort of groups into low and high catastrophe exposure (by the sample median) and treated versus control. It reports mean cession ratios before and after the BEAT implementation. Treated groups with low exposure cut their non-electing cessions

from 15.7% to 4.8% of gross premiums written, while their §953(d) cessions barely move (1.7% to 1.8%). They exit the taxed channel and do not redirect. Treated groups with high exposure instead cut non-electing cessions more sharply, from 23.4% to 2.8%, and simultaneously raise §953(d) cessions from essentially zero to 7.6%. Neither control cell shows this substitution. Low-exposure controls are flat on both channels, and high-exposure controls reduce both channels roughly in parallel, consistent with a common secular decline rather than a BEAT-driven reallocation. The implied ratio of the §953(d) increase to the non-electing decline is about 0.01 for low-exposure treated groups and about 0.36 for high-exposure treated groups. The triple-difference below isolates the treated-high-exposure cell, netting out both the common post-reform movement (through $Post \times CatExposure$) and the average treated response (through $Treated^{\text{aff}} \times Post$).

Table 12: Catastrophe Exposure: Descriptive Evidence

This table reports average cessions to non-electing and to electing foreign affiliates in Bermuda, the Cayman Islands, and Switzerland before and after the Base Erosion and Anti-Abuse Tax (BEAT). We distinguish between treated and control groups with low and high catastrophe exposure. Cessions are scaled by gross premiums written (GPW). The column *Pre* refers to the period 2013–2017 and *Post* to 2018–2022; *Change* is the difference between the two. Treated groups are those with $Treated^{\text{aff}} = 1$, that is, groups that met both statutory BEAT thresholds in 2017 and ceded premiums to at least one non-electing affiliate in these jurisdictions before the reform. Catastrophe exposure is high if a group's $CatExposure$ equals or exceeds the sample median, and low otherwise. N denotes the number of insurance groups in each cell. Variable definitions are provided in Table A3.

Group / channel	Pre	Post	Change
Treated, low catastrophe exposure ($N = 8$)			
Non-953(d)	0.1567	0.0483	-0.1084
953(d)	0.0173	0.0181	0.0009
Treated, high catastrophe exposure ($N = 9$)			
Non-953(d)	0.2337	0.0284	-0.2053
953(d)	0.0022	0.0757	0.0736
Control, low catastrophe exposure ($N = 16$)			
Non-953(d)	0.0373	0.0419	0.0046
953(d)	0.0189	0.0146	-0.0043
Control, high catastrophe exposure ($N = 15$)			
Non-953(d)	0.0476	0.0222	-0.0254
953(d)	0.0471	0.0230	-0.0241

Notes: All continuous variables are winsorized at the 5th and 95th percentiles, with cutoffs computed separately for treated and control groups.

Table 13 estimates the triple-DiD in Equation (10) separately for the two channels: the denominator of σ, non-electing cessions, in Columns (1)–(2), and the numerator, §953(d) cessions, in

Columns (3)–(4). For the denominator, the triple interaction $Treated^{\text{aff}} \times Post \times CatExposure$ is negative and significant at the 10% level (-0.128 and -0.125). That is, a one-standard-deviation increase in catastrophe exposure is associated with a further 12.5–12.8 percentage points of contraction in non-electing cessions, on top of the 13.3-point average contraction at mean exposure. For the numerator, the average treated effect at mean exposure is positive and significant at the 10% level (0.045 and 0.043), so groups do redirect toward the electing channel on average. The triple interaction in Columns (3)–(4) is positive but not individually significant (0.074 and 0.077), even though its magnitude matches the descriptive gap of about 7 percentage points between the two treated cells. The numerator's imprecise exposure coefficient is expected and is why we do not test the hypothesis coefficient by coefficient. What the catastrophe exposure story predicts is a pattern in the *ratio* of the two responses, not in either response alone. This ratio is σ, which we estimate as a function of catastrophe exposure and construct confidence sets for it.

We estimate both channels from a single regression rather than from the two-channel specifications in Table 14. We stack the non-electing and §953(d) observations and interact every term with a channel indicator, with fixed effects on group $\times$ channel and year $\times$ channel and standard errors clustered on the insurance group. As such, we get the joint covariance of the treated marginal effects $(\hat{\theta}_N, \hat{\theta}_D)$. At catastrophe exposure c (in standard-deviation units from the mean), the treated marginal effect on each channel is the relevant linear combination of the stacked coefficients, and the exposure-specific substitution ratio is $\sigma(c) = \hat{\theta}_N(c)/\hat{\theta}_D(c)$. We build Fieller confidence sets for $\sigma(c)$, since the Fieller method, unlike the delta method, remains valid when the denominator is weak and allows the intervals to be asymmetric. Although our denominator is strongly identified, we use it as the appropriate method for inference on $\sigma(c)$.

The estimates support the prediction. At mean exposure $\sigma = -0.34$, the 90% Fieller set excludes zero, so substitution into the electing channel is present even for the average treated group. This value reproduces the aggregate IV estimate of -0.36 almost exactly. The point estimate decreases monotonically with exposure, to -0.42 at half a standard deviation above the mean and -0.46 at one standard deviation. The 90% set excludes zero at every level. We do not claim that σ is significantly higher for high-exposure groups. The confidence sets overlap heavily across exposure levels and the numerator's exposure gradient is not individually significant. Therefore, the increase is suggestive of the diversification mechanism rather than sharp evidence for it. What the analysis

Table 13: Catastrophe Exposure and the Substitution Ratio

This table reports triple DiD estimates from Equation (10) of how catastrophe exposure shapes the two channels of the substitution ratio. The dependent variable is premiums ceded to non-electing foreign affiliates (the denominator of σ) in Columns (1)–(2) and to §953(d)-electing foreign affiliates (the numerator) in Columns (3)–(4), each scaled by GPW. $Treated^{\text{aff}}$ equals one for groups that met the statutory BEAT thresholds in 2017 and held a pre-reform non-electing affiliate in Bermuda, the Cayman Islands, or Switzerland. $Post$ indicates 2018–2022. $CatExposure$ is the standardized premium-weighted state hazard intensity. The coefficient of interest is the triple interaction $Treated^{\text{aff}} \times Post \times CatExposure$. Odd-numbered columns include only the interaction terms; even-numbered columns add the full set of firm-level controls—$Size$, $Leverage$, $LOB\ Concentration$, $CatRisk$, and $Return\ on\ Equity$. All specifications include group and year fixed effects. Variable definitions are provided in Table A3.

	Non-953(d) affiliates *denominator*		**953(d) affiliates** *numerator*	
	(1)	(2)	(3)	(4)
Treated^{aff} × Post × CatExposure	−0.128*	−0.125*	0.074	0.077
	(0.064)	(0.066)	(0.059)	(0.058)
Treated^{aff} × Post	−0.133***	−0.135***	0.045*	0.043*
	(0.033)	(0.032)	(0.023)	(0.022)
Post × CatExposure	−0.007	−0.008	−0.006	−0.007
	(0.009)	(0.010)	(0.004)	(0.005)
Control variables	no	yes	no	yes
Group fixed effects	yes	yes	yes	yes
Time fixed effects	yes	yes	yes	yes
Observations	480	480	480	480
Adjusted R^2	0.780	0.779	0.670	0.669

Notes: All continuous variables are winsorized at the 5th and 95th percentiles, with cutoffs computed separately for treated and control groups. Standard errors are in parentheses and clustered at the insurance group level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

establishes is that the surviving third of internal capital is genuinely redirected to a fully taxed affiliate rather than merely lost, and that this redirection is economically large where the need for global diversification is greatest. This locates the sticky capital in operational value. It does not, on its own, separate diversification from offshore regulatory-capital relief, a distinction we do not claim to resolve.

Table 14: The Substitution Ratio across Catastrophe Exposure

This table reports the substitution ratio $\sigma(c)$ as a function of catastrophe exposure c. Specifically, the substitution ratio is defined as $\sigma(c) = \theta_N(c)/\theta_D(c)$, where $\theta_D(c)$ and $\theta_N(c)$ are the treated marginal effects of the BEAT on non-electing and §953(d) cessions at catastrophe exposure c, measured in standard deviations from the sample mean. Both effects are estimated from a single regression in which the non-electing and §953(d) observations are stacked and fully interacted with a channel indicator, with fixed effects on group $\times$ channel and year $\times$ channel and standard errors clustered on the insurance group. By doing this, we get the joint covariance of (θ_N, θ_D). We construct Fieller confidence sets for $\sigma(c)$. Critical values use Student's t with $G - 1 = 47$ degrees of freedom, where G is the number of insurance groups. For each exposure level we report the point estimate $\sigma(c)$, the 90% and 95% Fieller sets, and the t-statistic on the denominator $\theta_D(c)$. Variable definitions are provided in Table A3.

Exposure level	σ	90% set	95% set	t (denominator)
Mean ($c = 0$)	-0.340	[-0.661, -0.059]	[-0.750, 0.008]	-4.018
$+0.5$ SD	-0.418	[-0.705, -0.101]	[-0.775, -0.014]	-4.011
$+1$ SD	-0.457	[-0.788, -0.043]	[-0.871, 0.096]	-3.418

Notes: All continuous variables are winsorized at the 5th and 95th percentiles, with cutoffs computed separately for treated and control groups.

