The Elusive Banker:

Using Hurricanes to uncover (Non-)Activity in Offshore Financial Centers

Jakob Miethe *

April 2025

Abstract: This paper studies financial service provision in offshore financial centers (OFCs). Exploiting the natural experiment of recurring hurricanes hitting small islands, I show that local conditions, captured by satellite data, deteriorate significantly for nine months on average. However, both the international bank sector and international investors do not react in OFCs while non-OFCs show strong negative reactions. Instead of being connected to local activity, OFC service provision declines during holidays in London, Tokyo, and New York. Thus, international regulation attempts requiring local enforcement could be targeted better, financial risks could be mis-assigned, and offshore finance is a doubtful development strategy.

JEL classification: H26, G15, C82

Keywords: Offshore finance, international taxation, nightlights, hurricane impacts

*I thank participants of several seminars and conferences (ASSA, MaTax, IIPF, NTA) where this paper was presented. In particular, I thank Katarzyna Bilicka, Sarah Clifford, Ron Davies, Nadja Dwenger, Marcel Fratzscher, Clemens Fuest, Niels Johannesen, Lukas Menkhoff, Brigitte Roth Tran, Dominik Sachs, Julia Schmieder, Kurt Schmidheiny, Sebastian Siegloch, Eric Strobl, Eddy Tam, and Gregory Veramendi for helpful comments and discussions at various stages of this project. All remaining errors are my own. Contact: LMU Munich, Center for Economic Studies, Schackstr. 4, 80539 Munich, email: jakob.miethe@econ.lmu.de

1. Introduction

On paper, small Offshore Financial Centers (OFCs) are home to more than a quarter of all international bank claims and at least 40% of foreign direct investment, while combining only 1.8% of the world's population.[1] A growing literature documents profit shifting, tax evasion, and other illicit financial flows as potential sources of these funds. However, the last decade has seen substantial changes in the regulation of such practices. The automatic exchange of bank information has made it much more costly to illegally evade taxes using OFC bank accounts. The recently introduced Global Minimum Tax limits the return to profit shifting by firms. Still, international financial positions in OFCs continue to increase. What makes them so successful?

Academic attention has focused on the outsized positions created through profits shifted or wealth moved to OFCs, both of which are generated elsewhwere (Alstadsæter et al., 2019; Tørsløv et al., 2023; Zucman, 2013). This is different for the offshore financial service sector that intermediates these funds by providing wealth management services, managing multi-level company structures, or reacting to policy measures. Very little is known about it and this paper tries to contribute to filling that gap. OFC financial institutions themselves argue to be providing skilled human capital and innovative legal and financial services that successfully attract international capital.[2] Indeed, OFCs are economically open, provide sophisticated communications infrastructures, and perform well on governance indicators (Dharmapala, 2008; Dharmapala and Hines, 2009). Companies, fund managers, and official government aid institutions argue that OFCs play a vital role in their international investment strategies by providing a set of unique skills and services.[3]

But are financial intermediation services actually carried out in the OFC where they are booked? Can they therefore be connected to a comparative advantage of OFCs to provide them? If not, where do they take place? And what are the implications of potentially elusive bankers for offshore finance as a development strategy?

[1] Bank claims data is introduced in the main text; see Damgaard et al. (2024) for the FDI statistic; see the CIA world fact book for population data.

[2] This is publicly advertised on websites of financial service providers, two examples: the Jersey Trust Company in the British Virgin Islands advertises a "reliable, flexible and professional service. With continual investment in specialist knowledge and innovation, we offer truly bespoke solutions.", see https://www.jtcgroup.com/offices/bvi/. Eltoma highlights about the British Virgin Islands that "The island is committed to retaining an investor's right to privacy, while providing a professional banking sector with top quality legal, accounting and trust and management services.", see https://www.eltoma-global.com/jurisdictions/bvi-british-virgin-islands. Both last accessed the 13th of March, 2025.

[3] The German official development aid, for example, explains its participation in investment funds on Mauritius along these lines. These funds are recorded here: https://www.deginvest.de/International-financing/DEG/Download-Center/Jahresberichte/, last accessed the 13th of March, 2025.

In this paper, I test if international financial services booked on small island OFCs can be traced back to local activity. I exploit natural disasters in the form of hurricanes hitting island economies and compare local impacts, measured using satellite data, to impacts in financial service provision. In both cases reactions of OFCs are compared to a sample of non-OFC islands to potentially falsify the identification strategy. After impact, local conditions deteriorate substantially in both samples but the financial service sector on OFCs is entirely unaffected. Both bank and equity price data do show significant deteriorations on non-OFCs, in step with local conditions. These results suggest that the financial service activity booked in OFCs is not, in fact, local. Indicative evidence based on leaked incorporation data instead links it to activity in Tokyo, London, and New York. Finally, while a strong correlation between local development and financial service activity is readily observable in non-OFCs, OFCs show no such connection.

The physical location of financial services matters for at least three reasons. First, recent policy initiatives are dependent on at least some (threat of) local enforcement. International information exchange in tax matters has led to reactions in OFC bank deposits but results are mixed.[4] Even a fully compliant OFC needs local access to be able to collect and provide information from its financial service industry to forward to its partner countries.[5] The Global Minimum Tax relies primarily on the reports of multi-national enterprises themselves but also here, misreporting is possible and would have to be addressed locally. Audits, which have proven successful in reducing both domestic (Boning et al., 2025) and international (Guyton et al., 2023) tax evasion, will be difficult if the activity concerned is not local. Such design flaws can further undermine the already low confidence in governments to tackle tax noncompliance (Stantcheva, 2021) and lead to unintended consequences when regulating tax havens (Dharmapala et al., 2011; Garrett and Suárez Serrato, 2019; Suárez Serrato, 2018). The second policy implication concerns

[4]The literature on reactions to international transparency measures is increasing quickly and reports strong reactions (Boas et al., 2024; Casi et al., 2020; Hanlon et al., 2015; Heckemeyer and Hemmerich, 2020; Johannesen, 2014; Johannesen et al., 2020, 2024; Langenmayr, 2017; Leenders et al., 2023) but also further evasive activity (Alstadsæter et al., 2024; Bomare and Le Guern Herry, 2022; De Simone et al., 2020; Johannesen and Zucman, 2014; Menkhoff and Miethe, 2019). Similar reactions have been documented for corruption in developing economies (Andersen et al., 2017, 2022; Langenmayr and Zyska, 2023).

[5]For the OECD's Common Reporting Standard, a country to country peer review is then in place to ensure implementation quality which requires local enforcement. The latest iteration of this peer review is published by the OECD here: https://www.oecd.org/en/publications/peer-review-of-the-automatic-exchange-of-financial-account-information-2023-update_5c9f58ae-en.html, last accessed the 13th of March, 2025. In the US FATCA system, most countries have opted for the agreement where they collect data locally and then send it to U.S. tax authorities. This is the 'Model 1 IGA' as opposed to the 'Model 2 IGA' and a list of countries with their respective model is maintained here: https://home.treasury.gov/policy-issues/tax-policy/foreign-account-tax-compliance-act, last accessed the 13th of March, 2025. At the time of writing, 100 of the 113 signatory countries use Model 1, requiring local enforcement.

financial stability. If positions that create risk at home are assigned to the OFC where they are booked, lenders of last resort will undervalue the risk within their jurisdiction.[6] A correlation between bank risk, OFC presence, and regulatory arbitrage opportunities is documented in Ge et al. (2022). The third policy implication, following from the disconnect of local development and financial service activity, casts doubt on offshore finance as a valid development strategy. The British Virgin Islands applied for UN emergency food relief after hurricanes Irma and Maria in 2017. The 373,917 companies and 1,499 mutual funds registered there could not provide sufficient resources for its 35,015 inhabitants following a shock.[7]

Studying the financial service sector on OFCs faces two non-trivial problems. First, researchers have to identify reactions of activities that are often shrouded in secrecy or at least not officially recorded. Second, data for OFCs, especially small island economies, is either unavailable, unreliable, or inflated by the financial service sector itself. I introduce an identification strategy and a number of datasets to alleviate these problems.

The effects of international policy measures on OFCs are difficult to identify because such policy measures are usually implemented as reactions to increasing capital positions.[8] Hurricane impacts do not suffer from such policy endogeneity. About half of all offshore capital is booked through island economies such as the Cayman Islands, the British Virgin Islands, or Mauritius. Based on sample choices outlined in detail below, 56 inhabited small island economies, 27 of which are OFCs, are located in the 'hurricane alley' of the Caribbean and the Pacific or Indian Oceans where cyclones hit. These disaster-type hurricanes lead to extended power outages, disabled infrastructure, evacuations, flooding, and direct casualties on affected islands and should therefore also affect the working of local financial service providers.[9]

[6] Progress in this area has been substantial in recent years. Three notable directions: The BIS consolidated banking statistics is trying to address this by creating a dataset documenting where the ultimate risk of all bank positions lie (Hardy et al., 2024). The global capital allocation project is a comprehensive effort to do the same for equity (Beck et al., 2024; Coppola et al., 2021). Damgaard et al. (2024) correct FDI positions for tax haven activity. Still, the problem is thorny as most commonly used international financial statistics are based on the residence principle. Zucman (2013) shows that problems created through such discrepancies can overturn conventional wisdom on debtor-creditor relations in international capital flows.

[7] Data taken from the BVI statistical bulletin 2020Q1, available here, last accessed 11th of March 2025: https://www.bvifsc.vg/library/publications/q1-2020-bvi-fsc-statistical-bulletin. Population data is taken from the CIA world factbook.

[8] Identification using leaks do not suffer from such concerns as leaks are unexpected events, see for example Londoño-Vélez and Ávila-Mahecha (2021); Omartian (2017). An important contribution in understanding OFCs is provided by Garcia-Bernardo et al. (2017) who classify OFCs into sinks versus conduit countries. In both cases, however, local activity is possible but not necessarily required.

[9] Hurricanes, typhoons, and cyclones are subsumed under 'hurricanes' here. Following convention, OFCs are defined as jurisdictions with high secrecy regulations and low to zero tax rates for foreigners. The list is the union of the tax haven lists of Johannesen and Zucman (2014) and Gravelle (2015). Annex A.2 provides robustness tests using other lists. Robustness tests show that any single country can be

The remaining problem with this identification strategy is that data on small islands is scant. Here, progress is possible using a number of new data sources and existing ones in new ways. First, I construct a monthly nightlight dataset with global coverage based on satellite images. These data are constructed both for entire jurisdictions in the sample and their sub-national regions. Nightlight proxies for physical conditions on the island in question and is used as an impact measure in the main results. On average, a hurricane leads to decreases in nightlight intensity of close to 21% (0.19 log points) for the first nine months after impact both on OFCs and non-OFCs. Recovery takes six months to one year. Both the impact and the recovery effects are in line with the literature on natural disasters (Mohan and Strobl, 2017; Strobl, 2011, 2012).

As a first measure of financial service activity, I operationalize the Locational Banking Statistics (LBS) of the Bank for International Settlements (BIS) differently from its usual use in the tax evasion literature. If an American bank lends to a bank, maybe even its own subsidiary, on the Bahamas, it reports this claim to its central bank in quarterly reports which aggregates bilateral macroeconomic time series. Adding these for all BIS reporting countries leads to a total 'mirror claims' series that measures the funding received by the Bahamas from internationally active banks. No data reported from the Bahamas are required. Due to the high leverage ratios of banks and non-bank financial firms, such mirror claims provide a good proxy for financial service activity. When financial operations on the Bahamas decline, so will foreign bank funding. Indeed, in the non-OFC part of the sample, mirror claims are reduced by almost 40% (0.33 log points) after hurricanes hit suggesting decreased financial activity on the island. However, despite the significant impact on the local economy visible in nightlight data there are virtually no effects of hurricanes on financial activity in OFCs. Results are statistically insignificant, coefficients are very close to zero and do not exhibit sign certainty. This evidence is not consistent with a local presence of financial service activity on OFCs of a magnitude that would explain why the worlds top ten countries in per capita mirrorclaims are all OFCs (Figure A.1.5 in Annex A.1.2 provides an overview of such statistics).

I verify that these results are not an artifact of my data source by showing a falsification exercise using the more commonly employed deposit data of the same dataset and carry out extensive robustness tests. To confirm that the results are not driven by particularities in the BIS data, I collect a dataset of daily equity prices for companies incorporated in affected islands and show that international investors react analogously to international banks in a sample dominated by financial firms. Equity prices of firms domiciled in non-OFCs significantly underperform those of firms domiciled in OFCs after hurricanes hit.

dropped from either sample, alleviating concerns of mis-classifying one country such as Puerto Rico (treated as a tax haven in Garrett and Suárez Serrato, 2019; Suárez Serrato, 2018).

This negative impact for non-OFC firms confirms work on hurricane impacts in stock markets (Kruttli et al., 2025).

If financial service activity is not local, where is it carried out? Among other information, the 'Paradise Paper' data leak included the incorporation dates of firms, including shell companies, in six OFC corporate registries. I collect these into daily time series of incorporation activity and document substantially lower company incorporation activity during public holidays in London, Tokyo, and New York that are normal workdays on the island in question. This indicative evidence suggests a connection of OFC service provision to activity in these financial hubs. Finally, if financial service activity is not local, does the local economy benefit from it? Comparing the relationship between nightlights and the financial sector directly shows the expected positive correlation as countries develop for non-OFCs both within and between countries. This correlation is entirely absent for OFCs, casting doubt on offshore finance as a viable development strategy.