7. Risk Repatriation and the Regulatory Solvency Blind Spot

Sections 5 and 6 show that the BEAT sharply reduced cross-border affiliate reinsurance and that only about a third of the displaced flows returned through fully taxed §953(d) affiliates. Reinsurance contracts shift economic risk. When a contract is cancelled and not replaced by external reinsurance, the profit leaves the tax base of the foreign affiliate, but the insurance risk returns to the domestic ceding insurer. Taxing internal capital markets thus trades a smaller scope for profit shifting against less risk sharing, and this section turns to the second side of this trade-off. Specifically, it asks how much risk U.S. insurers bear after the BEAT. This question matters because risk that was previously pooled across global groups is now concentrated on the balance sheets on which U.S. policyholders depend. It also raises the question of whether solvency regulation, which is designed

to require capital in proportion to the risk an insurer retains, recognizes this shift. Section 7.1 therefore examines whether affected insurers retain more risk on their own balance sheets, and Section 7.2 examines whether the risk-based capital formula translates this retained risk into higher required capital.

7.1 Risk Repatriation

We start by testing whether treated insurers kept more insurance risk on their domestic balance sheets. Specifically, we regress net premiums written on our interaction term $Treated^{\text{stat}} \times Post$. We also study heterogeneity across business lines, asking which insurance risk was repatriated to domestic balance sheets. We distinguish between catastrophe-exposed lines and long-tail lines.[37] Catastrophe risk requires global diversification, and catastrophe-exposed insurers show a more pronounced shift toward §953(d) affiliates. We would therefore expect the change in repatriated premiums to be concentrated in long-tail lines rather than catastrophe lines.

Net premiums written are direct plus assumed premiums minus ceded premiums. They therefore reflect the amount of premiums that stays on the U.S. balance sheet. As shown in Section 6, underwriting volume did not change after the BEAT. Table 15 presents the results. We scale net premiums written by gross premiums written, so the interpretation is: when an insurance group writes \$1, how much of it stays on its domestic balance sheet?

The overall treatment effect in Column (1) is positive and significant ($\beta = 0.083$, $t = 2.18$). BEAT-affected groups thus retained substantially more net premiums after the reform, in line with the substitution gap found earlier. Again, the effect is driven entirely by the most exposed groups ($\beta = 0.154$, $t = 3.50$): for every \$1 they write, they keep about \$0.15 more on their own balance sheet. The line-of-business split reveals where this retention landed. For $HighBEP$ groups, net retention in long-tail lines rises by 7.8 percentage points ($t = 3.12$), against 3.6 percentage points in catastrophe lines ($t = 1.69$); the $LowBEP$ estimates are small and insignificant on both margins, consistent with these firms showing no significant retention response. To test whether retention rose more in long-tail than in catastrophe business, we regress the difference between the two

[37] Catastrophe-exposed lines: allied lines, auto physical damage, commercial multiple peril, earthquake, farmowners multiple peril, fire, homeowners multiple peril, inland marine. Long-tail lines: commercial auto liability, medical professional liability, other liability, product liability, workers' compensation, private passenger auto liability.

Table 15: Risk Repatriation: Net Retention by Line of Business

This table investigates in which business lines insurance risk was repatriated following the Base Erosion and Anti-Abuse Tax (BEAT). The dependent variables are net premiums written (NPW) scaled by gross premiums written (GPW), decomposed into three categories. *NPW: All Lines* captures total net retention across the full underwriting portfolio. *NPW: CAT Lines* captures net retention exclusively in catastrophe-exposed business lines. *NPW: Long-Tail Lines* captures net retention in lines characterized by extended claims development periods, including liabilities and compensation lines. $Treated^{\text{stat}}$ is an indicator variable equal to one if the insurance group met the statutory BEAT thresholds in 2017, comprising average annual gross receipts of at least $500 million and a base erosion percentage of at least 3%. $Post$ is an indicator variable for the years 2018–2022. Odd-numbered columns report the average treatment effect using the binary $Treated^{\text{stat}}$ indicator. Even-numbered columns replace the binary indicator with $LowBEP \times Post$ and $HighBEP \times Post$, where $LowBEP$ and $HighBEP$ denote indicators for treated groups below and above the median 2017 base erosion percentage, respectively. All specifications include the full set of firm-level controls—$Size$, $Leverage$, $LOB\ Concentration$, $CatRisk$, and $Return\ on\ Equity$—as well as group and year fixed effects. Variable definitions are provided in Table A3.

	NPW: All Lines		**NPW: CAT Lines**		**NPW: Long-Tail Lines**	
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{Treated}^{\text{stat}} \times \text{Post}$	0.083**		0.011		0.051**	
	(0.038)		(0.018)		(0.019)	
LowBEP × Post		0.011		−0.014		0.023
		(0.038)		(0.021)		(0.021)
HighBEP × Post		0.154***		0.036*		0.078***
		(0.044)		(0.021)		(0.025)
Control variables	yes	yes	yes	yes	yes	yes
Group fixed effects	yes	yes	yes	yes	yes	yes
Time fixed effects	yes	yes	yes	yes	yes	yes
Observations	480	480	480	480	480	480
Adjusted R^2	0.819	0.837	0.928	0.932	0.920	0.922

Notes: All continuous variables are winsorized at the 5th and 95th percentiles, with cutoffs computed separately for treated and control groups. Standard errors are in parentheses and clustered at the insurance group level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

net-retention ratios on the same specification, so the coefficient is the differential response with its own clustered standard error.[38] The differential is positive and significant at the 10% level. We do not read the catastrophe estimate as zero as the data do not rule out a modest increase there. Rather, when the tax advantage disappeared, long-tail risk was retained at higher rates, while catastrophe risk largely stayed pooled within the group's offshore affiliates.

These findings are consistent with the view that the terminated contracts were the ones whose viability depended most heavily on the tax differential. The two business line groups differ in where the gains from an offshore affiliate come from. Catastrophe losses are highly correlated within a peril region, so a domestic book cannot pool them at any scale; therefore, global diversification is the only route to a smaller joint tail. Long-tail losses are more nearly independent across policies and states, so a large U.S. book already realizes much of the available pooling gain and the marginal benefit of an offshore affiliate is correspondingly smaller. We do not claim it is zero: long-tail liabilities share claims-inflation, legal, and reserve-development shocks that an affiliate with non-U.S. business could in principle help absorb. In addition, long-tail risks bind the most capital under the NAIC RBC framework, so ceding them to a lighter-touch regime releases regulatory capital, and as Seessel (2001) notes, the reserves backing them can sit offshore and be invested over a long horizon at lower tax rates. What the repatriation pattern reveals is therefore a ranking rather than an absence: for long-tail lines, the combined non-tax offshore value of diversification and regulatory capital relief was not large enough to justify electing §953(d) and paying full U.S. tax, whereas for catastrophe lines it was.

We next ask whether the premiums retained after the reform translate into a larger stock of risk on U.S. balance sheets. We use unpaid losses and loss adjustment expenses (LAE) from Schedule P, Part 1. Unpaid losses measure the insurer's outstanding claim obligations. Gross unpaid losses measure the exposure the insurer has written. Ceded unpaid losses measure the part transferred to reinsurers. Net unpaid losses, the difference between the two, measure the part that stays on the U.S. balance sheet. This decomposition separates what a group underwrites from what it keeps. We scale these measures by lagged total assets rather than by premiums. Premium-based ratios measure losses

[38]The dependent variable is the group-year difference between the long-tail and catastrophe net-retention ratios, $NPW_{i,t}^{\text{long-tail}}/GPW_{i,t} - NPW_{i,t}^{\text{cat}}/GPW_{i,t}$. We include group and year fixed effects and the common control variables except for $CatRisk$, with standard errors clustered at the insurance group level. The differential for $HighBEP$ groups is 5.8 percentage points ($t = 1.69$, $p = 0.10$); for $LowBEP$ groups it is smaller and insignificant (3.7 percentage points, $t = 1.63$).

relative to the size of a particular underwriting activity. For insurer solvency, however, the relevant question is whether the insurer can absorb the retained exposure. Total assets are the resources from which an insurer ultimately pays its claims, so scaling by assets measures the retained exposure relative to this capacity.

We distinguish between unpaid losses from the current accident year and unpaid losses from all accident years. Losses from the current accident year are ceded under that year's reinsurance contracts and respond immediately. Losses from all accident years remain covered by the contracts in place when they occurred. We expect the BEAT to affect reserves for the current accident year immediately, whereas the total stock of reserves, which is dominated by earlier accident years that remain reinsured under pre-reform contracts, should adjust only gradually as new accident years replace old ones. Table 16 reports the results.

For $HighBEP$ groups, gross unpaid losses from the current accident year do not change significantly (-0.022, $t = -1.22$). Ceded unpaid losses fall by 4.5 percentage points of lagged assets ($t = -2.65$). Net unpaid losses rise by 1.9 percentage points ($t = 1.73$). Groups thus continue to write business at a similar scale but pass a smaller share of it on to reinsurers. The retention share gives the same picture. Treated insurers kept a larger share of both the premiums they wrote and the claims those premiums generated. None of these effects appear for $LowBEP$ groups. Across all accident years, none of the estimates is statistically significant. This pattern follows from how the stock of unpaid losses builds up. Claims from earlier accident years remain ceded under the contracts in place when they occurred and stay on the balance sheet until they are paid. The BEAT changes only the reinsurance of new accident years. Over our sample period, this gradual shift is small relative to the large variation in the stock of unpaid losses. We also compare net unpaid losses between long-tail and catastrophe lines. Based on the point estimates, net unpaid losses increase more in long-tail lines than in catastrophe lines, both for the current accident year and for all accident years. However, neither coefficient is statistically significant.

7.2 Decoupling of Economic and Reported Solvency

Section 7.1 documents that the BEAT changed the economic exposure of the most affected groups. If regulatory capital reflects retained risk, these groups should subsequently be required to hold

Table 16: Unpaid Losses and Loss Adjustment Expenses

This table tests whether the contraction in internal capital markets following the Base Erosion and Anti-Abuse Tax (BEAT) shifted insurance risk back onto the domestic balance sheets. All dependent variables are constructed from unpaid losses and loss adjustment expenses (LAE) reported in Schedule P, Part 1: gross (direct and assumed), ceded to reinsurers, and net (gross minus ceded). Columns (1)–(6) use unpaid losses and LAE from the current accident year, which capture newly written business. Columns (7)–(12) use unpaid losses and LAE across all accident years, which capture the accumulated stock. Within each block, the first column reports gross unpaid losses and LAE, which measure the underlying insured exposure; the second reports ceded unpaid losses and LAE, the exposure transferred to reinsurers; the third reports net unpaid losses and LAE, the exposure retained on the U.S. balance sheet; and the fourth reports the ratio of net to gross unpaid losses and LAE, the fraction of loss exposure retained. The fifth and sixth columns restrict net unpaid losses and LAE to catastrophe-exposed lines and long-tail lines, respectively. Except for the retention share in Columns (4) and (10), all outcomes are scaled by lagged total assets. $LowBEP$ and $HighBEP$ are indicators based on a median split of the 2017 base erosion percentage among groups that met both statutory BEAT thresholds in 2017: average annual gross receipts of at least $500 million and a base erosion percentage of at least 3%. $Post$ equals one for 2018–2022. Control variables are $Leverage$, $LOB\ Concentration$, $CatRisk$, and $Return\ on\ Equity$; we omit $Size$ because the logarithm of total assets is mechanically related to the denominator of the outcomes. Variable definitions are provided in Table A3.

	Current Accident Year						**All Accident Years**					
	Gross */ assets*	**Ceded** */ assets*	**Net** */ assets*	**Net** */ gross*	**Net: CAT** */ assets*	**Net: Long-Tail** */ assets*	**Gross** */ assets*	**Ceded** */ assets*	**Net** */ assets*	**Net** */ gross*	**Net: CAT** */ assets*	**Net: Long-Tail** */ assets*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
LowBEP × Post	0.010	0.009	0.004	−0.030	−0.001	0.005	0.035	0.032	0.007	−0.026	0.0005	0.011
	(0.012)	(0.010)	(0.008)	(0.036)	(0.003)	(0.006)	(0.034)	(0.032)	(0.022)	(0.038)	(0.005)	(0.018)
HighBEP × Post	−0.022	−0.045**	0.019*	0.118**	0.003	0.013	−0.044	−0.065	0.003	0.035	0.005	0.011
	(0.018)	(0.017)	(0.011)	(0.054)	(0.003)	(0.008)	(0.049)	(0.053)	(0.027)	(0.044)	(0.005)	(0.024)
Control variables	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Group fixed effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Time fixed effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	432	432	432	480	432	432	432	432	432	480	432	432
Adjusted R^2	0.810	0.777	0.780	0.754	0.858	0.838	0.890	0.858	0.841	0.771	0.892	0.864

Notes: All continuous variables are winsorized at the 5th and 95th percentiles, with cutoffs computed separately for treated and control groups. Standard errors are in parentheses and clustered at the insurance group level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

more capital. In this section, we ask how the NAIC Risk-Based Capital (RBC) formula translates these changes in exposure into capital charges. Our central claim is that the formula rewards a firm for shedding reinsurance even when that firm retains more risk overall.

A change in reinsurance enters the RBC formula through two channels that move in opposite directions. The first is the *credit channel*. Before 2018, the formula charged a flat 10% on reinsurance recoverables from unaffiliated and foreign affiliated reinsurers, irrespective of the reinsurer's credit quality. Half of this charge is booked in R3 and the other half in R4. We combine both pieces into a single credit charge:

$$CreditCharge_{i,t} = 0.10 \times Recoverables_{i,t} = \mathrm{R3}_{i,t} + \mathrm{R4}^{\mathrm{rec}}_{i,t}, \tag{11}$$

where $\mathrm{R4}^{\mathrm{rec}}_{i,t}$ is the recoverables part of R4, which excludes net reserves. The second is the *underwriting channel*. When an insurer cedes less, it retains more reserves and more premiums, which raise the reserve-risk (R4) and premium-risk (R5) charges. We define

$$UnderwritingCharge_{i,t} = \mathrm{R4}^{\mathrm{net}}_{i,t} + \mathrm{R5}_{i,t}, \tag{12}$$

where $\mathrm{R4}^{\mathrm{net}}_{i,t} = \mathrm{R4}_{i,t} - \mathrm{R4}^{\mathrm{rec}}_{i,t}$ is the reserve-risk charge excluding the credit risk component generated by reinsurance recoverables. The central test is whether the two effects offset each other. On the one hand, affected insurers ceded less to affiliated foreign reinsurers after the BEAT, which lowers their recoverables and thus the credit charge. On the other hand, the risk they no longer cede remains on their own balance sheets, which raises the underwriting charge. The two channels offset each other if

$$-\Delta\, CreditCharge \approx \Delta\, UnderwritingCharge.$$

For each year, we recompute the buckets from that year's statutory filing, using the pre-2018 method for the credit charge throughout. The resulting ACL applies the risk factors in force in each year, but holds the rule for the credit charge fixed. It differs from the ACL insurers reported after 2018, and it isolates changes in exposure from the 2018 change in the credit-risk rule.[39]

[39]We thank S&P Global Market Intelligence for providing us with historical templates to gather the historical bucket calculations. In 2018, the NAIC changed how it calculates the credit risk charge. To ensure our results reflect the response to the BEAT rather than this regulatory change, we compute R3 for all post-BEAT years using the pre-period

To quantify the capital released through the credit channel, we compute for each insurer and year the actual required capital, $ACL_{i,t}^{\text{actual}}$, and a counterfactual, $ACL_{i,t}^{\text{no credit release}}$. The counterfactual uses the insurer's actual post-BEAT net reserves, premiums and asset charges, but holds its reinsurance intensity at the pre-reform level. Specifically, we compute each insurer's ratio of recoverables to gross premiums written over 2013–2017 and, from 2018 onward, set counterfactual recoverables equal to this ratio times the insurer's actual gross premiums written. The credit charge is then computed from these counterfactual recoverables.[40] The difference

$$CreditRelease_{i,t} = ACL_{i,t}^{\text{no credit release}} - ACL_{i,t}^{\text{actual}} \tag{13}$$

measures the regulatory capital released through the reduction in reinsurance recoverables. All outcomes are dollar amounts scaled by lagged total assets, which puts recoverables, reserves, premiums and capital charges on a common scale. We estimate the same difference-in-differences specification as in Section 7.1, with group and year fixed effects and the $LowBEP$/$HighBEP$ split. As in Section 7.1, we do not control for $Size$, because the logarithm of assets is mechanically related to the denominator of the outcomes.

Table 17 presents the results. Column (1) confirms the credit channel. For $HighBEP$ groups, the credit charge falls by 0.92 percentage points of lagged assets ($t = -1.91$), which corresponds to a decline in charged recoverables of 9.2 percentage points of lagged assets. For $LowBEP$ groups, the credit charge does not change. One might argue that the decline in the credit charge reflects an improvement in the collectibility of the remaining recoverables. To address this concern, we test whether the share of overdue recoverables in total recoverables, a proxy for collectibility problems, changes after the BEAT. Column (2) reports the results. The coefficients are statistically insignificant for both groups, and the point estimates are positive, so if anything, the share of overdue recoverables rises. We therefore find no evidence that collectibility improved. The credit charge

method. Specifically, we take each insurer's recoverables from unaffiliated and foreign affiliated reinsurers and apply a 10% charge. Recoverables from domestic affiliates are not charged under the RBC framework, so we exclude them. The factor is flat at 10% and does not vary with the counterparty's identity or rating. This follows Odomirok et al. (2015). We assign half of the resulting charge to R3 and half to R4. We split it this way because the reserve charge in R4 typically exceeds the combined charge for recoverables and non-invested assets (Odomirok et al., 2015).

[40] Scaling by gross premiums written, rather than freezing recoverables at a fixed dollar amount, allows counterfactual recoverables to grow with the insurer's business, so that the counterfactual isolates the change in reinsurance intensity rather than changes in size.

falls because the volume of recoverables falls, not because the remaining recoverables became safer. Columns (3)–(5) show the underwriting channel. The premium-risk charge R5 rises by 0.86 percentage points of lagged assets for $HighBEP$ groups ($t = 1.85$), consistent with the retention of new premiums. The reserve-risk charge excluding recoverables does not change (Column 3). This matches the evidence in Section 7.1: the BEAT taxes reinsurance premiums, not existing reserves, so claims from earlier accident years remain covered under pre-BEAT contracts, and the stock of net reserves adjusts only gradually. The combined underwriting charge rises by 0.53 percentage points of lagged assets (Column 5). Column (6) presents the central test. The sum of the reinsurance-sensitive charges changes by -0.39 percentage points of lagged assets, with a standard error of 0.81 percentage points. We cannot reject that the two channels offset exactly. In terms of point estimates, the increase in the premium-risk charge alone offsets about 93% of the decline in the credit charge. Consistent with this offset, required capital does not respond: the coefficient on actual ACL is -0.07 percentage points of lagged assets and statistically indistinguishable from zero (Column 7). Columns (8) and (9) report the counterfactual ACL. Holding reinsurance intensity at its pre-reform level, the counterfactual ACL of $HighBEP$ groups changes by -0.28 percentage points of lagged assets ($t = -0.47$), and the implied credit release is -0.21 percentage points ($t = -0.58$). Neither estimate is statistically significant. We therefore find no evidence that the lower credit charge released a measurable amount of required capital.