Beyond the policy implications mentioned above, this paper improves the understanding of financial intermediation activities on OFCs on which the literature on tax evasion and illicit financial flows summarized above is largely silent. The literature on profit shifting also focuses on the customer of financial services instead of the financial service provider (see Beer et al., 2020; Riedel, 2018; Slemrod, 2019, for overviews).[10] Banks themselves have been studied in their role as MNEs shifting profits themselves (Langenmayr and Reiter, 2022), not in their role as financial intermediaries. The theoretical literature in turn has provided models that allow for both interpretations: the cost of an intermediation service could arise offshore or at home (see for example Slemrod and Wilson, 2009, footnote 14 makes it explicit). Since financial intermediaries can eliminate all potential gains from new regulation attempts, as Bustos et al. (2022) show for regulation of transfer pricing, their role is central. In this paper, I isolate the local comparative advantage in providing financial services through skilled human capital from regulatory arbitrage opportunities. The former should react to local shocks, the latter remains a potential explanation for the high financial sector positions on OFCs.

The paper also makes several secondary contributions. First, it shows that identification strategies beyond the tax changes or transparency measures are possible to study offshore finance. At the same time, it circumvents the policy endogeneity issue endemic to the literature. Second, I provide a number of new datasets hitherto unavailable or unused in

[10]Estimates of profit shifting or tax evasion focus on discrepancies in international financial statistics resulting from such activities (Clausing, 2020; Tørsløv et al., 2023; Zucman, 2013) or on microeconomic data for multinational firms (Bachas et al., 2023; Becker et al., 2020; Clifford, 2019; Davies et al., 2018; Johansson et al., 2017), both of which quantify the amounts but do not focus on the activity that creates them.

the study of offshore finance. The monthly nightlight dataset for small island economies is the first time satellite data is used to study financial service activity on OFCs, to the best of my knowledge.(Bilicka and Seidel, 2022, study car manufacturing using nightlight data). The three proxies of financial service activity provide the most comprehensive image of OFC activity so far: 'mirror claims', equity prices, and daily incorporation series from leaked datasets. None of these data sources rely on data voluntarily reported by OFCs, data reliability concerns are therefore mitigated. Third, the paper is accompanied by an R package for other researchers to build nightlight datasets for any geospatial unit on the planet.[11] Finally, for researchers working on offshore finance, I show that the assumption of no significant activity on OFCs can be extended to the financial sector. Models including financial service cost should assign this cost to the home economy of the tax evader or the MNE.

The study proceeds as follows. Section 2 outlines the identification strategy based on hurricane impacts in detail. Section 3 introduces sample choices and the data sources: geo-spatial data on nightlight intensity, hurricane data, and data measuring financial service activity. Section 4 introduces the methodology and provides results on hurricane impacts, comparing the responses of local conditions and international bank positions. It also includes summaries of the robustness tests and addresses the main threats to identification. In section 5, extended results on company incorporations and the direct connection between nightlights and financial service activity are discussed. Section 6 concludes.

[11] This package allows the construction of nightlight statistics beyond the small islands used in this study. It was prepared and is maintained together with Mark Toth and available at: github.com/JakobMie/nightlightstats.

2. Identification: Hurricane Impacts

Identification in research on offshore finance is often achieved by exploiting policy changes tackling OFCs (see Lejour and Schindler, 2024; Slemrod, 2019; Zucman, 2014, for overviews) which introduces endogeneity concerns if they are implemented in response to rising capital positions. The natural experiment of recurring hurricanes provides a source of more plausibly exogenous variation. This paper only uses storms categorized as natural disasters, leading to extended power outages, disabled infrastructure, evacuations, flooding, and direct casualties. Hurricane Irma in autumn 2017, for example, directly affected 1.2 million people with wind speeds of up to 295 kilometers (183 miles) per hour, leading to damages of 50 billion USD in the United States alone, and cut electricity for several million inhabitants on Caribbean islands and in Florida.[12] The hurricane affected eight OFCs and five non-OFC islands, narrowly missing others. Local impacts were substantial: 90% of all buildings on Barbuda were destroyed, 95% of all houses on Sint Maarten became uninhabitable and the death toll of Puerto Rico reached 4,645 (Kishore et al., 2018).

— Figure 1 about here —

Figure 1 summarizes how these shocks can inform us about financial service provision on OFCs. A hurricane at time t is expected to lead to a deterioration of local conditions, both on OFCs (top) and non-OFCs (bottom). The impact on financial service activity can be verified using non-OFCs (bottom right). If financial service providers operate locally, physical destruction such as power outages and infrastructure breakdowns impact their working conditions. The identifying assumption is that financial service providers in OFCs cannot completely insulate themselves against the same hurricanes that non-OFC providers react to. If it holds and if hurricanes do not affect the financial service provision of OFCs (top right), I take this as evidence that their activity is not local.

The strength of this simple identification strategy is the large number of OFCs and non-OFCs that are in hurricane areas and provide rich treatment variation. The Caribbean part of the sample is called 'hurricane alley' due to the recurring tropical storms that form due to the Gulf Stream. In the Pacific and Indian Oceans, islands are more spread out but also regularly hit by typhoons. Its weakness is that for most islands in the sample, neither local conditions nor financial service activity are readily observable. The following data section constructs new datasets to address this issue.

[12] US Office for Coastal Management, see https://coast.noaa.gov/states/fast-facts/hurricane-costs.html, last accessed the 13th of March, 2025.

3. Data

There are 104 island jurisdictions on the planet but not all are useful for studying hurricane impacts. First, I drop islands in oceans without hurricane activity. Second, I exclude countries without geospatial data or iso3 codes, for example pure military bases. Third, I find two clear cutoff points in size and dispersion of island groups. Annex A.1 describes these choices in detail and Table A.1.1 provides an overview of all island jurisdictions on the planet and potential reason(s) for exclusion. These transparent choices lead to sample of 56 island economies to test for hurricane impacts. Below, I introduce the three data sources with large to complete global coverage that are used in the main results.

3.1. A monthly nightlight dataset for small island jurisdictions

Satellite data is frequently used by development economists trying to measure economic conditions in remote areas or countries with unreliable national accounts which also makes them useful for studying small island economies.[13] Here, I employ the Visible Infrared Imaging Radiometer Suite (VIIRS) nightlight data which provide a resolution of around 750 meters at the equator, has low light detection limits, and several technical improvements over older datasets used in the literature on hurricane impacts.[14]

— Figures 2 and 3 about here —

The VIIRS satellite has a nightly overpass time at 1:30 am and scans are aggregated into monthly composites. I combine these globally available shapefiles[15] with geospatial data on national and sub-national boundaries of the sample islands available from the Global Administrative Areas dataset.[16] Figure 2 shows the Caribbean part of the data with nightlight intensity plotted in blue. The geospatial polygons of the island economies are plotted in solid black lines for OFCs and in dashed grey lines for non-OFCs.

[13] Henderson et al. (2012) provide a seminal contribution relating nightlight data to economic growth, for a summary see Donaldson and Storeygard (2016). Strictly speaking, the identification strategy used here does not rely on the ability of nightlight data to proxy GDP well. For the main results, nightlights are merely used as an impact measure to test if both OFC and non-OFC islands are affected by local shocks.

[14] Most sources in the literature relating storms to nightlights as well as most studies in development economics are based on an older yearly data source based on the Defense Meteorological Satellite Program (DMSP) of the US military (Bertinelli and Strobl, 2013). Abrahams et al. (2018) provide details of the VIIRS improvements, such as corrections for stray light, lightning, cloud cover, and other outliers and better comparability as scans move away from the equator (Abrahams et al., 2018). The VIIRS sensors also have no light saturation point which had made differentiation of very light areas difficult in the older DMSP data (Mohan and Strobl, 2017).

[15] These large monthly nightlight maps are available via the Colorado School of Mines. Last accessed 13th of March, 2025, at: https://payneinstitute.mines.edu/eog/

[16] Last accessed 13th of March, 2025, at http://www.gadm.org

Within each country polygon, be it national or smaller, it is then possible to calculate statistics of the nightlight intensity in each jurisdiction and each year-month available. I create monthly time series starting in April 2012 when maps become available for every sample island. Figure 3 shows the mean of nightlight intensity for five islands to provide intuition on the variation.[17] The sudden drop in the British Virgin Islands in September 2017 coincides with hurricanes Irma & Maria mentioned above. These time series have no gaps and a relatively high (monthly) frequency. Most importantly, data on Montserrat, a British Overseas Territory with only around five thousand inhabitants and little usable data from other sources, are just as readily available as data on Jamaica with 3 million inhabitants. This is the dataset used to measure the local impact of hurricanes.[18]

3.2. Mirror data on bank claims

The data availability problem for OFCs extends to financial data. However, bilateral datasets allow the construction of mirror data, i.e. data reported *against* the jurisdiction of interest from other countries. In its Locational Banking Statistics, the Bank for International Settlements (BIS) provides bilateral quarterly time series on banks' international claims on an immediate counterparty basis.[19] These positions include loans to banks and non-bank financial institutions (NBFIs), including intra-group positions. They capture the active funding channel of the financial sector. A drop in these international mirror claims indicates decreased funding requirements and thus decreased activity of the counterparty banks and NBFIs as internationally active financial service providers operate with high leverage ratios.

While only four islands in the sample report any data to the BIS, reports against 30 island economies are available.[20] These reports are summed at the counterparty country level

[17] Radiance of nightlight is measured in units of $Wcm^{-2}sr^{-1}$, or watt per steradian per square centimeter. For usability, these radiance values are multiplied by $1E9$ by the NOAA National Geophysical Data Center. They are used in the resulting unit here, which leads to a continuous scale with a maximum of around 30 for most jurisdictions in the sample.

[18] An R package allowing other researchers to build such data for any geospatial unit on the planet is was prepared and is maintained together with Mark Toth and available at: github.com/JakobMie/nightlightstats.

[19] The tax evasion literature instead uses data on liabilities, especially deposits, reported by tax havens themselves (see for example Alstadsæter et al., 2024; Casi et al., 2020; Johannesen and Zucman, 2014; Langenmayr, 2017; Menkhoff and Miethe, 2019). The data used in this study are from a different part of banks' balance sheets and reported by different countries: it captures the active funding side of the financial sector.

[20] While coverage is not complete, the BIS makes data reported by 19 large non-OFC economies public, including mostly OECD countries. These reporting non-OFC countries are: Australia, Brazil, Canada, Germany, Denmark, Spain, Finland, France, the United Kingdom, Greece, Italy, Japan, South Korea, Mexico, Philippines, Sweden, Taiwan, the United States, and South Africa. Luxembourg and the Netherlands are excluded due to their presence on several tax haven lists.

as $Mirrorclaims_{it} = \sum_{j=1}^{J} claims_{jit}$. Here, country i can either be an OFC or a non-OFC island and claims are summed for all non-OFC reporting countries, $j = 1, ..., J$. A balancing choice is needed, as reports by different countries and against different counterparties start at different points in time, introducing a tradeoff between the cross sectional and time dimensions. Here, data balanced in 2012q2 is used after which satellite data is available. Any bilateral series that starts later is excluded. Annex A.1 provides details and figures that show that the data lost are negligible in aggregate and do not affect the dynamics. Crucially, these mirror claims can also be constructed for any island in the sample such as Mauritius with 1.3 million inhabitants but close to 13.5 billion USD of mirror claims reported against it in 2018qI by banks from large non-OFCs.

3.3. Data on hurricanes

National data on hurricanes are taken from the EM-DAT[21] disaster database that collects the exact timing of natural disasters, including statistics on the number of inhabitants and locations affected.[22] Since such disasters are precisely dated, these data can be used at all frequencies employed here: monthly to analyze data on nightlight intensity, quarterly to analyze BIS bank claim data, and daily to analyze equity prices. Many hurricanes in the sample hit both OFCs and non-OFCs but never all islands. The classification of hurricanes into natural disasters follows that of the Emergency Events Database.[23]

— Table 1 about here —

The resulting sample and summary statistics are shown in Table 1. The average OFC is small. The Cayman Islands or Bermuda have only 60,000 and 70,000 inhabitants, respectively (column 1), but mirror claims of 1.5 trillion USD and 63 billion USD (column 3). With an estimated GDP per capita of \$85,700 (column 2), citizens of Bermuda are theoretically much richer than US (roughly \$60,000), German (\$50,000), or French (\$43,000) citizens. The mean of nightlights could be constructed for all islands in the sample (column 4). It can be used to evaluate hurricane impacts, the frequency of which also varies across countries (column 5).

[21]The Emergency Events Database - Université catholique de Louvain (UCL) - CRED, D. Guha-Sapir - www.emdat.be, Brussels, Belgium, last accessed the 18th of March, 2025.

[22]The literature focusing on establishing precise growth declines due to hurricanes (see for example Strobl, 2011, 2012) provides detailed geo-spatial impact estimations. The present project, however, is limited by financial data that are only available nationally.

[23]This database, for example, does not classify Cyclone Gita as an emergency event in American Samoa in 2018.

4. Main Results: Hurricane Impacts

This section first discusses the empirical methodology. With two main data sources available to test for hurricane impacts on small island economies, it proceeds in two steps: first, testing for reactions of local conditions and, second, of the financial service sector. For both dimensions, results in the non-OFC sample are provided to verify the identification strategy.