Two features of the RBC formula make required capital insensitive to the repatriation of risk. First, the charges that react to a change in reinsurance move in opposite directions. The credit charge is levied on reinsurance recoverables and protects the cedent against reinsurer default. The underwriting charges move the other way. When a group cedes less, it keeps more premiums and, over time, more reserves on its own books. Therefore, the decline in the credit charge is almost fully offset by the increase in the premium-risk charge. This offset might look economically sound. Lower recoverables reduce the insurer's exposure to reinsurer default, so some reduction in the credit charge is warranted even if counterparty quality is unchanged. The formula, however, cannot tell how large this reduction should be. Under the pre-2018 rule, the recoverables charge was a flat 10%, irrespective of the reinsurer's rating, so the capital released is proportional to the volume of recoverables shed rather than to the credit quality of the counterparties. The formula thus nets a volume-based reduction in the credit charge against the additional underwriting risk the group

Table 17: RBC Charges and Required Capital

This table analyzes how the BEAT-induced change in reinsurance affects the capital charges and the required capital under the NAIC Risk-Based Capital (RBC) formula. Except for Column (2), all dependent variables are dollar amounts divided by lagged total assets. Column (1) reports the credit charge on reinsurance recoverables. In all years, we compute it with the pre-2018 rule as 10% of recoverables from unaffiliated and foreign affiliated reinsurers; it combines the half of the charge booked in R3 and the half booked in R4. Column (2) reports overdue recoverables divided by total recoverables, a proxy for collectibility problems. Columns (3)–(5) report the underwriting channel: the reserve-risk charge excluding the recoverables component (Column 3), the premium-risk charge R5 (Column 4), and their sum (Column 5). Column (6) reports the sum of the credit and underwriting charges and tests whether the two channels offset each other. Column (7) reports the Authorized Control Level (ACL), which represents the minimum capital insurance groups are required to hold, recomputed from the risk buckets. Column (8) reports a counterfactual ACL that uses actual reserves, premiums and asset charges but holds the ratio of recoverables to gross premiums written at its 2013–2017 level. Column (9) reports the credit release, defined as the counterfactual minus the actual ACL. For each year, we compute R0, R1, R2, the reserve-risk charge and R5 using that year's filing and the risk factors in force that year. The sums in Columns (5), (6) and (9) are constructed from the winsorized components, so that the identities in the table hold exactly. $LowBEP$ and $HighBEP$ are indicators based on a median split of the 2017 base erosion percentage among groups that met both statutory BEAT thresholds in 2017: average annual gross receipts of at least $500 million and a base erosion percentage of at least 3%. $Post$ equals one for 2018–2022. Control variables are $Leverage$, $LOB\ Concentration$, $CatRisk$, and $Return\ on\ Equity$; we omit $Size$ because the logarithm of total assets is mechanically related to the denominator of the outcomes. Variable definitions are provided in Table A3.

	Credit Channel		**Underwriting Channel**			**Offset**	**Authorized Control Level**		
	Credit charge / *assets*	Overdue recoverables / *recoverables*	Reserve charge / *assets*	R5 / *assets*	Underwriting charge / *assets*	Credit + underwriting / *assets*	ACL actual / *assets*	ACL counterfactual / *assets*	Credit release / *assets*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
LowBEP × Post	0.0008	0.0331	−0.0019	0.0002	−0.0018	−0.0010	0.0042	0.0047	0.0005
	(0.0037)	(0.0261)	(0.0054)	(0.0051)	(0.0092)	(0.0090)	(0.0043)	(0.0053)	(0.0029)
HighBEP × Post	−0.0092*	0.0318	−0.0033	0.0086*	0.0053	−0.0039	−0.0007	−0.0028	−0.0021
	(0.0048)	(0.0363)	(0.0058)	(0.0047)	(0.0080)	(0.0081)	(0.0049)	(0.0059)	(0.0036)
Identity					(3)+(4)	(1)+(5)			(8)−(7)
Control variables	yes	yes	yes	yes	yes	yes	yes	yes	yes
Group fixed effects	yes	yes	yes	yes	yes	yes	yes	yes	yes
Time fixed effects	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	405	459	405	405	405	405	405	405	405
Adjusted R^2	0.834	0.625	0.842	0.619	0.746	0.760	0.723	0.671	0.510

Notes: All continuous variables are winsorized at the 5th and 95th percentiles, with cutoffs computed separately for treated and control groups. Standard errors are in parentheses and clustered at the insurance group level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

retains. The RBC formula rewards a group for shedding reinsurance even when that group retains more risk overall.

Second, ACL aggregates the buckets through a square root, which dampens a change in any single bucket before it reaches required capital. Evaluated at the pre-reform bucket composition of $HighBEP$ groups, the decline in the credit charge of 0.92 percentage points of lagged assets reduces ACL by only about 0.24 percentage points.[41] This expected release is smaller than the standard error of our estimate in Column (9) and therefore cannot be detected at the level of required capital. The same dampening applies to the underwriting side, even without the offset. As it holds reinsurance intensity at its pre-reform level, the counterfactual ACL in Column (8) holds the credit channel constant and reflects only the additional risk that $HighBEP$ groups retained. Even so, it does not increase. The underwriting charges rise by only 0.53 percentage points of lagged assets (Column 5), and the square root dampens this increase further.

The magnitudes illustrate the gap between economic and regulatory exposure. $HighBEP$ groups reduced their recoverables by 9.2 percentage points of lagged assets and retained a substantially larger share of their new business, with the net-to-gross ratio of current-year unpaid losses rising by 11.8 percentage points (Table 16). Yet their required capital changed by less than 0.1 percentage points of lagged assets, and it would not have increased measurably even without the credit release.

8. What the BEAT Raised for the Treasury

In the previous sections, we identified how the U.S. property-casualty insurance industry responded to the BEAT. In this section, we want to give a rough estimate of the BEAT revenues for the U.S. Treasury. Specifically, we ask how much revenue the minimum tax raised from the insurance groups in our sample and how much it would have raised had those groups left their reinsurance programs unchanged. When the Joint Committee on Taxation estimated the BEAT revenue across all industries in December 2017, it projected that the provision would raise $149.6 billion in additional

[41] The Authorized Control Level equals half of total risk-based capital after the covariance adjustment, $ACL = 0.5\left(\mathrm{R0} + \sqrt{\sum_{k=1}^{5} \mathrm{R}_k^2}\right)$. As the credit charge C is split equally between R3 and R4, a change ΔC changes each of the two buckets by $0.5\,\Delta C$. To a first-order approximation, the resulting change in ACL is therefore $\Delta ACL \approx 0.25 \times (\mathrm{R3}+\mathrm{R4})/\sqrt{\sum_{k=1}^{5} \mathrm{R}_k^2} \times \Delta C$. For *HighBEP* groups before 2018, the median of $(\mathrm{R3}+\mathrm{R4})/\sqrt{\sum_{k=1}^{5} \mathrm{R}_k^2}$ is 1.04, so a decline in the credit charge of 0.92 percentage points of lagged assets reduces ACL by about $0.25 \times 1.04 \times 0.92 \approx 0.24$ percentage points.

corporate tax revenue over 2018–2027, and $51.7 billion over its first five years (Joint Committee on Taxation, 2018). Realized revenues fell far short. In 2018, 479 corporations paid a combined base erosion minimum tax of $1.8 billion across all sectors, of which $305 million came from the finance and insurance sector (Internal Revenue Service, 2021). Those figures show what the provision collected, not what it raised. When firms respond to a minimum tax by restructuring their internal capital markets, the revenue must account for the income they bring back into the U.S. base. Measuring that component, however, requires a counterfactual, which official statistics cannot supply. The framework in this paper does. We therefore add two quantities to the revenue figure: the BEAT liability that would have arisen absent any response and the total revenue including the tax generated on the onshored base.

Section 2 sets out how to calculate an insurance group's base erosion minimum tax amount ($BEMTA$). For group i in year $t \geq 2018$,

$$\text{BEMTA}_{i,t} = \max\left\{0,\ b_t\left(R_{i,t} + B_{i,t}\right) - \tau_{i,t} R_{i,t}\right\}, \tag{14}$$

where $B_{i,t}$ denotes base erosion payments, $R_{i,t}$ income before taxes, b_t the statutory BEAT rate, and $\tau_{i,t}$ the group's effective tax rate. The rate b_t is five percent in 2018 and ten percent from 2019 onward. The maximum operator matters since revenue accrues only from groups whose ratio of base erosion payments to income, $\kappa_{i,t} \equiv B_{i,t}/R_{i,t}$, exceeds the binding threshold $\kappa_{B,t} = (\tau_t - b_t)/b_t$ derived in Equation (16).

We calculate and compare two figures: the actual BEAT revenue and a counterfactual one, which accounts for the response of insurance groups to the BEAT. To calculate the actual BEAT revenue based on our data, we calculate Equation (14) for each insurance group in each post-treatment year and then sum across groups. The counterfactual revenue applies the same equation to the base erosion payments groups would have made had they not restructured. Constructing the counterfactual revenue requires an assumption about what those groups would have done. We assume that, absent the reform, each treated group would have maintained its 2017 ratio of foreign affiliate cessions to gross premiums written, and we apply that fixed ratio to its realized gross premiums written in each year from 2018 through 2022. The counterfactual base therefore grows with the group's own underwriting rather than being fixed at 2017 dollar levels. Section 6 supports

holding the ratio rather than the level: Tables 9 and 10 show no change in portfolio composition or underwriting volume, so the premium base itself is likely to be unaffected by the tax.

Calculating the actual BEAT liability for each post-treatment year would understate its total fiscal revenue. The reason is that affected insurance groups did not simply stop making base erosion payments. They retained more premium in the U.S. and shifted cessions from non-electing foreign affiliates toward electing ones. In both cases the displaced premium re-entered the U.S. tax base through the regular corporate tax. The total revenue effect is therefore the sum of the BEAT actually paid and the tax generated on the onshored base. To measure the onshored volume we re-estimate Equation (4), for each counterparty channel. Our specification includes group and year fixed effects as well as the interaction term. It is worth noting that we weight observations by gross premiums written, which is the reason we do not reuse the coefficients reported earlier in the paper. An unweighted coefficient estimate of interaction term $Treated^{\text{stat}} \times Post$ recovers the average response across groups, giving a group writing two hundred million dollars of premium the same influence as one writing forty billion. Multiplying such a coefficient by aggregate premium volume would distort the results since the dollar base is concentrated in very large groups. Weighting each observation by gross premiums written makes $\hat{\beta}$ the premium weighted average of the group level responses, and the product of that coefficient with aggregate treated premium recovers the aggregate dollar response directly.

Converting volume into revenue requires a margin. For premiums retained domestically we apply the treated group median pre-tax margin on retained premium, $m = 12.7$ percent. For premium redirected to §953(d) electing affiliates we apply the affiliate margin, $\mu = 26.1$ percent. Both numbers represent pre-treatment medians and have been introduced in Section 2. For simplicity, we assume a tax rate of 21%, which is the statutory tax rate following the TCJA. The remaining channels generate no offsetting revenue. Cessions to non-affiliates remain fully deductible and fall outside the BEAT base.

Our estimates rest on four assumptions. First, absent the reform each treated group would have maintained its 2017 ratio of foreign affiliate cessions to gross premiums written, which we apply to realized premium volume in each year from 2018 through 2022. Second, we do not observe the tax a group would have paid in the absence of the BEAT and rely on its effective tax rate, the ratio of federal and foreign income taxes to income before taxes, as an approximation. Group

years for which this ratio is below zero or above one are treated as missing and contribute zero to both the realized and the hypothetical figure. Third, income before taxes is floored at zero, so a group reporting a loss contributes no regular tax liability against which the minimum tax is compared. Fourth, foreign affiliate cessions reported as negative are set to zero. Taken together, these assumptions mean that our estimates should be read as an order of magnitude rather than as a point estimate.

Our tax revenue estimates appear in Table 18. The two panels differ in how base erosion payments are defined, which follows from our classification of §953(d) and non-electing entities. Panel A treats all cessions to foreign affiliates other than those we identify as flowing to electing entities as base erosion payments. Panel B restricts base erosion payments to cessions we verify as flowing to non-electing affiliates. We focus on Panel A and treat Panel B as a robustness test. The conclusions remain the same across the two. We find that if treated groups had maintained their 2017 cession ratios, the BEAT would have raised $12.1 billion from them over 2018 through 2022. Realized liability was $1.5 billion, which is 12.4 percent of the counterfactual figure. The pattern is consistent with the aggregate statistics reported above and with the behavioral responses documented earlier in the paper. By reducing cessions to non-electing foreign affiliates, treated groups reduced their base erosion payments and with them their exposure to the minimum tax.

The directly paid liability is only part of the fiscal story. Premium displaced from non-electing foreign affiliates did not leave the tax base. We estimate that the onshored base generated $4.2 billion in additional federal tax over the same period, nearly three times the BEAT actually collected. Domestic retention accounts for $2.5 billion of that total and the shift toward §953(d) electing affiliates for the remaining $1.7 billion. That is, premium brought back onto domestic balance sheets is the larger of the two channels. The election channel is nonetheless substantial. Taken together, the BEAT and the responses it induced generated $5.7 billion from treated groups, against $12.1 billion under fixed behavior. Measured against the hypothetical benchmark, the share of revenue actually realized rises from 12.4 percent when only the explicit minimum tax is counted to 47.0 percent once the onshored base is included. Assessing a minimum tax by its direct revenues alone therefore understates its fiscal effect, and in this setting by nearly a factor of four.[42]

[42]Panel B confirms the pattern under the narrower definition. The counterfactual figure falls to $10.4 billion and realized liability to $0.7 billion, so the directly collected share is lower at 6.9 percent, while the share including the onshored base is essentially unchanged at 47.2 percent. The narrower definition mechanically reduces both the

Table 18: Treasury Revenue Effects of the BEAT

This table compares the federal tax revenue raised from treated insurance groups ($Treated^{\text{stat}} = 1$) with a counterfactual in which these groups had not restructured their reinsurance programs. All figures are in USD billions. *Counterfactual* applies each group's 2017 ratio of base erosion payments to gross premiums written to its realized gross premiums written and computes the resulting base erosion minimum tax amount. *BEMTA* applies the same calculation to realized base erosion payments. In both, the BEAT rate is 5% in 2018 and 10% thereafter, and the regular tax against which the minimum tax is compared equals the group's effective tax rate, defined as federal and foreign income taxes divided by income before taxes, times its realized income before taxes, floored at zero. *§953(d)* and *Domestic* report the federal tax generated on premiums redirected to electing affiliates and on premiums retained domestically, respectively. These volumes are obtained from channel-level difference-in-differences estimates, weighted by gross premiums written and applied to the aggregate gross premiums written of treated groups. The volumes are converted into pre-tax income using profit margins of 26.1% for electing affiliates and 12.7% for retained premiums, both introduced in Section 2, and this income is taxed at the statutory federal rate of 21%. *Total* is the sum of BEMTA and the two onshored components, and *Share of counterfactual* is Total divided by Counterfactual. Panel A defines base erosion payments as all cessions to foreign affiliates except those to affiliates we identify as §953(d)-electing, which provides an upper bound on the base. Panel B restricts them to cessions to verified non-electing affiliates in Bermuda, the Cayman Islands, and Switzerland, which provides a lower bound. The onshored components are identical in both panels.

Panel A: Base erosion payments as all foreign affiliate cessions except electing affiliates

			Onshored income			
Year	Counterfactual	BEMTA	§953(d)	Domestic	Total	Share of counterfactual
2018	0.99	0.19	0.28	0.43	0.90	91.3%
2019	2.78	0.36	0.30	0.45	1.11	40.2%
2020	1.64	0.29	0.32	0.47	1.09	66.3%
2021	3.21	0.26	0.36	0.54	1.17	36.5%
2022	3.49	0.40	0.41	0.61	1.42	40.5%
Total	12.10	1.51	1.67	2.51	5.69	47.0%

Panel B: Base erosion payments as verified non-electing affiliates

			Onshored income			
Year	Counterfactual	BEMTA	§953(d)	Domestic	Total	Share of counterfactual
2018	0.82	0.10	0.28	0.43	0.81	98.0%
2019	2.37	0.26	0.30	0.45	1.01	42.7%
2020	1.29	0.08	0.32	0.47	0.87	67.6%
2021	2.83	0.08	0.36	0.54	0.99	34.9%
2022	3.05	0.20	0.41	0.61	1.22	39.9%
Total	10.36	0.72	1.67	2.51	4.89	47.2%

9. Conclusion

Internal capital markets let firms move resources to where they are most valuable. In multinational insurance groups, these markets run through internal reinsurance. But a cross-border reinsurance contract does two things at once: it pools risk and shifts profit to low-tax jurisdictions. The two motives are bundled in the same transaction, so they are difficult to tell apart. In this paper, we use the 2017 Base Erosion and Anti-Abuse Tax (BEAT) to separate them. The BEAT raised the tax cost of ceding reinsurance to foreign affiliates. Therefore, it acts as an exogenous shock to the cost of internal capital. To identify groups exposed to the tax, we classify them by the statutory thresholds as they were in 2017, so treatment is fixed before the reform. We then exploit the §953(d) election, which lets a foreign affiliate keep its offshore operations but pay full U.S. tax. This election separates the tax motive from the operational motive, allowing us to measure the relative strength of these motives among the flows disrupted by the tax.

We find that treated groups sharply cut reinsurance to their foreign affiliates after the reform. The drop was about 7 percentage points of gross premiums written. It happened at once in 2018 and did not reverse. The cut was largest for the firms with the highest exposure to BEAT, that is, those most dependent on foreign affiliate reinsurance markets. However, the affected groups replaced only part of the lost capacity. Reinsurance cessions to foreign unaffiliated reinsurers rose modestly, leaving a substantial substitution gap. This substitution gap is a first sign that demand for these internal flows was highly tax-sensitive; without the tax advantage, firms found it more cost-effective to retain the risk domestically rather than pay the margin required for external replacement.

The §953(d) election lets us put a number on this. In a two-stage least squares design that instruments the decline in non-electing cessions with BEAT exposure, we estimate a policy-induced substitution ratio. For each percentage point of BEAT-driven decline in cessions to non-electing affiliates, about 0.36 percentage points reappeared through a fully taxed §953(d) affiliate. Roughly a third of the flows the reform drove out of the non-electing channel were thus preserved at the cost of full U.S. taxation. These flows capture enough non-tax operational value to justify that cost. The remaining two-thirds were not redirected to an electing affiliate. The ratio we estimate is a marginal ratio, defined on the flows the policy moved rather than a decomposition of the entire

counterfactual and the realized base without altering the substitution volumes, which are estimated from the same channel regressions in both panels.

pre-reform stock. What the ratio establishes is that the viability of most of these flows depended on the tax differential. The surviving, sticky part of the market was concentrated in firms with high catastrophe exposure, which have the greatest need for global diversification, and preserved their offshore capacity, either by paying the U.S. tax to keep the internal link or by migrating to arm's-length markets.