4.1. Methodology: Multiple Event Study with Binned Endpoints

More than one hurricane can hit an island at different event-times, I therefore use an event study design that allows for staggered adoption and multiple treatments of the following form:

$$i.h.s.(y_{it}) = \sum_{j=\underline{j}}^{\bar{j}} \beta_j b_{it}^j + \mu_i + \theta_t + \varepsilon_{it} \tag{1}$$

Where $i.h.s.(y_{it})$ are the outcome variables (the inverse hyperbolic sine transformation of nightlights or mirror claims),[24] μ_i are unit specific intercepts, θ_t calendar time fixed effects, and ε_{it} idiosyncratic errors. The sum around b_{it}^j collects event study dummies around hurricane impact as well as binned end points.[25] Binning the endpoints intuitively assigns observations outside the effects window to the control group, therefore improving identification of the time trend in θ_t. The sample includes never treated islands that do not experience hurricanes during the observation window for the treatment status, so no further conditions on the endpoints (as pointed out in Schmidheiny and Siegloch, 2023) or the event study dummies (as pointed out in Borusyak et al., 2024) are needed. The period $d_{i,t-1}$ is omitted as a reference period in the monthly data and $d_{i,t}$ in the quarterly data.[26] Coefficients β_j are interpreted in relation to this reference point.

The results section employs an effects window of $+/-1.5$ years of event time. The sample ends with 2020, avoiding impacts of the Covid pandemic. Standard errors are clustered at the island level, the level of treatment variation. While this choice might seem generic,

[24] The log equivalent inverse hyperbolic sine transformation is calculated as $ihs(x) = log(x+(x^2+1)^{1/2})$. Log and level results are provided in Annex A.3, Figure A.3.4.

[25] Note that the bins here are not dummies, deviating from Schmidheiny and Siegloch (2023), due to the presence of multiple treatments per country.

[26] This differentiation is necessary because, in quarterly data, a hurricane in the quarter preceding impact could be up to three months away from the actual effect if it takes place at the beginning of the quarter. A quarter is classified as a hurricane quarter if at least one (but sometimes more) hurricanes take place in a particular quarter. When a hurricane spans quarters, both quarters are defined as hurricane quarters.

it is motivated by the unpredictability of hurricane impacts across hurricane-island pairs, following the suggestions in Abadie et al. (2022). If a hurricane actually makes landfall on a small island can still be uncertain hours before impact. To quantify results, conventional differences-in-differences estimations are provided below.

A recent literature has pointed out potential bias of standard fixed effects panels due to cohort specific treatment effects differentiated by treatment timing (Borusyak et al., 2024; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021). Some of the problems treated there extend to event studies with staggered adoption as the one employed here. One example is a comparison of "already treated" cohorts on a new post treatment trend to "later treated" cohorts while "not yet treated" are not problematic. This literature focuses on single events and persistent policy changes. Hurricanes instead have been happening long before my sample starts, "not yet treated" islands are not a meaningful reference point in the population. Instead, hurricanes are recurring events with transitory effects. Periods outside a generous effects window will not be affected by the impact, making the 'forbidden comparison' issue coined in Borusyak et al. (2024) less salient. I thus explicitly maintain the assumption that the treatment, in within changes, has no effect outside of the effects window specified above (Schmidheiny and Siegloch, 2023; Sun and Abraham, 2021) a weaker version of the effects homogeneity assumption (De Chaisemartin and d'Haultfoeuille, 2023). Island-time observations of treated islands outside the effects window are also not part of the control group (hence cannot bias the effects) beyond the identification of the calender time trend because they load on the binned endpoints instead of being the reference point for the post event dummy. This additionally avoids an over-weighting of 'early adopters' which otherwise would have more weight through long post-periods in the treatment group (De Chaisemartin and d'Haultfoeuille, 2022). Instead, each full effects window contributes with equal weight to the average effects reported in the difference-in-differences and event study results below. Coefficients on the endpoints are reported to directly test and confirm the transitory nature of hurricane impacts. For completeness, I confirm the results of this paper using the estimator provided in Borusyak et al. (2024) in the robustness section, repeatedly drawing at most one hurricane per island in a bootstrapping exercise to circumvent its limitation to a single treatment per treated island in my multiple treatment setting. I prefer the more intuitive and flexible event study with binned endpoints as the main specification here because both intuitively and empirically, heterogenous treatment effects over time do not seem to be a central threat to identification at hand.

4.2. The Local Impact of Hurricanes on Island Economies

This section provides the baseline impact of hurricanes on local conditions. To the best of my knowledge, no panel results on the Caribbean using VIIRS nightlight data exist to date, making the results in this section a contribution in their own right. Available studies focus on the effects on GDP of hurricanes hitting South America and the Caribbean as well as US county per capita income (Strobl, 2011). Hurricane impacts on nightlights are analyzed for the Caribbean using older yearly DSMP data (Bertinelli and Strobl, 2013) with one study employing VIIRS data to analyze the impact of cyclone Pam hitting Vanuatu in the Pacific Ocean (Mohan and Strobl, 2017). Assumptions about the relationship between nightlights and economic activity are not needed here, this section merely provides a baseline measure of hurricane impact. This impact will then be compared to reactions of the financial service industry.

— Figure 4 about here —

Figure 4 shows the results of the multiple event study with binned endpoints introduced above using nightlights as the outcome variable. The graph plots the β^j coefficients with 95% confidence bands. The top panel provides results for the entire sample and shows a stable pre-trend for 1.5 years of event time before hurricanes hit. With the hurricane hitting at $j = 0$ a significant impact is visible. Recovery sets in immediately after that. However, it takes 5 months for the negative coefficients to be statistically insignificant and 9 months for the coefficients to return to zero. The second and third panels split the sample into OFCs (middle panel) and non-OFCs (bottom panel). Here, the control group is made up of never treated islands in both sub samples and observations that are in the binned endpoints within the same group of countries. This sample split decreases statistical power but results on both the immediate impact as well as the long recovery period are confirmed.

— Table 2 about here —

To quantify results, Table 2 provides results of difference-in-differences specifications where a treatment dummy collects the first nine months after hurricane impact. Coefficients can therefore be interpreted as the average effect of the hurricane impact compared to the nine months before it hits. The first two columns show this regression on the log of nightlight intensity. In offshore financial centers, nightlight intensity drops by 21% (0.19 log points) on average for the 9 months after a hurricane hits (column 1). The effect is comparable to the non-OFC sample (20% or 0.18 log points, column 2) and statistically significant in both sub-samples. Here, the binned endpoints are shown as well and indicate that the event study is well specified with no significant results for the endpoints and coefficients on the bins close to 0.[27] Columns 3 and 4 show the results on the inverse hyperbolic sine transformation but due to the presence of many values at 0 or between 0 and 1, the size of these coefficients is not straightforward to interpret here. A discussion of this issue is provided in Annex A.3. Figure A.2.5 of Annex A.2 provides nightlight results using only the capital regions of the islands in the sample and confirms the main results.

The drop in nightlights is therefore strong on impact and the average effect over the recovery period shows around 20% lower nightlight intensity on both island groups in the sample. These impacts are in line with existing research on hurricane impacts that shows recovery periods of at least half a year and decreases of GDP growth by 0.45% to 1.5% in a given year (Bertinelli and Strobl, 2013; Mohan and Strobl, 2017; Strobl, 2011, 2012). Effects last several months with two existing studies showing an effect on nightlights that lasts up to 15 months in the Dominican Republic (Ishizawa et al., 2019) and around 7 months for cyclone Pam hitting the Pacific island of Vanuatu (Mohan and Strobl, 2017). As a case study confirming the regression results, Annex A.2 shows the impact of hurricanes Irma and Maria in the Caribbean, the strongest hurricanes in the sample. These hurricanes are visible by eyeballing nightlight maps (Figures A.2.1 and A.2.2). Hurricanes that hit island economies are thus associated with a substantial deterioration of local conditions. These impacts are visible in both OFCs and non-OFCs and only die out nine months after impact in the OFC sample. They are consistent with qualitative evidence on power outages, evacuations, infrastructure breakdowns and general uncertainty around hurricane impacts on island economies. The next section now explores how these impacts affect the operation of the financial service sector.

4.3. The Impact of Hurricanes on Financial Service Provision

The prolonged recovery period documented above validates the use of a quarterly dataset, which is the highest frequency available from the BIS. Except for this change in frequency, results below employ the same methodology, sample choices, and treatments as the last section, and again show coefficients of 1.5 years of event time before and after hurricane impacts. Contrary to the strong impacts on local conditions, the top panel of Figure 5 shows a striking non-result in the financial sector on offshore financial centers. BIS mirror claims reported against OFCs do not react to hurricanes. The pre-trend between

[27] The bins are not plotted in the treatment graphs following Fuest et al. (2018).

affected and non-affected OFCs is quite stable for macroeconomic data on international capital movements. The post-hurricane coefficients are virtually zero for 1.5 years and never statistically significant. Over three years of event time around hurricane impacts, no significant effect is discernible.

— Figure 5 about here —

These results could show that the financial service sector is not affected by hurricanes in general. This is not the case. The bottom panel of Figure 5 shows the results of the same regression for the non-OFC part of the sample, a falsification exercise. As for OFCs, the pre-trend is statistically insignificant and coefficients are small. When the hurricane hits, however, mirror claims start to deteriorate. The drop builds up over time since mirror claims are a stock measure. In the first quarter after the hurricane hits, mirror claims reported against affected OFCs decrease by 0.176 log points relative to the control group. By the end of the effects window, this drop increases to 0.36 log points (roughly 42%) compared to the first quarter of the hurricane impact.

— Table 3 about here —

To make results more interpretable quantitatively, Table 3 shows differences-in-differences exercises. Here, the five treatment lags in the effects window are collected into one dummy and should be interpreted relative to the pre-event window. Column 1 shows the insignificant effect in the OFC sample. Again, the binned endpoints are also shown here and are not significant with coefficients close to 0. Column 2 shows the non-OFC sample. The average effect in the post-event period indicates a 0.34 log point (or 40%) reduction in mirror claims relative to the pre-event window. The upper bin ($\text{Bin}_{j=6:j=\bar{j}}$) remains significantly negative which indicates a persistent effect. This is intuitive for a stock measure. A transitory shock to the competitiveness of non-OFC banks and NBFIs reduces this measure long term unless affected islands start to *outperform* the control group in later periods. Such catching up is at best visible partially as the long term effect is about half the size of the short term impact (0.17 log points). To show the difference between reactions of OFCs and non-OFCs directly, column 3 uses the entire sample with an OFC interaction term on the hurricane dummy. Intuitively, this pools the control group of both sub-samples, hence the coefficients of the hurricane dummies doe not simply add up from the sample split. The post-hurricane dummy interacted with the OFC dummy shows insignificant and small coefficients while coefficient on the interaction term with the non-OFC dummy again shows a large negative and statistically significant effect. Columns 4 and 5 repeat columns 1 and 2 using a logarithmic specification. In this dataset without many values at 0 and none between 0 and 1, results are virtually identical to those using the inverse hyperbolic sine transformation.

There are a number of **threats to identification** that could be driving the result above. First, it is possible that the effects shown are an artifact of developments in the banking sector. The sample period from 2012q2 to 2019q4 includes the Euro crisis and a period of quantitative easing, which could lead to rising claims in OFCs compared to non-OFCs. To verify that the results above are not driven by developments in the banking sector that are independent of hurricanes, columns 6 and 7 of Table 3 carry out another falsification exercise. In addition to the active side of non-OFC banks' balance sheets that were used so far, the BIS also provides data on the passive side. These positions measure the amount of liabilities, mostly bank deposits, that are reported against islands in the sample. Such mirror-liabilities include, for example, the bank account of a Jamaican company at a bank in France. These positions do not need to be reduced when a hurricane hits because reporting banks are not hit by the hurricane. Results show small coefficients both for the OFC and non-OFC depositors. The small positive coefficient on depositors from OFCs (column 6) is marginally significant, however, the robustness section shows that it should not be interpreted as it does not coincide with hurricane timing. These results do not mirror the effects on mirror claims reported above. This shows that the significant negative effect on mirror claims in non-OFCs (column 2) is no statistical artifact of other bank sector developments.

To show that the results are not driven by a particular country in the sample, an extensive sample check is provided here. For both island groups and both outcome variables of the main results, specifications that drop each sample country in turn are provided. Figure 6 shows the results for nightlights. Results using the OFC sample are plotted in the top panel and show that results do not fluctuate much. The bottom panel shows the results for the non-OFC part of the sample where results are also robust across all specifications. Not a single specification deviates significantly from the main results.

— Figures 6 and 7 about here —

Figure 7 plots results of the same exercise for mirror claims. The top panel again shows the OFC part of the sample where not a single coefficient turns significant and coefficients are quite stable around 0. These results also reinforce the interpretation of this estimation as a non result: Coefficients are small, insignificant, and do not exhibit sign certainty. The bottom panel plots the same results for the non-OFC part of the sample. Results hold here as well. Generally, despite the limited availability of data on island economies, results are quite robust to changes in the sample specification.

Annex A.3 provides robustness tests of three other concerns: First, robustness to different OFC lists is tested for the mirror claims results where results for both groups differ. Second, I change methodological choices of the main results (binning, logs vs. i.h.s., event study results of falsification exercises). I also confirm the BIS results using the estimator provided by Borusyak et al. (2024) and show that the nightlight results hold even if only the capital regions of sample islands are considered.

What could be driving the negative reaction on non-OFCs other than financial institutions not being able to operate due to the hurricane? Since mirror claims capture the international lending channel for banks and non-bank financial institutions, the fact that the customer base of banks on OFCs and non-OFCs could differ is not crucial here: No matter what activity the local financial actor carries out, its day-to-day operations depend on foreign (including intra-group) funding. Only international positions are measured in the BIS claims data, making data comparable across island types irrespective of how the financial sector then uses this funding.

A reduction of local demand for credit in non-OFCs is also unlikely. Demand for credit increases after natural disasters (Koetter et al., 2020) and the destruction of physical capital leads to higher marginal productivity of capital in the affected islands. Cortés and Strahan (2017) show that financially integrated banks, the kind that would show up in the BIS data used here[28], make use of this opportunity by using their network to wire funds into affected areas. They also show that banks aggressively seek additional funding in non-affected areas to exploit the business opportunities such emergency lending offers. The steady recovery of nightlight intensity documented above shows that reconstruction is actually taking place on non-OFCs. Both increasing credit demand and supply accommodation through redeployment of funds to affected islands would show up as an *increase* of mirrorclaims if international lending is involved. If it is done entirely locally through balance sheet expansion or deposit drawdown, mirrorclaims would not react at all. Neither case can explain the negative reaction documented here. They are consistent with reduced activity due to physical destruction, however.

The BIS data captures bank claims based on bilateral links. This leaves open the possibility that effects are not driven by the bank on the island but by shocks to the counter-

[28] Brei et al. (2019) find a negative effect of natural disasters on bank deposits and, by extension, their activity. However, they explicitly distinguish their findings from those in Cortés and Strahan (2017) because they focus on a sample dominated by institutions that are "self-financing from deposits rather than credit" (p. 237). Koetter et al. (2020) also confirms that amongst shock exposed banks, credit risk is higher for those banks not integrated into diversified interbank markets. These are exactly the kind of institution that would not show up in the BIS data. Non-integrated self-financing banks do not rely on international loans to finance their activities. These loans are the main component of the mirrorclaim series employed in this study.

party.[29] Also, macroeconomic statistics on small island economies can be dominated by a few large banks. I therefore turn to the reaction of international investors by employing company level equity price data from Bloomberg. Bank and non-bank financial institutions are integrated internationally, therefore, a number of those domiciled in sample islands are listed on international stock exchanges. If these firms face difficulties providing their services post-hurricane, rational investors are expected to sell, thus pushing down equity prices.

While the treatment variation and hence the identification strategy is national, there are several advantages of using microeconomic financial market data here. First, they reflect investors' expectations about the future, hence immediate reactions to developments with medium term consequences such as hurricanes should register within a few trading days. This makes it less likely that results are conflated with reconstruction efforts. Second and again because prices reflect expectations, this data shows if market participants are aware of the fact that hurricanes do not impede the operation of the financial service industry that books its activities on OFCs. Third, it provides a second proxy of financial service activity from an entirely different dataset which can lend credibility to the main results introduced above. The downside of this data is that it is less globally available than the mirrorclaim data because few publicly traded companies are officially incorporated in the small islands that make up the sample here. Most importantly, my dataset does not include sufficient data for non-affected islands to use as a never treated control group.

I construct a daily dataset of 680 equity price series taken from Bloomberg for a sample period starting on the first of April 2012 and ending on the last day of 2019, mirroring the sample used so far. As common in analyses of financial markets, I carry out an event study in the spirit of Kothari and Warner (2007) using hurricanes as a potential shock to the net present value of the equity of firms domiciled in OFCs.[30] First, returns below the 1st percentile and above the 99th percentile are winsorized, then daily abnormal returns (AR_{it}) are calculated as the deviation of realized returns (RR_{it}) from expected returns (ER_t). For expected returns, I follow convention and use the S&P Global 1200 stock market index (like Johannesen and Larsen, 2016).

$$AR_{it} = RR_{it} - ER_t \tag{2}$$

[29] The immediate reaction identified in the staggered adoption setting together with the falsification exercise on bank liabilities hopefully alleviate some of these concerns.

[30] Manual inspection shows that this list of firms is dominated by banks as well as non-bank financial institutions, such as holding companies, insurance firms, credit companies, and other financial service firms. However, some shipping companies are also included and the odd agricultural exporter cannot be excluded. My data access does not allow me to clearly distinguish sector information. However, any non-financial firm will bias the results towards finding a negative impact in OFCs as well and hence against finding a difference between both island types.

In equation 2, i denotes the equity price series and t the respective trading day. As mentioned above, hurricanes are hard to anticipate and especially the extent of the impact comes as a surprise. Forward looking investors will therefore adjust their portfolio quickly if the business they are invested in experiences a detrimental shock. A treatment window of 1.5 years is not useful here. Instead, I look at a window of 14 days before and after the hurricane impact. In this window, I consider average abnormal returns for the entire period j trading days away from the hurricane impact. This is equivalent to taking means and then cumulating until one specific trading day when calculating Cumulative Abnormal Returns (CAR) as suggested in Kothari and Warner (2007). It allows an event study specification of the following form:

$$CAR_{it} = \alpha_i + \gamma_m \times \alpha_c + \sum_{j=\underline{j}}^{\bar{j}} \beta_j b_{it}^j + \epsilon_{it} \tag{3}$$

Here, CAR_{it} is the cumulative abnormal return of equity-storm combination i on trading day t, α_i are equity-storm fixed effects and γ_j trading-day-from-hurricane fixed effects effectively making OFCs the control group for non-OFCs, and $\gamma_m \times \alpha_c$ are yearmonth by country fixed effects. The event study dummies denote the distance j from hurricane impact.[31]

— Figure 8 about here —

Figure 8 shows the resulting β^j coefficients. Results show a relatively stable pre-trend before hurricane impacts. non-OFCs equity do seem to underperform OFCs in three days before impact but overperform another three days and all of these coefficients are small. Two days before the hurricane hits, substantial price changes are apparent suggesting some albeit very short term anticipation. After impact, returns decline substantially on non-OFCs compared to OFCs. The effect stabilizes by trading day five, after which cumulative abnormal returns remain 20% lower than those on OFCs. This impact is not made up for by the end of the effects window which, however, would require a subsequent overperformance of non-OFCs compared to OFCs which there is no reason to expect.

[31] There are several differences compared to the specification of the main section here that reflect data availability and the daily frequency of price data. First, I do not have access to sufficient data on non-affected islands which prohibits the split sample control group approach used above. Instead, this specification directly compares companies on affected non-OFCs to companies on affected OFCs, showing the difference between both reactions. Second, the dataset is made up of one month periods per hurricane (two weeks before and after impact), one equity series can enter several times if its country of domicile is hit twice and it was traded around both impacts. I include yearmonth by country fixed effects to broadly control for country growth rates and business cycles. No binned endpoints are employed as the pinning of long term time trends is not salient for short term market fluctuations. Instead, data outside of the effects window is dropped from the sample.

The results of this section show pronounced hurricane effects on local conditions as well as on the financial sector in non-OFCs. The only area where hurricanes did not appear to take place in the event studies is financial service provision on offshore financial centers. There is a disconnect between local conditions and international financial service activity on OFCs in reactions to local shocks and international investors seem aware of this fact. If, indeed, local banking or legal skills are useful for international customers, it is puzzling that these activities go on unabated during hurricanes. Instead, these results are consistent with the hypothesis that international financial activity that is booked as taking place in OFCs is not local. The next section provides suggestive evidence in this direction before turning to implications for development.

5. Extended Results

5.1. Local Company Incorporations

Corporate registries of OFCs are generally not publicly available. Secrecy is part of the OFC definition used in this text. However, together with the Paradise Papers leak, the international consortium of investigative journalists (ICIJ) published significant subsets of the leaked corporate registries of Aruba, the Cook Islands, Bahamas, Barbados, Malta, Nevis, and Samoa. Without the Appleby data, that made headline news but is not representative for a specific jurisdiction, the leaked registries include data on 265,150 unique company registrations and their incorporation dates. For the six OFCs for which company registers were leaked, I aggregate incorporation dates of firms into time series counting the number of incorporations per day on the island in question. While this sample is too small to analyze hurricane impacts statistically, the daily dimension shows interesting patterns across work days. It includes shell companies that are used for tax non-compliance.

— Figures 9 and 10 about here —

As a sanity check, Figure 9 shows incorporation activity over the entire sample by weekday in the six islands. The fact that almost no incorporations take place on weekends suggests some connection to actual human activity. If activity declines during weekends, it is reasonable to assume that it declines during public holidays as well. The interesting question is: During whose public holidays?

A decline in incorporation activity during local public holidays is clearly visible in the data, using data on all public holidays on these OFCs since 1990 (see Annex A.4). Figure

10 now shows the difference in daily incorporations during public holidays in the financial centers London, Tokyo, and New York that are normal workdays on the islands. The baseline against which these incorporations are compared excludes weekends and public holidays on the OFCs from the sample. These effects are therefore a lower bound: common holidays such as New Year's Eve and Christmas are excluded. Nevertheless, almost all differences are negative. During a public holiday in London that is a normal workday on St. Kitts and Nevis, incorporation activity on St. Kitts and Nevis still drop by 4.5 incorporations (left panel) or 44% of average daily workday incorporation activity on that island (right panel). Barbados, the Cook Islands, and Malta also show drops between 17 and 35%.[32] Financial service activity in OFCs is connected to activity elsewhere, but human work days do still matter. This evidence is selective since only six time series on incorporations are available. Still, it hints at an interesting avenue for future research investigating the bilateral links between OFCs and financial centers in OECD countries. The three cities shown in Figure 10 could be useful starting points.

5.2. Offshore Finance and Long Term Development

If financial service activity is not local, what does the OFC gain by allowing such activity to be booked within its jurisdiction? The large financial positions in OFCs could lead to high income in the form of fees or taxes. Such income can be substantial, even with very low tax rates due to the inflated foreign tax base relative to small island economies (see Tørsløv et al., 2023, for a similar point regarding European tax havens). However, it is an open question how these funds are used and to what extent they, or the revenue they generate, end up in the local economy. By investigating the direct relationship of nightlights and mirrorclaims, a result is borrowed here from development economics, namely, that nightlight intensity is positively correlated with measures of economic development (Donaldson and Storeygard, 2016; Henderson et al., 2012).

— Figure 11 about here —

Using both series at quarterly frequency by averaging nightlight intensity over the quarter, Figure 11 plots nightlights over international bank mirror claims for each year-quarter and country. Both variables are transformed using the inverse hyperbolic sine transformation to retain negative and zero values. An equivalent of Figure 11 using logs is provided in Annex Figure A.3.6. The top panel of Figure 11 shows that there is no relationship

[32] The strong relative drop in the Bahamas are due to close to no incorporations during foreign holidays there. However, the baseline incorporation activity of the Bahamas is also very low (on average 0.4 firms per work day compared to 5 in Barbados or 10 in St. Kitts and Nevis).

between local conditions and international bank claims in the OFC part of the sample, neither between nor within jurisdictions. This is an interesting finding in its own right: it suggests that foreign financing in the form of loans and assets held by foreign banks is not directly associated with higher economic activity in OFCs. The bottom panel shows a positive correlation for non-OFCs both between countries as well as within countries. The correlation coefficient of within transformed data for non-OFCs is still 0.14 aganst 0.026 for non-OFCs. The relationship is not linear but increases over mirror claims. If nightlights do proxy real economic activity, this image is intuitive: For countries not dominated by offshore finance, higher foreign capital positions are associated with higher local economic activity. The missing link in OFCs on the other hand raises the question if offshore finance actually supports the aggregate development of OFCs.

6. Conclusions

Little empirical evidence is available on activities of the financial service sector in Offshore Financial Centers (OFCs). A quickly growing literature studies the customers of OFC financial services, such as tax evading individuals, profit shifting firms, or corrupt politicians. However, the financial service sector that facilitates all of this activity is vastly understudied.

In this paper, I exploit the natural experiment of reccurring hurricanes to test if financial service activity registered in OFCs is taking place there. A first set of results shows significant hurricane impacts of around 20% on local conditions proxied with satellite data on nightlight intensity. These effects take 9 months to disappear and are observable for both OFCs and non-OFCs. They are robust to different samples and methodological approaches. Turning to the financial service activity on OFCs, I find no reaction in data measuring international lending against banks and non-bank financial institutions in OFCs. International financial service activity seems to continue unabated, with no significant effects in either direction and coefficients close to zero. A sample of non-OFCs, however, shows significant drops of around 40% after hurricanes hit, again robust across a large number of specifications, OFC lists, and methodological choices. Results are supported by falsification exercises using bank deposits (where no reactions are visible) and confirmed when looking at equity prices of firms incorporated in sample islands. I do not find evidence for a local presence of financial service firms on OFCs.

If financial intermediaries do not operate locally, even an entirely compliant OFC will find it difficult to tackle false reporting for example through audits. This would be necessary to robustly enforce current international regulation attempts to tackle tax evasion and avoidance. Do large OECD economies implicitly outsource the oversight of their large financial service sectors to small islands on the other side of the planet? A set of indicative results using 6 leaked corporate registries shows that the incorporation of (shell) companies on OFC islands decreases during local holidays in Tokyo, London, and New York that are normal work days on the island in question. If policy measures rely on local enforcement but financial service activity booked through OFCs is carried out elsewhere, this is a design flaw that could explain the mixed success of recent policy measures. It also means that financial risks generated through OFCs need to be included in the balance sheets of their home countries as several current data attempts are trying to do without becoming standard yet (see Beck et al., 2024; Coppola et al., 2021; Damgaard et al., 2024; Zucman, 2013, as well as the Consolidated Banking Statistics of the BIS). My results suggest that these approaches should be front and center in improving international financial statistics.