The flows that disappeared did not take their risk with them. As treated groups continued writing the same business, the risk they no longer ceded stayed on their domestic balance sheets. Net retention rose, and this increase came primarily from long-tail lines rather than catastrophe lines, consistent with catastrophe risk requiring global diversification that a domestic book cannot replicate. The same pattern appears in the losses groups keep on their books. For the current accident year, the share of unpaid losses retained net of reinsurance rose, while losses from earlier accident years, still covered by pre-BEAT contracts, did not change. Required regulatory capital, however, did not respond. Two features of the RBC formula explain this. First, as groups ceded less, their reinsurance recoverables fell, and the credit charge on those recoverables fell with them. This decline almost fully offset the higher premium-risk charge from the retained business. As the credit charge was a flat factor on the volume of recoverables rather than a function of the reinsurers' credit quality, the formula registered the reduction in ceded volume but not the additional underwriting risk the groups retained. Second, the square-root aggregation of the risk charges dampens the change in any single charge, so that even without the lower credit charge, required capital would not have increased measurably.

Our findings have three main implications, most of which extend beyond the insurance industry. First, they speak to the nature of internal capital markets. A large share of what looked like internal risk sharing was, in fact, tax arbitrage. Tax-motivated flows are highly elastic: they collapse when the subsidy is removed. Risk-motivated flows are sticky and firms pay to keep them. This distinction is important for any work that treats internal flows as a single object. Second, they speak to tax policy. The BEAT did what it was designed to do. It curbed cross-border profit shifting and most of the affected flows were not simply replaced in external markets. But the policy also worked outside the tax system. By making offshore risk pooling more expensive, it pushed genuine insurance risk back onto U.S. balance sheets. Thus, a tax aimed at base erosion changed the way risk is held in the financial system. Third, they speak to financial regulation. The risk-based capital framework did not

register the additional risk that the most exposed groups retained. These groups kept a larger share of their new business on their own balance sheets, yet their required capital did not change. This measurement gap arises because the lower charge on reinsurance recoverables offsets the higher charge on retained risk, and because the formula responds only weakly to the retained risk itself. The result points to an interaction between tax policy and prudential regulation that neither side fully accounts for.

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A. Derivation of the Election Threshold

This appendix derives Equations (2) and (3) of Section 2 and states the conditions under which the comparison is well defined. Notation follows the main text: P is gross premiums written, a and u the shares ceded to foreign affiliates and unaffiliated reinsurers, $N \equiv (1-a-u)P$ the premium retained inside the U.S., m the pre-tax margin on that premium, τ the effective U.S. tax rate, b the BEAT rate, t_f the tax rate in the affiliate's jurisdiction, and μ_u and μ_i the affiliate's underwriting and investment margins per dollar of assumed premium. As both components of the affiliate's income are taxed at t_f absent an election and at τ under one, we write $\mu \equiv \mu_u + \mu_i$ throughout.

Following the BEAT, an insurance group pays the following tax liability on its worldwide income, which consists of a U.S. and a foreign business:

$$T^{\text{BEAT}} = \max\left\{\tau\, mN,\ b\,(mN + aP)\right\} + t_f\,\mu\, aP. \tag{15}$$

The insurance group owes the BEAT when $b\,(mN+aP) > \tau\, mN$, that is, when $b\,aP > (\tau - b)\, mN$, or

$$\kappa \ \equiv\ \frac{aP}{mN} \ >\ \kappa_B \ \equiv\ \frac{\tau - b}{b}. \tag{16}$$

At the parameters used in the paper, $\kappa_B = 0.070/0.100 = 0.70$, while the median treated group has $\kappa = 2.65$. Therefore, the minimum tax binds comfortably. In the following, we condition on this binding case. If the minimum tax does not bind, then $T^{\text{BEAT}} = \tau\, mN + t_f\,\mu\, aP$, and since $T^{953} = \tau\, mN + \tau\,\mu\, aP$, we have $T^{953} \geq T^{\text{BEAT}}$ for any $t_f \leq \tau$. The election is then never optimal, and the trade-off we describe does not arise. Equation (1) of the paper therefore states T^{BEAT} based on the group being subject to the BEAT.

Conditional on the minimum tax binding, the insurance group compares

$$T^{\text{BEAT}} = b\,(mN + aP) + t_f\,\mu\, aP \qquad \text{and} \qquad T^{953} = \tau\,(mN + \mu\, aP), \tag{17}$$

and elects when $T^{953} < T^{\text{BEAT}}$:

$$\tau\, mN + \tau\,\mu\, aP \ <\ b\, mN + b\, aP + t_f\,\mu\, aP. \tag{18}$$

Subtracting $b\,mN$ and $\tau\,\mu\,aP$ from both sides and factoring gives

$$(\tau - b)\,mN \;<\; aP\,\big[\,b - (\tau - t_f)\,\mu\,\big], \tag{19}$$

which is Equation (2) in the paper. The left-hand side is the cost of electing as the group forfeits the lower BEAT rate on domestic income, costing $\tau - b$ per dollar of mN. The right-hand side is the benefit of electing. Per dollar of premium ceded, the BEAT taxes the full dollar at b, whereas the election taxes only the margin μ at the incremental rate $\tau - t_f$. Dividing both sides of (19) by $mN\,[\,b - (\tau - t_f)\,\mu\,]$ yields

$$\kappa \;\equiv\; \frac{aP}{mN} \;>\; \kappa^* \;\equiv\; \frac{\tau - b}{b - (\tau - t_f)\,\mu}, \tag{20}$$

which is Equation (3) in the paper. This relationship does only hold if the bracket is positive, which requires

$$b > (\tau - t_f)\,\mu \qquad \Longleftrightarrow \qquad \mu < \frac{b}{\tau - t_f}. \tag{21}$$

Economically, the election can dominate only if the BEAT costs more per dollar ceded than taxing the affiliate's margin on that dollar. At the medians the bound is $0.100/0.170 = 59\%$ of assumed premium, against an observed total margin of $\mu = 26.1\%$, so the condition holds. Were it to fail, the base saving would be negative and the election would never be cheaper, whatever the value of κ.

Three further properties follow directly from (20). First, $\partial\kappa^*/\partial\mu > 0$: a more profitable affiliate is more expensive to bring into the U.S. tax base, which raises the cession share at which electing becomes worthwhile. Second, $\partial\kappa^*/\partial t_f < 0$: an affiliate already taxed abroad is cheaper to bring in, since the election only collects the difference $\tau - t_f$. Third, because $(\tau - t_f)\mu > 0$, the denominator of κ^* is strictly smaller than b, so $\kappa^* > \kappa_B$. Any group for which electing dominates is therefore already in the region where the minimum tax binds, and the two conditions never conflict.

We can write an insurance group's decision to elect in terms of its reinsurance cession to its foreign affiliate. Setting $\kappa = \kappa^*$ and solving for a gives $a\,(1 + \kappa^* m) = \kappa^* m\,(1 - u)$, so the critical cession share is

$$a^* = \frac{\kappa^*\,m\,(1 - u)}{1 + \kappa^*\,m}. \tag{22}$$

Since κ is strictly increasing in a on $a \in [0, 1 - u)$, electing dominates for every $a > a^*$. At

treatment-group medians, $a^* = 12.2\%$ of gross premiums when the affiliate's investment income is included in μ and 9.7% when it is not, against an observed median cession share of 22.3%.

Table A1: Identification of BEAT Treatment Status

This table illustrates the step-by-step determination of treatment assignment based on the 2017 statutory thresholds defined in the Tax Cuts and Jobs Act (TCJA). Using the Zurich Insurance Group (NAIC group 212) as a representative case, the table quantifies the two requirements for treatment: (i) average annual gross receipts of at least $500 million for the three-tax-year period ending with the preceding tax year, and (ii) a base erosion percentage of at least 3%. Panel A calculates the gross receipts for a single fiscal year (2016). Panel B computes the rolling three-year average of gross receipts as of 2017. Panel C derives the base erosion percentage, defined as the ratio of base erosion tax benefits (reinsurance cessions to foreign affiliates) to total allowable deductions. In this example, Zurich's average gross receipts exceed $500 million and its base erosion percentage of 24.4% exceeds the 3% threshold; the group is thus classified as treated. All numbers are in USD thousand and negative numbers are placed in parentheses.

Panel A: Annual Gross Receipts Calculation (2016)

Investment income		
(1) Earned investment income [a]		$ 1,291,431
(2) Total realized capital gains [b]		$ 247,866
Gross investment income	(3) = (1) + max [(2),0]	$ 1,539,298
Premiums written		
(4) Direct premiums written		$ 32,538,791
(5) Premiums assumed from non-affiliates		$ 246,372
(6) Premiums assumed from foreign affiliates		$ 316,256
Total premiums written	(7) = max[(4)+(5)+(6),0]	$ 33,101,419
Other gross income	(8) = max[(8),0]	$ (28,804)
Gross receipts	(3)+(7)+(8)	$ 34,640,717

Panel B: Average Gross Receipts and Statutory Threshold (2017)

Gross receipts		
(1) 2014		$ 32,585,475
(2) 2015		$ 33,350,976
(3) 2016		$ 34,640,717
Average gross receipts in 2017	[(1)+(2)+(3)] / 3	$ 33,525,722

Panel C: Base Erosion Percentage Calculation (2017)

Base erosion payments		
(1) Base erosion payments / Reinsurance ceded to foreign affiliates	(1) = max[(1),0]	$ 6,409,299
(2) Underwriting deductions	(2) = max[(2),0]	$ 19,474,903
(3) Investment income deductions	(3) = max[(3),0]	$ 300,927
(4) Capital gains tax	(4) = max[(4),0]	$ 78,909
(5) Capital loss deductions	(5) = max[-(5),0]	$ 0
Base erosion percentage	(1) / [(1)+(2)+(3)+(4)+(5)]	24.4%

Notes:

[a] Investment income (e.g., dividends) before taxes; excludes capital gains.

[b] Pre-tax realized capital gains and losses, including permanent write-downs but excluding temporary ones.

Table A2: Composition of Treatment and Control Group

This table lists all insurance groups included in the treatment set (Panel A) and the control set (Panel B). Treatment groups ($Treated^{\text{stat}}$) are identified as insurance groups subject to the BEAT statutory thresholds in 2017. All figures are reported for the 2017 fiscal year and represent the consolidated values of each group's U.S. property-casualty subsidiaries. The column *Total assets* denotes the book value of the group's total assets (in USD billions). The column *Relative size* represents the group's share of total assets within its respective treatment or control cohort. Groups are ordered alphabetically.

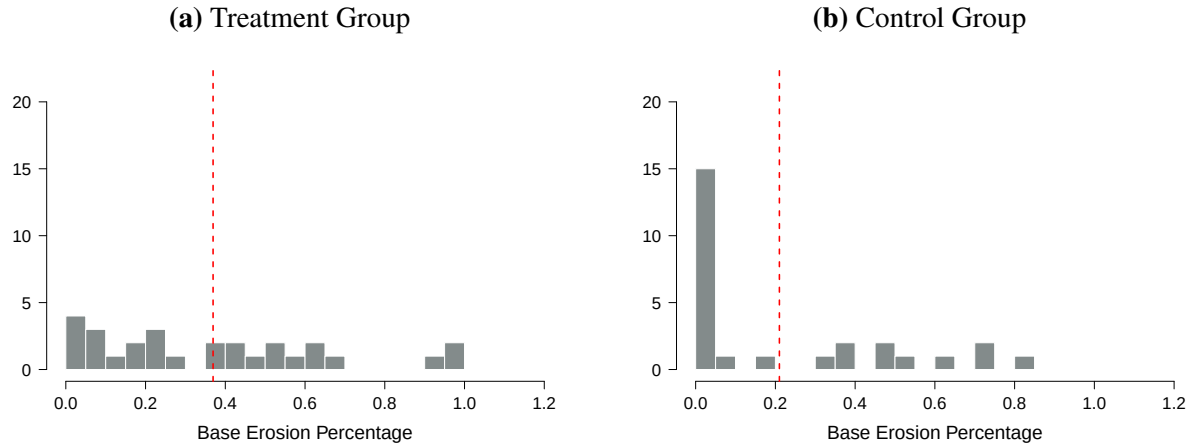
Panel A: Treatment Group

Insurance Group	NAIC group number	Total assets (USD billion)	Relative size (%)
AXIS	3416	5.01	1.14
Allianz	761	13.15	2.99
AmTrust Financial	2538	14.53	3.30
Arch Capital	1279	10.65	2.42
Argo	457	3.51	0.80
Assured Guaranty	194	9.58	2.18
Chubb	626	66.07	15.02
Everest Re	1120	10.41	2.37
FM Global	65	21.03	4.78
Fairfax Financial	158	21.79	4.95
Farmers Insurance	69	29.99	6.82
Great American Insurance	84	12.16	2.76
IAT Insurance	225	1.83	0.42
Liberty Mutual	111	80.19	18.23
MAPFRE	411	4.06	0.92
MS&AD Insurance Group	4715	1.41	0.32
Markel Corporation	785	10.48	2.38
Motors Insurance Corporation	79	2.15	0.49
Munich Re	361	22.98	5.22
PartnerRe	3483	5.18	1.18
ProAssurance	2698	3.39	0.77
QBE	796	5.59	1.27
SCOR	749	4.42	1.00
Swiss Re	181	20.43	4.64
Tokio Marine	3098	25.72	5.85
Zurich	212	34.22	7.78

Panel B: Control Group

Insurance Group	NAIC group number	Total assets (USD billion)	Relative size (%)
American Business & Mercantile Insurance Mutual	150	12.10	1.70
American International Group	12	80.90	11.37
Amerisure	124	3.09	0.43
Assurant	19	8.44	1.19
Auto Club Insurance Association	55	5.49	0.77
Berkshire Hathaway	31	351.29	49.38
CNA	218	45.26	6.36
Coaction	256	2.28	0.32
Essent	4694	1.86	0.26
GeoVera Holdings	3829	0.29	0.04
Global Indemnity	920	0.86	0.12
ICW	922	2.88	0.40
James River Group	3494	1.15	0.16
Medical Mutual	184	0.62	0.09
National American Insurance	4935	0.21	0.03
New Mexico Mutual Group	1112	0.45	0.06
Plateau Casualty Insurance	629	0.05	0.01
Sompo	3219	6.31	0.89
The Hanover Insurance Group	88	8.83	1.24
Travelers	3548	158.86	22.33
UPMC Health Plan	1324	0.27	0.04
W. R. Berkley Corporation	98	19.95	2.80

Figure A1: Distribution of the Base Erosion Percentage



This figure shows the distribution of the base erosion percentage in 2017. Panel A shows the treatment group, and Panel B shows the control group. The vertical dashed line marks the mean within each group.

Table A3: Variable Definitions

This table defines the variables used in the empirical analysis. All variables are computed at the consolidated group level, using each group's historical composition from that year's Schedule Y. The treatment indicators are defined separately in Table 2.

Reinsurance Foreign Affiliates	Premiums ceded to foreign affiliates / GPW
Reinsurance Domestic Affiliates	Premiums ceded to domestic affiliates / GPW
Reinsurance Foreign Non-Affiliates	Premiums ceded to foreign non-affiliates / GPW
Reinsurance Domestic Non-Affiliates	Premiums ceded to domestic non-affiliates / GPW
953d	Premiums ceded to §953(d)-electing affiliates in Bermuda, the Cayman Islands, and Switzerland / GPW
Non953d	Premiums ceded to non-electing affiliates in Bermuda, the Cayman Islands, and Switzerland / GPW
Intensive Margin	Premiums ceded to non-affiliated reinsurers (foreign, domestic, or total) that the group already used before 2018 / GPW
Extensive Margin	$\log(1 + \text{number of distinct non-affiliated reinsurance counterparties})$, by type (foreign, domestic, or total)
ILS / GPW	Outstanding volume of insurance-linked securities in which the group is the ceding insurer / GPW
Gross Premiums Written (GPW)	Sum of direct premiums written and premiums assumed
Direct Premiums Written (DPW)	Premiums written in the primary market
Net Premiums Written (NPW)	Direct premiums written plus premiums assumed less premiums ceded; computed for all lines and separately for catastrophe-exposed and long-tail lines
Total Premiums Assumed	Premiums assumed from affiliates and non-affiliates
Post	Dummy variable equal to 1 for group-year observations after 2017 and 0 otherwise
Size	Natural logarithm of total assets
Return on Equity	Income after tax / capital and surplus
Leverage	Surplus and capital divided by total assets
CatRisk	DPW in catastrophe-prone lines within catastrophe-prone states / DPW
CatExposure	Premium-weighted average of state catastrophe intensity, standardized. State intensity is the mean annual inflation-adjusted property damage from catastrophe hazards over 1996–2017 (SHELDUS). Weights are the group's 2013–2017 average share of direct premiums written by state, renormalized to sum to one
LOB Concentration	Herfindahl–Hirschman index of DPW shares across lines of business
Portfolio Distance (L1)	Sum of absolute deviations of line-of-business DPW shares from the group's 2017 shares
Portfolio Distance (L2)	Euclidean distance between the line-of-business DPW share vector and the group's 2017 share vector

Table A3: Variable Definitions – Continued

Gross Unpaid Losses	Direct and assumed unpaid losses and loss adjustment expenses (case, bulk and IBNR, defense and cost containment, adjusting and other) from Schedule P, Part 1, divided by lagged total assets; computed for the current accident year and for all accident years
Ceded Unpaid Losses	Ceded unpaid losses and loss adjustment expenses from Schedule P, Part 1, divided by lagged total assets; computed for the current accident year and for all accident years
Net Unpaid Losses	Gross minus ceded unpaid losses and loss adjustment expenses, divided by lagged total assets; computed for the current accident year and for all accident years, and separately for catastrophe-exposed and long-tail lines
ACL / Assets	Authorized control level, adjusted for intra-group holdings, divided by lagged total assets
ACL (Counterfactual)	Recomputed ACL with recoverables set to the group's 2013–2017 ratio of recoverables to GPW times its actual GPW, divided by lagged total assets
Credit Release	Counterfactual ACL minus recomputed ACL, divided by lagged total assets
Credit Charge	10% of recoverables from unaffiliated and foreign affiliated reinsurers (pre-2018 rule applied in all years), equal to R3 plus the recoverables part of R4, divided by lagged total assets
Overdue Recoverables	Overdue reinsurance recoverables divided by total recoverables (Schedule F)
Reserve Charge	Reserve-risk charge R4 excluding the recoverables part, divided by lagged total assets
Underwriting Charge	Sum of the reserve charge and the premium-risk charge R5, divided by lagged total assets

Table A4: Parallel Trends Test

This table presents event-study estimates of the effect of the Base Erosion and Anti-Abuse Tax (BEAT) on reinsurance cession ratios. The specification follows the difference-in-differences framework in Equation (5), with coefficients estimated for each year relative to 2017, which serves as the omitted reference year. $Treated^{\text{stat}}$ equals one if the insurance group met both statutory BEAT thresholds in 2017: average annual gross receipts of at least $500 million and a base erosion percentage of at least 3%. The dependent variable in all columns is the ratio of premiums ceded to gross premiums written (GPW). The columns partition cessions by counterparty type: (1) foreign affiliates, (2) domestic affiliates, and (3) non-affiliates. Control variables are $Size$, $Leverage$, $LOB\ Concentration$, $CatRisk$, and $Return\ on\ Equity$. Variable definitions are provided in Table A3.