Two final observations concern developments on the OFC itself. I document a readily observable correlation between nightlights and international financial positions in non-OFCs, consistent with economic development if nightlight is a useful GDP proxy (Donaldson and Storeygard, 2016; Henderson et al., 2012). No such correlation is visible for OFCs. This casts doubt on offshore finance as a valid development strategy. Finally, the results of this paper rule out a comparative advantage of OFCs in providing financial services locally. While this doesn't show that OFCs don't provide value, it raises the question how they do so. Regulatory arbitrage is left as the most likely candidate to explain the high financial sector positions in OFCs where they are as dominant as they are in profits booked and bank deposits (see Annex A.1.2, figure A.1.5).

This study can provide a starting point for research into how international financial flows are organized through OFCs. The data, identification strategy, and methodology lend themselves to further analysis. Future research can focus on establishing the bilateral links between specific OFCs and financial centers such as London, Tokyo and New York and continue to expand data availability for financial service activity on OFCs. Evaluations of policy initiatives attempting to regulate OFCs can take into account that the OFC itself might not be able to enforce access to the activities it is supposed to monitor. The development implications of the results presented here raise the question which alternative development strategies are available to small island economies in the face of increasing natural shocks of which hurricanes are only one. Finally, future research should aim to repeat the significant progress made in the analysis of the customers of offshore finance, such as profit shifting firms or tax evading individuals, in the analysis of the financial intermediaries that facilitate their international strategies.

References

Abadie, A., Athey, S., Imbens, G. W., and Wooldridge, J. M. (2022). When should you adjust standard errors for clustering? *Quarterly Journal of Economics*, 138(1):1–35.

Abrahams, A., Oram, C., and Lozano-Gracia, N. (2018). Deblurring DMSP nighttime lights: A new method using Gaussian filters and frequencies of illumination. *Remote Sensing of Environment*, 210:242–258.

Alstadsæter, A., Casi, E., Miethe, J., and Stage, B. (2024). Lost in information: National implementation of global tax agreements. *NHH Dept. of Business and Management Science Discussion Paper No. 2023/22*.

Alstadsæter, A., Johannesen, N., and Zucman, G. (2019). Tax evasion and inequality. *American Economic Review*, 109(6):2073–2103.

Andersen, J. J., Johannesen, N., Lassen, D. D., and Paltseva, E. (2017). Petro rents, political institutions, and hidden wealth: Evidence from offshore bank accounts. *Journal of the European Economic Association*, 15(4):818–860.

Andersen, J. J., Johannesen, N., and Rijkers, B. (2022). Elite capture of foreign aid: Evidence from offshore bank accounts. *Journal of Political Economy*, 130(2):388–425.

Bachas, P., Dom, R., Brockmeyer, A., and Semelet, C. (2023). Effective tax rates and firm size. *EU Tax Observatory Working Paper No. 14*.

Beck, R., Coppola, A., Lewis, A. J., Maggiori, M., Schmitz, M., and Schreger, J. (2024). The geography of capital allocation in the Euro Area. *NBER Working Paper No. 32275*.

Becker, J., Johannesen, N., and Riedel, N. (2020). Taxation and the allocation of risk inside the multinational firm. *Journal of Public Economics*, 183:104–138.

Beer, S., De Mooij, R., and Liu, L. (2020). International corporate tax avoidance: A review of the channels, magnitudes, and blind spots. *Journal of Economic Surveys*, 34(3):660–688.

Bertinelli, L. and Strobl, E. (2013). Quantifying the local economic growth impact of hurricane strikes: An analysis from outer space for the Caribbean. *Journal of Applied Meteorology and Climatology*, 52(8):1688–1697.

Bilicka, K. and Seidel, A. (2022). Measuring firm activity from outer space. *NBER working paper No. 29945*.

Boas, H., Johannesen, N., Kreiner, C., Larsen, L., and Zucman, G. (2024). Taxing capital in a globalized world: The effects of automatic information exchange. *NBER Working Paper No. 32714*.

Bomare, J. and Le Guern Herry, S. (2022). Will we ever be able to track offshore wealth? Evidence from the offshore real estate market in the UK. *Sciences Po Economics Discussion Paper No. 2022-10*. Sciences Po Economics Discussion Paper 2022-10.

Boning, W. C., Hendren, N., Sprung-Keyser, B., and Stuart, E. (2025). A welfare analysis of tax audits across the income distribution. *Quarterly Journal of Economics*, 140(1):63–112.

Borusyak, K., Jaravel, X., and Spiess, J. (2024). Revisiting event-study designs: Robust and efficient estimation. *Review of Economic Studies*, 91(6):3253–3285.

Brei, M., Mohan, P., and Strobl, E. (2019). The impact of natural disasters on the banking sector: Evidence from hurricane strikes in the Caribbean. *Quarterly Review of Economics and Finance*, 72:232–239.

Bustos, S., Suarez Serrato, J. C., Pomeranz, D., Vila-Belda, J., and Zucman, G. (2022). The race between tax enforcement and tax planning: Evidence from a natural experiment in Chile. *NBER Working Paper No. 30114*.

Callaway, B. and Sant'Anna, P. H. (2021). Difference-in-differences with multiple time periods. *Journal of Econometrics*, 225(2):200–230.

Casi, E., Spengel, C., and Stage, B. (2020). Cross-border tax evasion after the Common Reporting Standard: Game over? *Journal of Public Economics*, 190:104240.

Clausing, K. A. (2020). Profit shifting before and after the tax cuts and jobs act. *National Tax Journal*, 73(4):1233–1266.

Clifford, S. (2019). Taxing multinationals beyond borders: Financial and locational responses to CFC rules. *Journal of Public Economics*, 173:44–71.

Coppola, A., Maggiori, M., Neiman, B., and Schreger, J. (2021). Redrawing the map of global capital flows: The role of cross-border financing and tax havens. *Quarterly Journal of Economics*, 136(3):1499–1556.

Cortés, K. R. and Strahan, P. E. (2017). Tracing out capital flows: How financially integrated banks respond to natural disasters. *Journal of Financial Economics*, 125(1):182–199.

Damgaard, J., Elkjaer, T., and Johannesen, N. (2024). What is real and what is not in the global FDI network? *Journal of International Money and Finance*, 140:102971.

Davies, R. B., Martin, J., Parenti, M., and Toubal, F. (2018). Knocking on tax haven's door: Multinational firms and transfer pricing. *Review of Economics and Statistics*, 100(1):120–134.

De Chaisemartin, C. and d'Haultfoeuille, X. (2022). Two-way fixed effects and differences-in-differences estimators with several treatments. *NBER Working Paper No. 30564*.

De Chaisemartin, C. and d'Haultfoeuille, X. (2023). Two-way fixed effects and differences-in-differences with heterogeneous treatment effects: A survey. *Econometrics Journal*, 26(3):C1–C30.

De Simone, L., Lester, R., and Markle, K. (2020). Transparency and tax evasion: Evidence from the Foreign Account Tax Compliance Act (FATCA). *Journal of Accounting Research*, 58(1):105–153.

Dharmapala, D. (2008). What problems and opportunities are created by tax havens? *Oxford Review of Economic Policy*, 24(4):661–679.

Dharmapala, D., Foley, C. F., and Forbes, K. J. (2011). Watch what I do, not what I say: The unintended consequences of the Homeland Investment Act. *Journal of Finance*, 66(3):753–787.

Dharmapala, D. and Hines, J. R. (2009). Which countries become tax havens? *Journal of Public Economics*, 93:1058–1068.

Donaldson, D. and Storeygard, A. (2016). The view from above: Applications of satellite data in economics. *Journal of Economic Perspectives*, 30(4):171–198.

Fuest, C., Peichl, A., and Siegloch, S. (2018). Do higher corporate taxes reduce wages? Micro evidence from Germany. *American Economic Review*, 108(2):393–418.

Garcia-Bernardo, J., Fichtner, J., Takes, F. W., and Heemskerk, E. M. (2017). Uncovering offshore financial centers: Conduits and sinks in the global corporate ownership network. *Nature Scientific Reports*, 7(1):1–10.

Garrett, D. and Suárez Serrato, J. C. (2019). How elastic is the demand for tax havens? evidence from the us possessions corporations tax credit. *AEA Papers and Proceedings*, 109:493–499.

Ge, W., Kim, J.-B., Li, T., and Zhang, J. (2022). Subsidiary operations in offshore financial centers and bank risk-taking: International evidence. *Journal of International Business Studies*, 53:1–34.

Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics*, 225(2):254–277.

Gravelle, J. G. (2015). Tax havens: International tax avoidance and evasion. *Congressional Research Service 7-5700*.

Guyton, J., Langetieg, P., Reck, D., Risch, M., and Zucman, G. (2023). Tax evasion at the top of the income distribution: Theory and evidence. *NBER Working Paper No. 28542*.

Hanlon, M., Maydew, E. L., and Thornock, J. R. (2015). Taking the long way home: US tax evasion and offshore investments in US equity and debt markets. *Journal of Finance*, 70(1):257–287.

Hardy, B., McGuire, P., and von Peter, G. (2024). International finance through the lens of BIS statistics: Bank exposures and country risk. *BIS Quarterly Review*, Sep24:69–86.

Heckemeyer, J. H. and Hemmerich, A. K. (2020). Information exchange and tax haven investment in OECD securities markets. *National Tax Journal*, 73(2):291–330.

Henderson, J. V., Storeygard, A., and Weil, D. N. (2012). Measuring economic growth from outer space. *American Economic Review*, 102(2):994–1028.

Hines, J. R. and Rice, E. M. (1994). Fiscal paradise: Foreign tax havens and American business. *Quarterly Journal of Economics*, 109(1):149–182.

Ishizawa, O. A., Miranda, J. J., and Strobl, E. (2019). The impact of hurricane strikes on short-term local economic activity: Evidence from nightlight images in the Dominican Republic. *International Journal of Disaster Risk Science*, 10:362–370.

Johannesen, N. (2014). Tax evasion and Swiss bank deposits. *Journal of Public Economics*, 111:46–62.

Johannesen, N., Langetieg, P., Reck, D., Risch, M., and Slemrod, J. (2020). Taxing hidden wealth: The consequences of US enforcement initiatives on evasive foreign accounts. *American Economic Journal: Economic Policy*, 12(3):312–346.

Johannesen, N. and Larsen, D. T. (2016). The power of financial transparency: An event study of country-by-country reporting standards. *Economics Letters*, 145:120–122.

Johannesen, N., Reck, D., Risch, M., Slemrod, J., Guyton, J., and Langetieg, P. (2024). The offshore world according to FATCA: New evidence on the foreign wealth of U.S. households. *Tax Policy and the Economy*, 38:61–99.

Johannesen, N. and Zucman, G. (2014). The end of bank secrecy? An evaluation of the G20 tax haven crackdown. *American Economic Journal: Economic Policy*, 6(1):65–91.

Johansson, Å., Skeie, Ø. B., Sorbe, S., and Menon, C. (2017). Tax planning by multinational firms: Firm-level evidence from a cross-country database. *OECD Economics Department Working Papers*, 1355.

Kishore, N., Marqués, D., Mahmud, A., Kiang, M. V., Rodriguez, I., Fuller, A., Ebner, P., Sorensen, C., Racy, F., Lemery, J., Maas, L., Leaning, J., Irizarry, R. A., Balsari, S., and Buckee, C. O. (2018). Mortality in Puerto Rico after hurricane Maria. *New England Journal of Medicine*, 379(2):162–170.

Koetter, M., Noth, F., and Rehbein, O. (2020). Borrowers under water! Rare disasters, regional banks, and recovery lending. *Journal of Financial Intermediation*, 43:100811.

Kothari, S. P. and Warner, J. B. (2007). Econometrics of event studies. In *Handbook of empirical corporate finance*, pages 3–36. Elsevier.

Kruttli, M. S., Tran, B. R., and Watugala, S. W. (2025). Pricing poseidon: Extreme weather uncertainty and firm return dynamics. *The Journal of Finance*, 80(2):783–832.

Langenmayr, D. (2017). Voluntary disclosure of evaded taxes – Increasing revenue, or increasing incentives to evade? *Journal of Public Economics*, 151:110–125.

Langenmayr, D. and Reiter, F. (2022). Trading offshore: Evidence on banks' tax avoidance. *Scandinavian Journal of Economics*, 124(3):797–837.

Langenmayr, D. and Zyska, L. (2023). Escaping the exchange of information: Tax evasion via citizenship-by-investment. *Journal of Public Economics*, 221:104865.

Leenders, W., Lejour, A., Rabaté, S., and Van't Riet, M. (2023). Offshore tax evasion and wealth inequality: Evidence from a tax amnesty in the Netherlands. *Journal of Public Economics*, 217:104785.

Lejour, A. and Schindler, D. (2024). *Research Handbook on the Economics of Tax Havens*. Edward Elgar Publishing.

Londoño-Vélez, J. and Ávila-Mahecha, J. (2021). Enforcing wealth taxes in the developing world: Quasi-experimental evidence from Colombia. *American Economic Review: Insights*, 3(2):131–148.

Menkhoff, L. and Miethe, J. (2019). Tax evasion in new disguise? Examining tax havens' international bank deposits. *Journal of Public Economics*, 176:53–78.

Mohan, P. and Strobl, E. (2017). The short-term economic impact of tropical cyclone Pam: An analysis using VIIRS nightlight satellite imagery. *International Journal of Remote Sensing*, 38(21):5992–6006.

OECD (2000). *Towards global tax cooperation. Progress in identifying and eliminating harmful tax practices*. OECD publishing: Paris.

Omartian, J. D. (2017). Do banks aid and abet asset concealment: Evidence from the Panama Papers. *Unpublished Working Paper*.

Riedel, N. (2018). Quantifying international tax avoidance: A review of the academic literature. *Review of Economics*, 2(69):169–181.

Schmidheiny, K. and Siegloch, S. (2023). On event studies and distributed-lags in two-way fixed effects models: Identification, equivalence, and generalization. *Journal of Applied Econometrics*, 38(5):695–713.

Slemrod, J. (2019). Tax compliance and enforcement. *Journal of Economic Literature*, 57(4):904–54.

Slemrod, J. and Wilson, J. D. (2009). Tax competition with parasitic tax havens. *Journal of Public Economics*, 93(11):1261–1270.

Stantcheva, S. (2021). Understanding tax policy: How do people reason? *The Quarterly Journal of Economics*, 136(4):2309–2369.

Strobl, E. (2011). The economic growth impact of hurricanes: Evidence from US coastal counties. *Review of Economics and Statistics*, 93(2):575–589.

Strobl, E. (2012). The economic growth impact of natural disasters in developing countries: Evidence from hurricane strikes in the Central American and Caribbean regions. *Journal of Development Economics*, 97(1):130–141.

Suárez Serrato, J. C. (2018). Unintended consequences of eliminating tax havens. *NBER Working Paper No. 24850*.

Sun, L. and Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, 225(2):175–199.

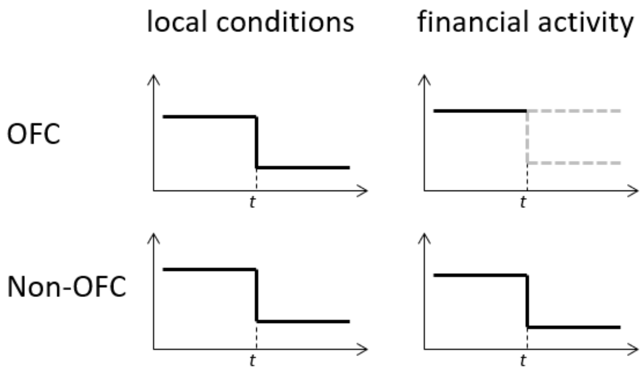
Tørsløv, T., Wier, L., and Zucman, G. (2023). The missing profits of nations. *Review of Economic Studies*, 90(3):1499–1534.

Zucman, G. (2013). The missing wealth of nations: Are Europe and the U.S. net debtors or net creditors? *Quarterly Journal of Economics*, 128(3):1321–1364.

Zucman, G. (2014). Taxing across borders: Tracking personal wealth and corporate profits. *Journal of Economic Perspectives*, 28(4):121–148.

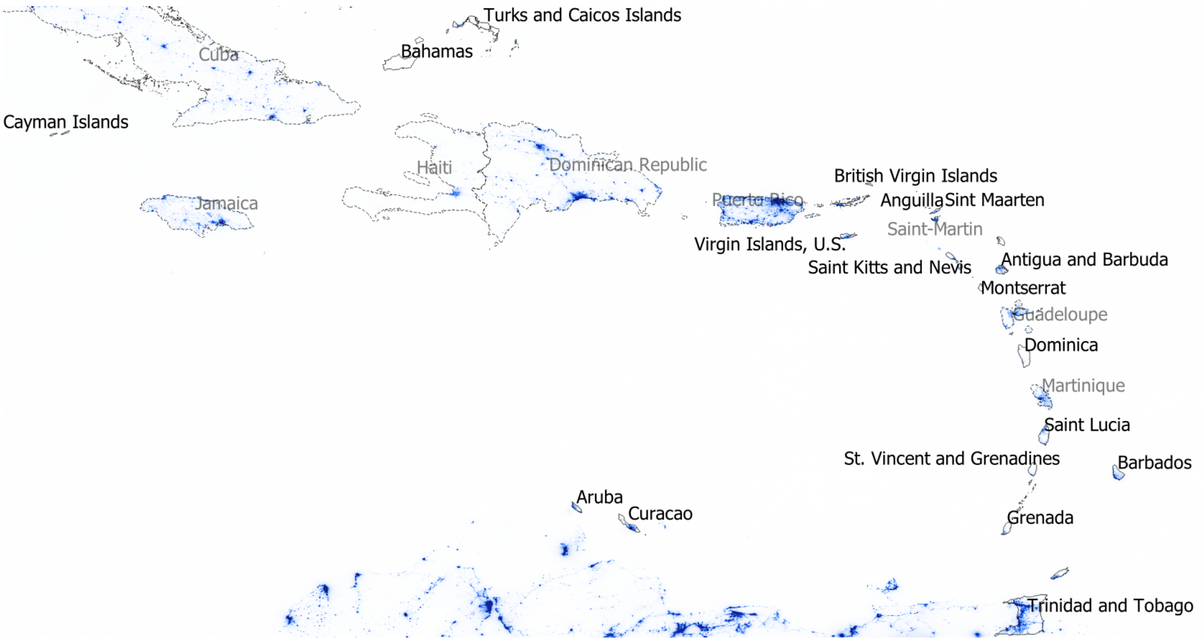
Tables and Figures

Figure 1: Schematic reactions to hurricane impacts



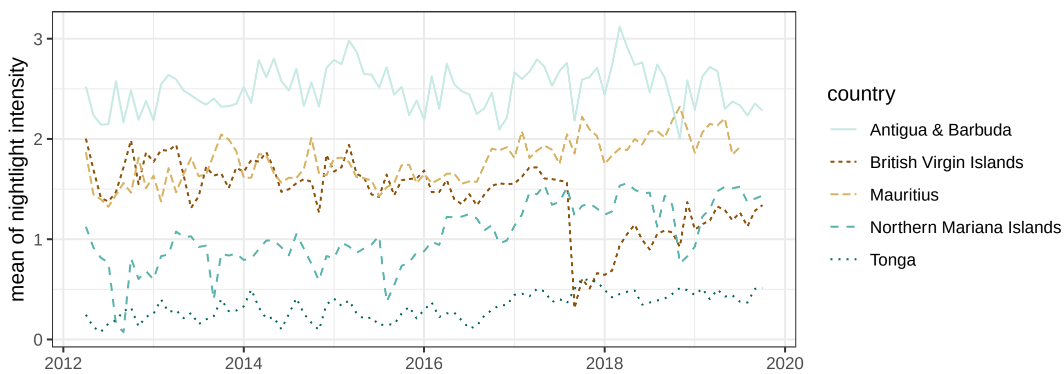
Notes: Hypothetical reactions to hurricane impact at time t on the horizontal axis for offshore financial centers (OFCs, top panel) and non-OFCs (bottom panel).

Figure 2: Nightlights and jurisdictions in the Caribbean



Notes: Shows VIIRS nightlights and administrative boundaries of Caribbean island economies. Offshore financial centers are shown with black labels and solid boundaries, non-OFCs with grey labels and dashed lines. Blue areas without borders are northern areas of South America. Radiance of nightlight is measured in units of $W sr^{-1} cm^{-2}$, or watt per steradian per square centimeter multiplied by 10^9. Data sources: NASA, GADM, NOAA.

Figure 3: Monthly nightlight intensity for selected jurisdictions



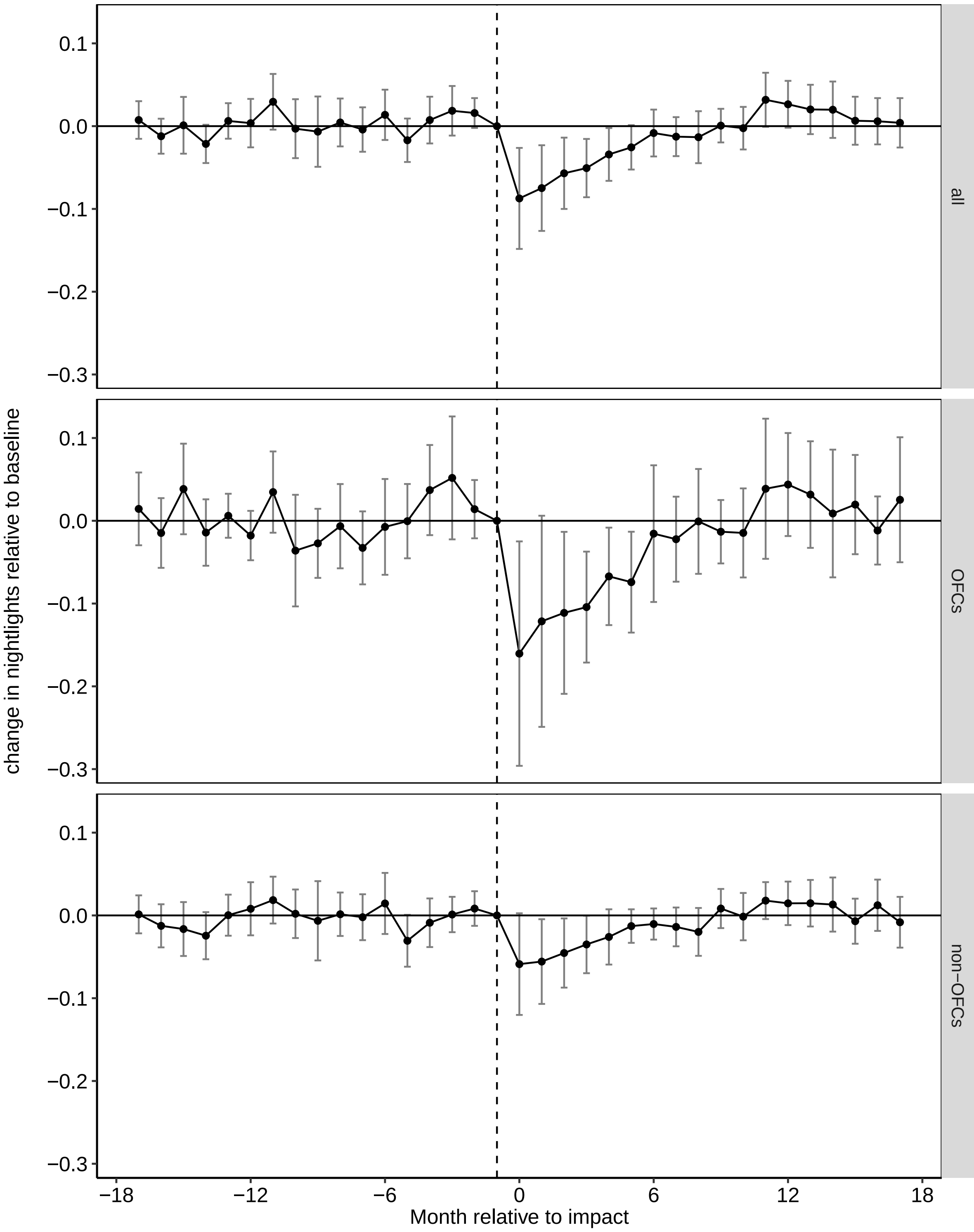
Notes: Shows time series of the mean of VIIRS nightlights starting in April 2012, when data becomes available, for selected countries in the Caribbean and the Pacific & Indian oceans. The series show the mean of nightlight intensity (vertical axis) plotted over the year-month where the images were taken (horizontal axis). Data sources: NASA, GADM, NOAA, authors' calculations.

Table 1: Sample of island economies

	population	GDP p.C.	bn. mirror claims 2018q1	mean(light)	hurricanes after 2012q2	OFC
	(1)	(2)	(3)	(4)	(5)	(6)
American Samoa	49,437	11,200		1.444	0	0
Anguilla	17,087	12,200		3.039	1	1
Antigua & Barbuda	94,731	26,300		2.500	1	1
Aruba	115,120	25,300	0.384	6.038	0	1
Bahamas	329,988	25,100	57.480	0.559	6	1
Barbados	293,131	18,600	20.580	4.577	1	1
Bermuda	70,864	85,700	65.970	6.510	0	1
British Virgin Islands	35,015	42,300		1.439	1	1
Caribbean Netherlands	26,220			1.020	0	0
Cayman Islands	58,441	43,800	1,602.000	4.813	0	1
Christmas Island	2,205			0.530	0	0
Cocos (Keeling) Islands	596			0.174	0	0
Comoros	846,281	1,600	0.014	0.116	2	0
Cuba	11,059,062	12,300	0.538	0.447	8	0
Curaçao	149,648	15,000	17.470	6.166	0	1
Dominica	73,897	12,000	0.033	0.337	2	1
Dominican Republic	10,734,247	17,000	2.545	0.890	7	0
Fiji	920,938	9,900	0.376	0.138	4	0
Grenada	111,724	14,700	0.008	1.222	0	1
Guadeloupe	397,990			2.610	1	0
Guam	167,772	35,600		4.829	0	0
Haiti	10,646,714	1,800	0.197	0.189	7	0
Jamaica	2,990,561	9,200	0.814	1.287	4	0
Maldives	391,904	18,600	0.165	1.806	0	1
Martinique	380,877	27,305		3.718	1	0
Mauritius	1,379,365	21,500	13.510	1.782	1	1
Mayotte	272,730			1.449	0	0
Montserrat	5,292	8,500		0.289	0	1
Nauru	11,359	12,200	0.0005	3.426	0	1
New Caledonia	279,070	31,100	5.309	0.212	0	0
Niue	2,000			0.090	0	1
Norfolk Island	1,748			0.108	0	0
Northern Mariana Islands	51,994	24,500		1.051	2	0
Palau	21,516	14,700		0.220	2	0
Pitcairn Islands	50			0.099	0	0
Puerto Rico	3,351,827	37,900		4.470	3	0
Réunion	895,231			1.997	2	0
Saint Martin (French part)	32,556			5.499	1	0
Samoa	203,774	5,700	4.172	0.114	2	1
Seychelles	95,981	27,800	2.154	0.691	1	1
Sint Maarten	43,847	66,800		12.520	1	1
Solomon Islands	647,581	2,200	0.052	0.049	3	0
Sri Lanka	22,889,201	12,600	1.767	0.396	3	0
St. Barthélemy	7,122			2.354	1	0
St. Kitts & Nevis	52,715	26,800		2.293	1	1
St. Lucia	164,994	26,800	0.026	1.877	1	1
St. Vincent & Grenadines	102,089	11,600	0.409	0.783	1	1
Taiwan	23,603,049	49,100	53.010	4.385	8	0
Tokelau	1,647			0.039	0	0
Tonga	106,095	5,900	0.028	0.332	4	1
Trinidad & Tobago	1,218,208	31,200	1.266	6.254	0	1
Turks & Caicos Islands	52,570	29,100	0.197	0.730	1	1
Tuvalu	11,052	3,700		0.152	1	0
U.S. Virgin Islands	107,268	36,100		5.670	1	1
Vanuatu	288,037	2,700	0.132	0.333	3	1
Wallis & Futuna	15,854		0.026	0.132	1	0

Notes: Shows data on island economies in the Caribbean and the Pacific & Indian Oceans. Population and GDP per capita are taken from the CIA World Factbook estimates (Jul. 2017, where available). Column 3 shows the sum of international claims (in billion USD) reported on sample islands by non-OFCs that provide data to the BIS locational banking statistics in 2012q2 or earlier. Column 4 shows means of nightlight intensity over the sample period. Column 5 shows the number of hurricanes after 2012q2 and column 6 indicates if the jurisdiction is classified as an OFC or not based on the unions of the lists in Johannesen and Zucman (2014) and Gravelle (2015).