	Foreign Affiliates	**Domestic Affiliates**	**Non-Affiliates**
	(1)	(2)	(3)
Treated^{stat} × 2013	−0.047	−0.034	0.011
	(0.036)	(0.068)	(0.025)
Treated^{stat} × 2014	−0.045	−0.072	0.005
	(0.031)	(0.049)	(0.023)
Treated^{stat} × 2015	−0.021	−0.067	0.014
	(0.022)	(0.046)	(0.023)
Treated^{stat} × 2016	−0.024	−0.030	0.014
	(0.018)	(0.032)	(0.018)
Treated^{stat} × 2018	−0.112***	0.003	0.030*
	(0.040)	(0.033)	(0.015)
Treated^{stat} × 2019	−0.122***	−0.011	0.028
	(0.034)	(0.030)	(0.018)
Treated^{stat} × 2020	−0.092***	−0.013	0.015
	(0.034)	(0.035)	(0.018)
Treated^{stat} × 2021	−0.096***	−0.025	0.017
	(0.033)	(0.032)	(0.020)
Treated^{stat} × 2022	−0.083**	−0.026	0.020
	(0.038)	(0.063)	(0.020)
Control variables	yes	yes	yes
Group fixed effects	yes	yes	yes
Time fixed effects	yes	yes	yes
Observations	480	480	480
Adjusted R^2	0.738	0.885	0.877

Notes: All continuous variables are winsorized at the 5th and 95th percentiles, with cutoffs computed separately for treated and control groups. Standard errors are in parentheses and clustered at the insurance group level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A5: Parallel Trends Test After Removing Differential Pre-Trend

This table presents event-study estimates following the difference-in-differences framework in Equation (5), where coefficients are estimated for each year relative to the 2017 baseline. We adjust reinsurance cession ratios by their linear pre-trend. We construct the adjusted outcomes in two steps. First, we estimate the differential linear trend between treated and control groups using only pre-reform data (2013–2016). Second, we subtract this trend, extrapolated over the full sample period, from the outcome of treated groups. We omit the year 2017 as reference year. We base statistical inference on a two-step cluster bootstrap-t procedure with 1,000 replications. In each replication, we resample insurance groups with replacement and re-estimate both steps: the pre-reform trend and the event study on the adjusted outcome. We then compute the studentized statistic for each treatment interaction. The reported p-values compare the original t-statistic with the bootstrap distribution of these studentized statistics. Control variables are $Size$, $Leverage$, $LOB\ Concentration$, $CatRisk$, and $Return\ on\ Equity$. Variable definitions are provided in Table A3.

	Foreign Affiliates	**Domestic Affiliates**	**Non-Affiliates**
	(1)	(2)	(3)
$\text{Treated}^{\text{stat}} \times 2013$	-0.009	-0.055	0.013
	(0.036)	(0.068)	(0.025)
$\text{Treated}^{\text{stat}} \times 2014$	-0.017	-0.088^{*}	0.007
	(0.031)	(0.049)	(0.023)
$\text{Treated}^{\text{stat}} \times 2015$	-0.002	-0.078	0.016
	(0.022)	(0.046)	(0.023)
$\text{Treated}^{\text{stat}} \times 2016$	-0.015	-0.035	0.015
	(0.018)	(0.032)	(0.018)
$\text{Treated}^{\text{stat}} \times 2018$	-0.121^{**}	0.009	0.029
	(0.040)	(0.033)	(0.015)
$\text{Treated}^{\text{stat}} \times 2019$	-0.141^{***}	-0.0004	0.027
	(0.034)	(0.030)	(0.018)
$\text{Treated}^{\text{stat}} \times 2020$	-0.121^{**}	0.003	0.013
	(0.034)	(0.035)	(0.018)
$\text{Treated}^{\text{stat}} \times 2021$	-0.134^{**}	-0.004	0.014
	(0.033)	(0.032)	(0.020)
$\text{Treated}^{\text{stat}} \times 2022$	-0.130	-0.0001	0.017
	(0.038)	(0.063)	(0.020)
Control variables	yes	yes	yes
Group fixed effects	yes	yes	yes
Time fixed effects	yes	yes	yes
Observations	480	480	480
Adjusted R^2	0.753	0.886	0.877

Notes: All continuous variables are winsorized at the 5th and 95th percentiles, with cutoffs computed separately for treated and control groups. Standard errors are in parentheses. *, **, and *** are based on the bootstrap-t p-values and indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A6: Placebo Test

This table presents a placebo test by artificially shifting the treatment timing to a pre-reform period. The sample is restricted to the years 2013–2016, with 2016 assigned as a placebo treatment year. We estimate an event-study specification similar to Equation (5), where the dependent variable is the ratio of premiums ceded to gross premiums written (GPW). Columns partition cessions by counterparty type: (1) foreign affiliates, (2) domestic affiliates, and (3) non-affiliates. $Treated^{\text{stat}}$ is an indicator variable equal to one if the insurance group met the statutory BEAT thresholds in 2017. These include average annual gross receipts of at least $500 million and a base erosion percentage of at least 3%. The absence of systematic significance across all counterparty types in this table supports the validity of our primary results. Control variables are $Size$, $Leverage$, $LOB\ Concentration$, $CatRisk$, and $Return\ on\ Equity$. Variable definitions are provided in Table A3.

	Foreign Affiliates	**Domestic Affiliates**	**Non-Affiliates**
	(1)	(2)	(3)
Treated^{stat} $\times$ 2016	0.015	0.023	0.002
	(0.020)	(0.033)	(0.013)
Control variables	yes	yes	yes
Group fixed effects	yes	yes	yes
Time fixed effects	yes	yes	yes
Observations	192	192	192
Adjusted R^2	0.865	0.935	0.933

Notes: All continuous variables are winsorized at the 5th and 95th percentiles, with cutoffs computed separately for treated and control groups. Standard errors are in parentheses and clustered at the insurance group level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A7: Common Support Regression

This table reports the propensity scores that define the common support samples in Table 5. The dependent variable is an indicator for meeting both statutory thresholds in 2017: the $500 million average gross receipts test and the 3% base erosion percentage test ($Treated^{\text{stat}}$). Covariates are pre-reform (2013–2017) group averages. Column (1) includes $Size$, $Leverage$, $LOB\ Concentration$, and $CatRisk$. Column (2) adds $Reinsurance\ Foreign\ Affiliates$, the ratio of premiums ceded to foreign affiliates to gross premiums written. Estimation is by Firth-penalized maximum likelihood, which corrects the small-sample bias of the logit in a cross-section of this size. The likelihood ratio test compares each model against an intercept-only specification. Variable definitions are provided in Table A3.

	(1) *Excluding cessions*	(2) *Including cessions*
Size	0.339*	0.643***
	(0.206)	(0.260)
LOB Concentration	-0.409	0.847
	(1.852)	(1.984)
CatRisk	8.603	12.851
	(20.593)	(21.713)
Leverage	-3.969	-1.901
	(4.056)	(4.308)
Reinsurance Foreign Affiliates		7.235***
		(2.758)
Constant	-6.175	-15.337***
	(4.748)	(6.329)
Likelihood ratio χ^2	$\chi^2_{(4)} = 7.48$	$\chi^2_{(5)} = 15.93$
p-value	0.113	0.007
Observations	48	48

Notes: All continuous variables are winsorized at the 5th and 95th percentiles, with cutoffs computed separately for treated and control groups. Standard errors are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A8: Geography of Affiliated Reinsurance

This table reports the ten largest destination countries for reinsurance premiums ceded to foreign affiliates by the U.S. property-casualty subsidiaries of the insurance groups in our sample, in 2017 and 2018. A country is assigned according to the domicile of the receiving affiliate. For each country, the table reports the premiums ceded to affiliates domiciled there, summed across all sample groups and expressed in USD thousands, and their share of total premiums ceded to foreign affiliates in that year.

2017			2018		
Country	**Premiums (USD)**	**Share (%)**	**Country**	**Premiums (USD)**	**Share (%)**
Bermuda	23,460,514	52.53	Bermuda	13,729,675	77.48
Switzerland	15,186,501	34.01	Germany	1,159,270	6.54
Germany	1,890,690	4.23	Switzerland	577,500	3.26
Cayman	1,016,629	2.28	Japan	435,360	2.46
France	976,478	2.19	Cayman	420,892	2.38
Japan	444,216	0.99	Turks & Caicos	362,612	2.05
Ireland	378,931	0.85	France	275,342	1.55
Turks & Caicos	364,318	0.82	United Kingdom	220,268	1.24
Malta	265,465	0.59	Spain	170,489	0.96
United Kingdom	235,712	0.53	Canada	131,516	0.74