Figure 4: Hurricane impacts on nightlights



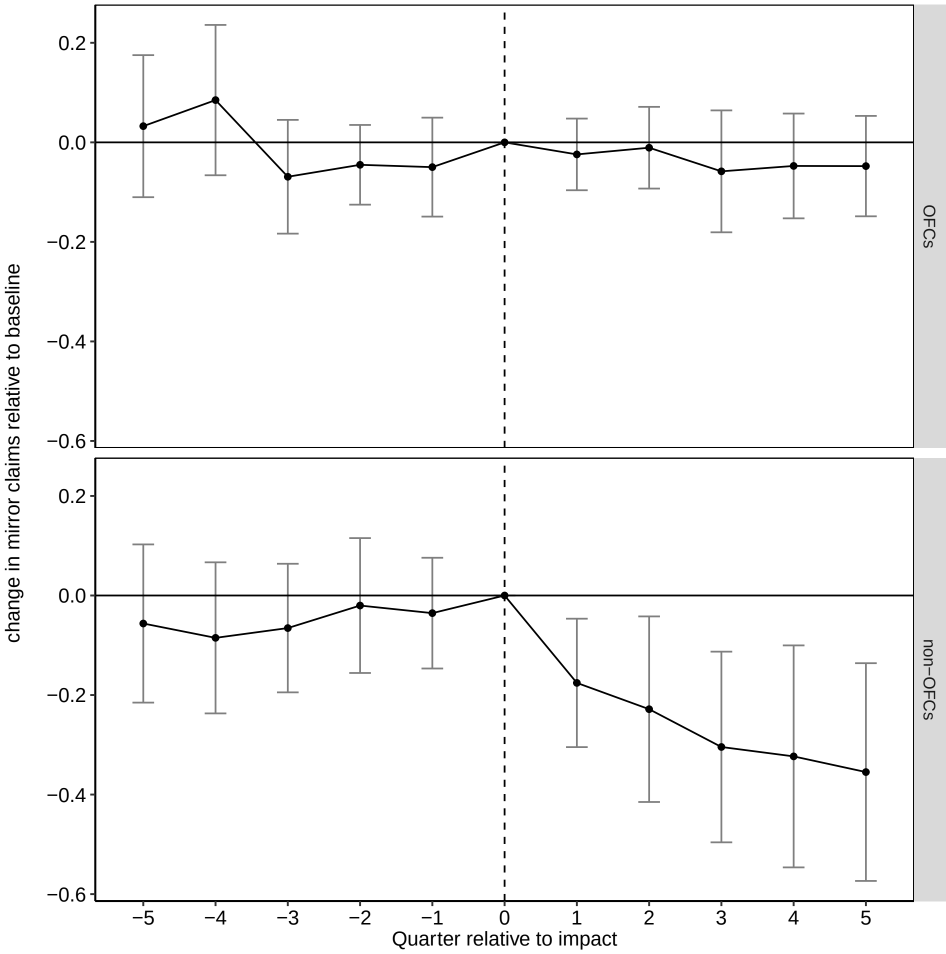
Notes: Plots coefficients of the multiple event study with binned endpoints outlined in the main text for monthly nighlight data. The top panel shows the entire sample, the second and third panels split this sample into OFC and non-OFC islands respectively. The baseline dummy left out of the regression is the month before the hurricane ($j = -1$) and 95% confidence intervals are plotted in grey, based on heteroskedasticity and autocorrelation robust standard errors clustered at the country level.

Table 2: Quantifying nightlight impacts

	Dependent variable:			
	log(nightlight intensity)		i.h.s.(nightlight intensity)	
	OFCs	non-OFCs	OFCs	non-OFCs
	(1)	(2)	(3)	(4)
$\text{hurricane}_{j=0:j=8}$	-0.194^{***}	-0.180^{***}	-0.098^{**}	-0.073^{***}
	(0.068)	(0.068)	(0.041)	(0.028)
$\text{Bin}_{j=\underline{j}:j=-9}$	0.003	0.067	0.014	-0.005
	(0.049)	(0.041)	(0.017)	(0.007)
$\text{Bin}_{j=9:j=\bar{j}}$	0.081	0.049	0.023	0.005
	(0.055)	(0.048)	(0.021)	(0.006)
country f.e.	Yes	Yes	Yes	Yes
year-qtr f.e.	Yes	Yes	Yes	Yes
Observations	2,145	2,189	2,187	2,349
R^2	0.172	0.280	0.175	0.279

Notes: Shows results of a difference-in-difference exercise with a dummy ($\text{hurricane}_{j=0:j=9}$) taking value 1 if there was a hurricane in the last nine year-months. Results are reported split-sample, first showing the OFC part of the sample, then the non-OFC part of the sample. Columns 1 and 2 show results using the log of nightlight intensity, columns 3 and 4 using the inverse hyperbolic sine transformations. *p<0.1; **p<0.05; ***p<0.01 based on heteroskedasticity and autocorrelation robust standard errors clustered at the country level.

Figure 5: Hurricane impacts on the financial sector



Notes: Plots coefficients of the multiple event study with binned endpoints outlined in the main text for quarterly data. The baseline dummy left out of the regression is the quarter of the hurricane ($j = 0$) and 95% confidence intervals are plotted in grey, based on heteroskedasticity and autocorrelation robust standard errors clustered at the country level. The top panel plots the OFC part of the sample, the bottom panel plots the non-OFC part of the sample.

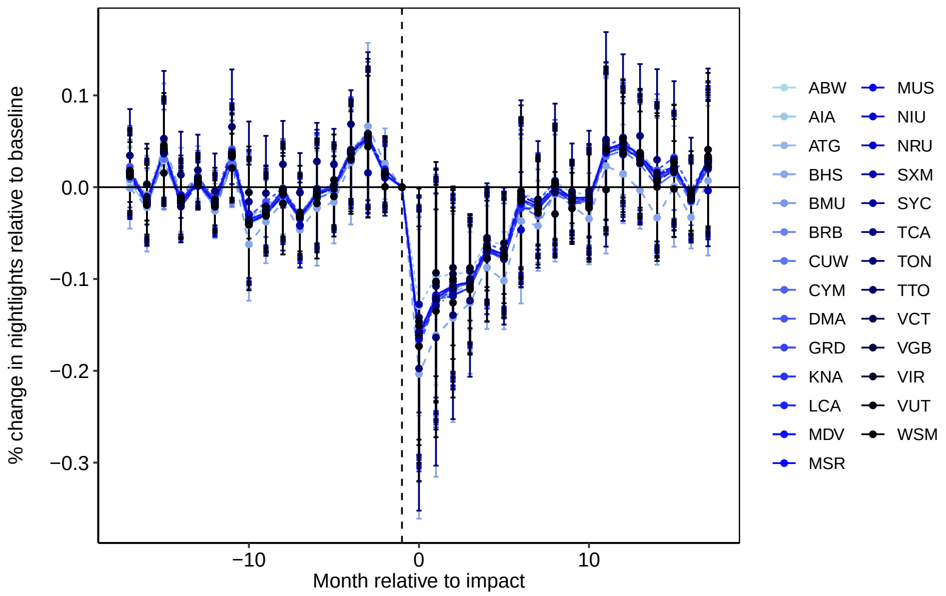
Table 3: Further results using BIS data

dependent var.:	i.h.s.(mirror claims)			log(mirror claims)		i.h.s.(mirror liabs)	
sample:	OFCs (1)	non-OFCs (2)	all (3)	OFCs (4)	non-OFCs (5)	OFCs (6)	non-OFCs (7)
$\text{hurricane}_{j=1:j=5}$	0.006	-0.336^{***}		0.061	-0.337^{***}	0.125^{*}	0.086
	(0.057)	(0.083)		(0.089)	(0.084)	(0.075)	(0.064)
$\text{hurricane}_{j=1:j=5}$ OFC			-0.077				
			(0.076)				
$\text{hurricane}_{j=1:j=5}$ non-OFC			-0.210^{**}				
			(0.087)				
$\text{Bin}_{j=\underline{j}:j=-6}$	0.056	-0.098	-0.066	0.026	-0.099	0.161	0.031
	(0.086)	(0.082)	(0.055)	(0.095)	(0.083)	(0.107)	(0.050)
$\text{Bin}_{j=6:j=\bar{j}}$	-0.072	-0.174^{**}	-0.074	-0.008	-0.175^{**}	0.088	0.056
	(0.077)	(0.078)	(0.066)	(0.108)	(0.079)	(0.067)	(0.045)
country f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
year-qtr f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	548	317	865	548	317	548	344
R^2	0.243	0.212	0.132	0.186	0.212	0.061	0.098

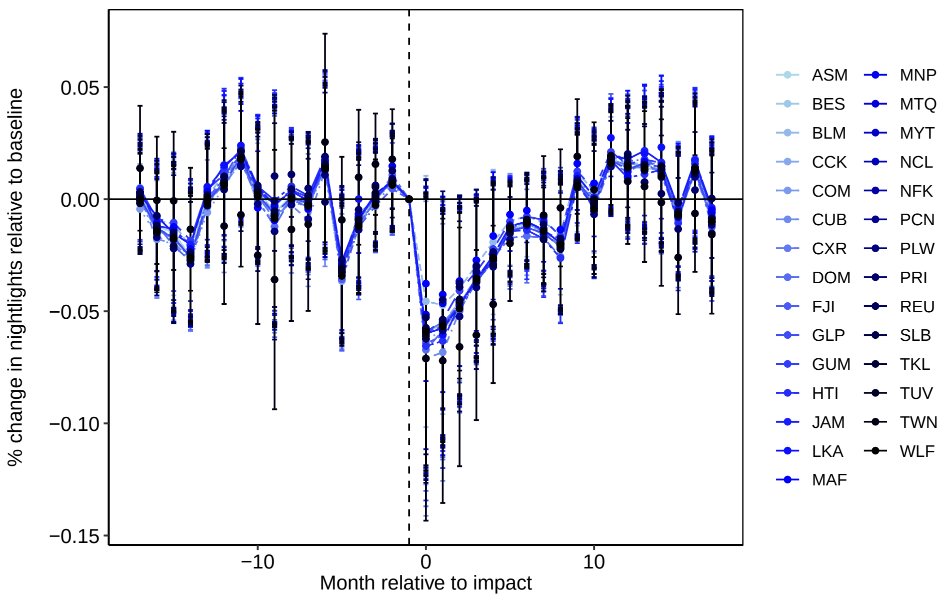
Notes: Shows results of a difference in difference exercise with a dummy ($\text{hurricane}_{j=1:j=6}$) taking value 1 if there was a hurricane in the last six year-quarters. Columns 1 to 3 show results on mirror claims for OFCs (1) and non-OFCs (2), as well as the entire sample (3) with an interaction term. Columns 4 and 5 repeat columns 1 and 2 but with log transformations instead of inverse hyperbolic sine transformations. Columns 6 and 7 report a falsification exercise showing results on all liabilities reported against islands in the sample. *p<0.1; **p<0.05; ***p<0.01 based on heteroskedasticity and autocorrelation robust standard errors clustered at the country level.

Figure 6: Sample robustness of nightlight results: Excluding each country in turn

(a) OFCs



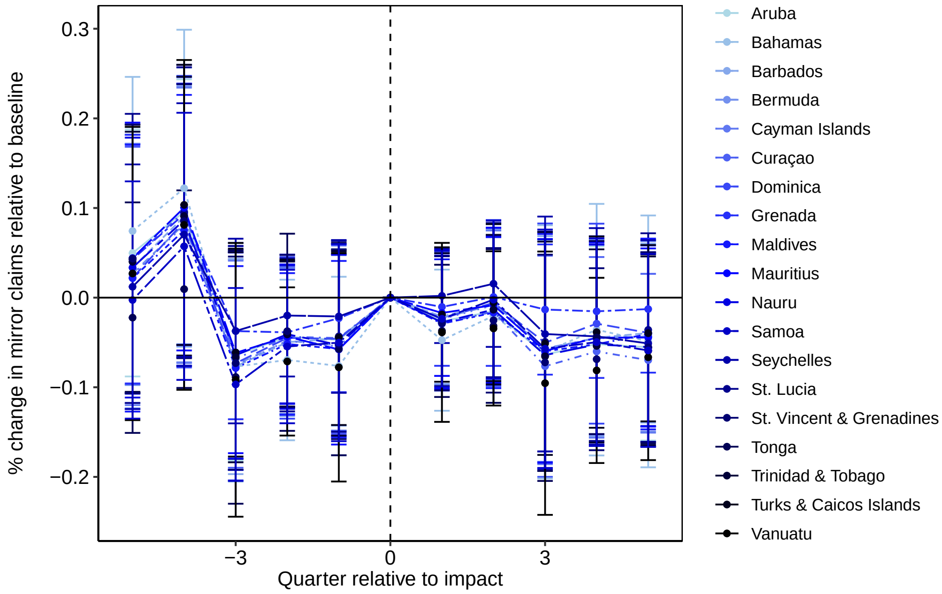
(b) non-OFCs



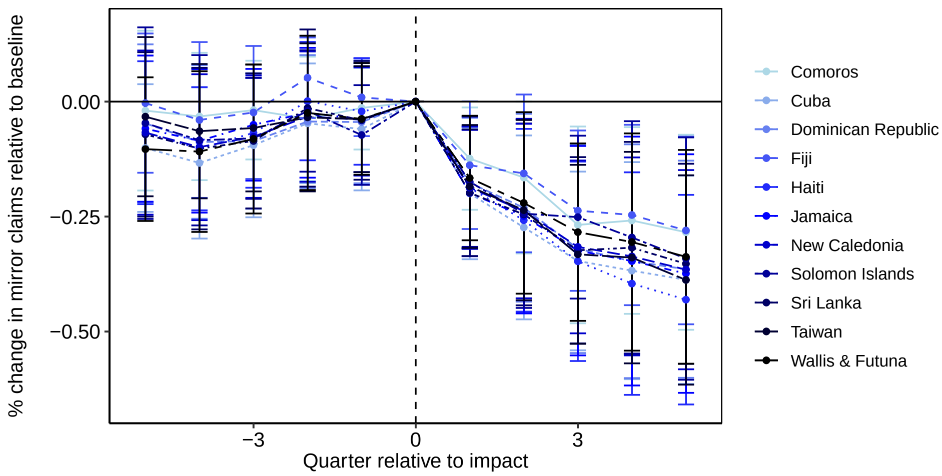
Notes: Plots coefficients of the multiple event study with binned endpoints outlined in the main text for monthly data. The baseline dummy left out of the regression is the month before the hurricane ($j = -1$) and 95% confidence intervals are plotted based on heteroskedasticity and autocorrelation robust standard errors clustered at the country level. The top panel shows results for the OFC part of the sample, the bottom panel for the non-OFC part. In both sub-samples, each country is excluded in turn and results for the rest of the sub-sample are plotted.

Figure 7: Sample robustness of mirror claim results: Excluding each country in turn

(a) OFCs

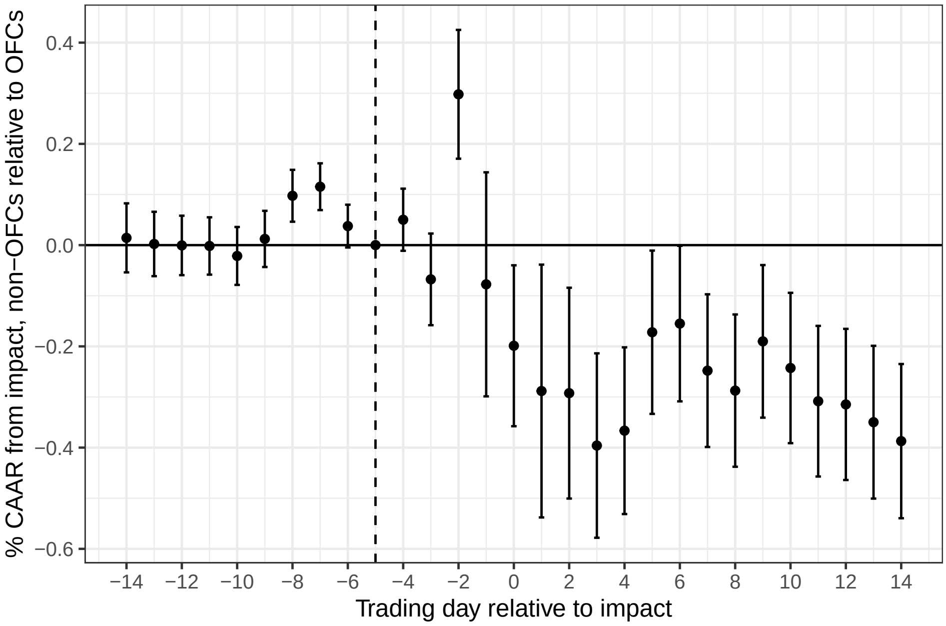


(b) non-OFCs



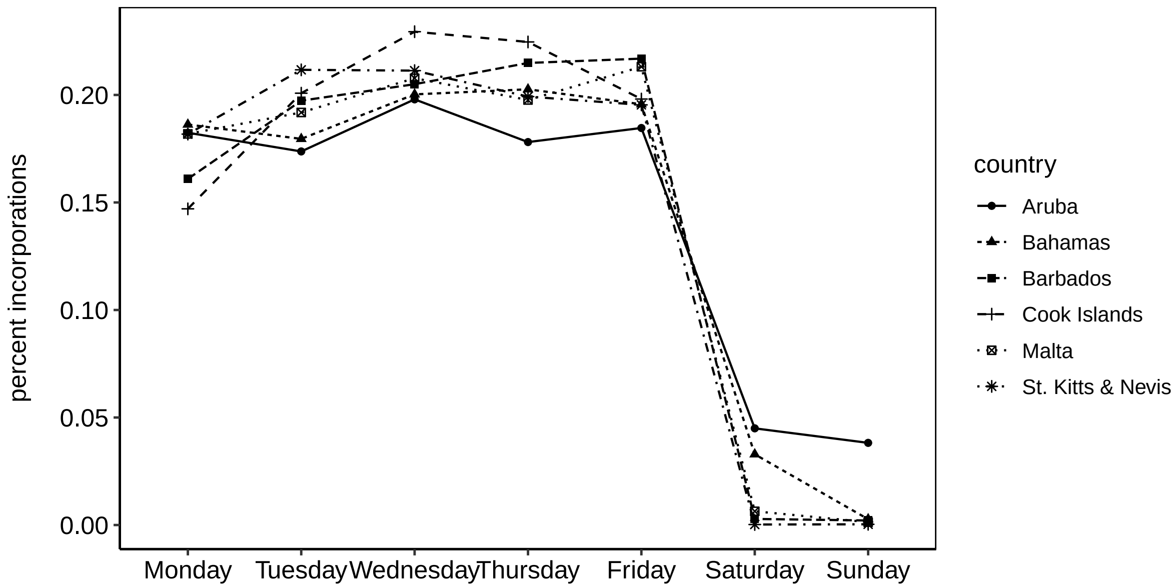
Notes: Plots coefficients of the multiple event study with binned endpoints outlined in the main text for quarterly data. The baseline dummy left out of the regression is the quarter of the hurricane ($j = 0$) and 95% confidence intervals are plotted based on heteroskedasticity and autocorrelation robust standard errors clustered at the country level. The top panel shows results for the OFC part of the sample, the bottom panel for the non-OFC part.

Figure 8: Hurricane impacts as perceived by international investors



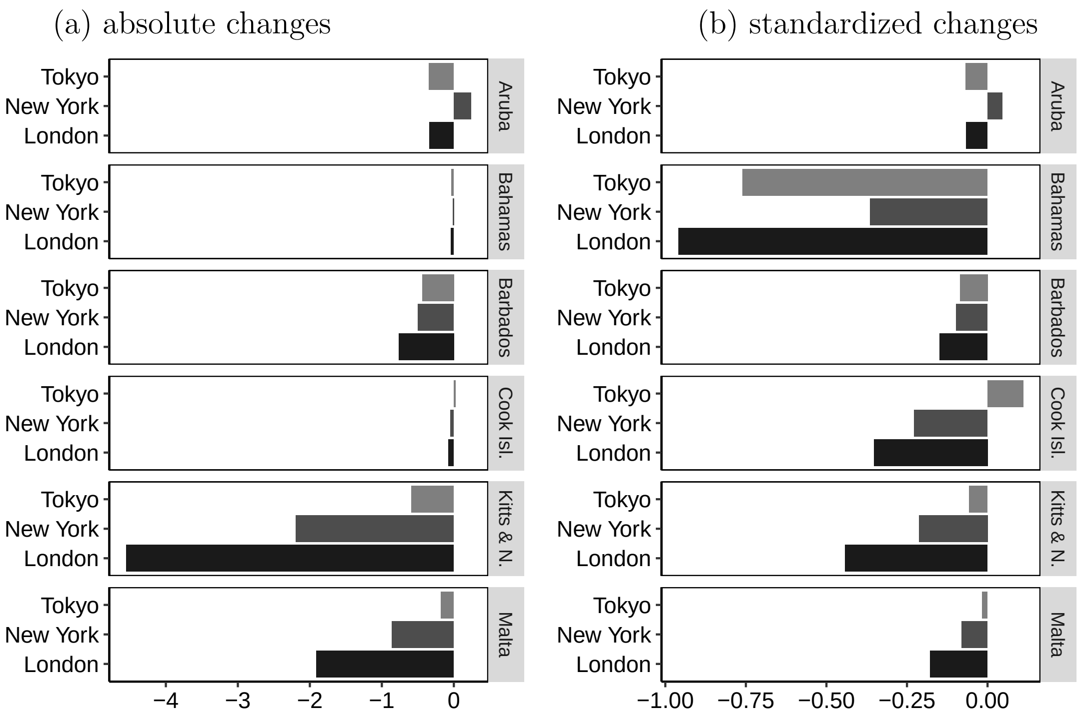
Notes: This figure shows the results of the event study on cumulative abnormal returns. It shows a one month trading window around hurricane impacts and directly compares the abnormal returns of equity prices of firms on affected non-OFCs to those on OFCs. The left out category is the fifth day prior to impact and 95% confidence bands based on heteroskedasticity and autocorrelation robust standard errors are plotted in black errorbars.

Figure 9: Incorporations per weekday



Notes: Shows percentage of daily incorporations by weekday for the six OFCs for which corporate registries were leaked.

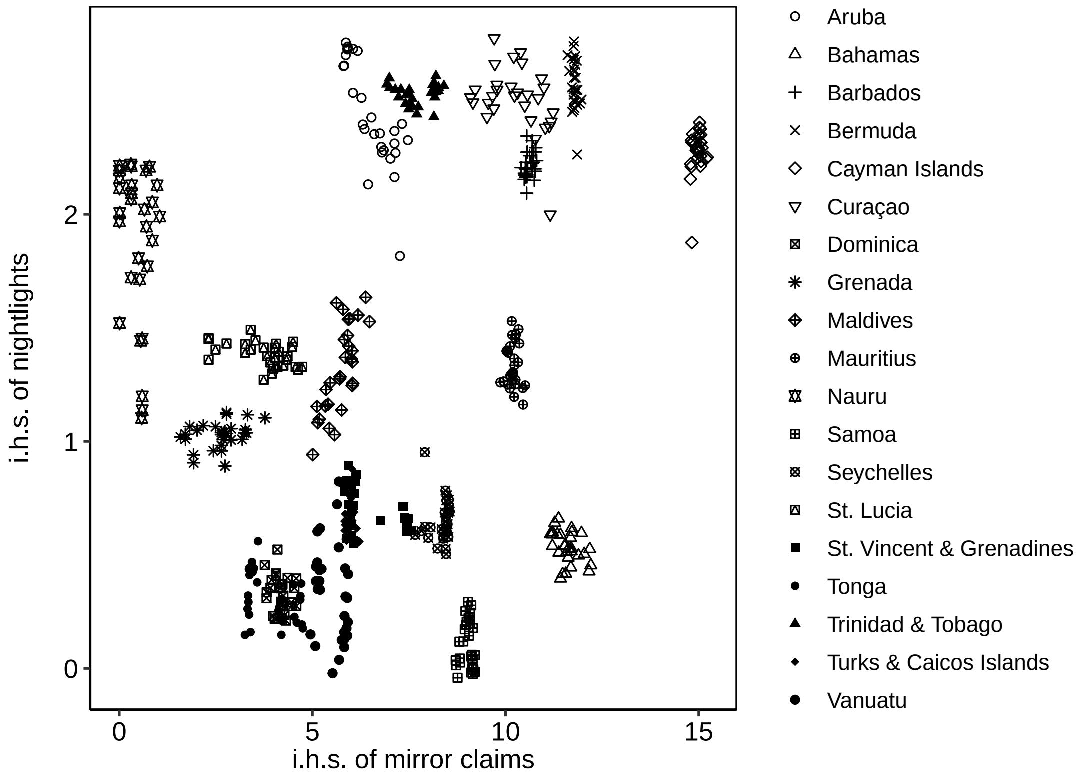
Figure 10: Changes in incorporations on foreign public holidays



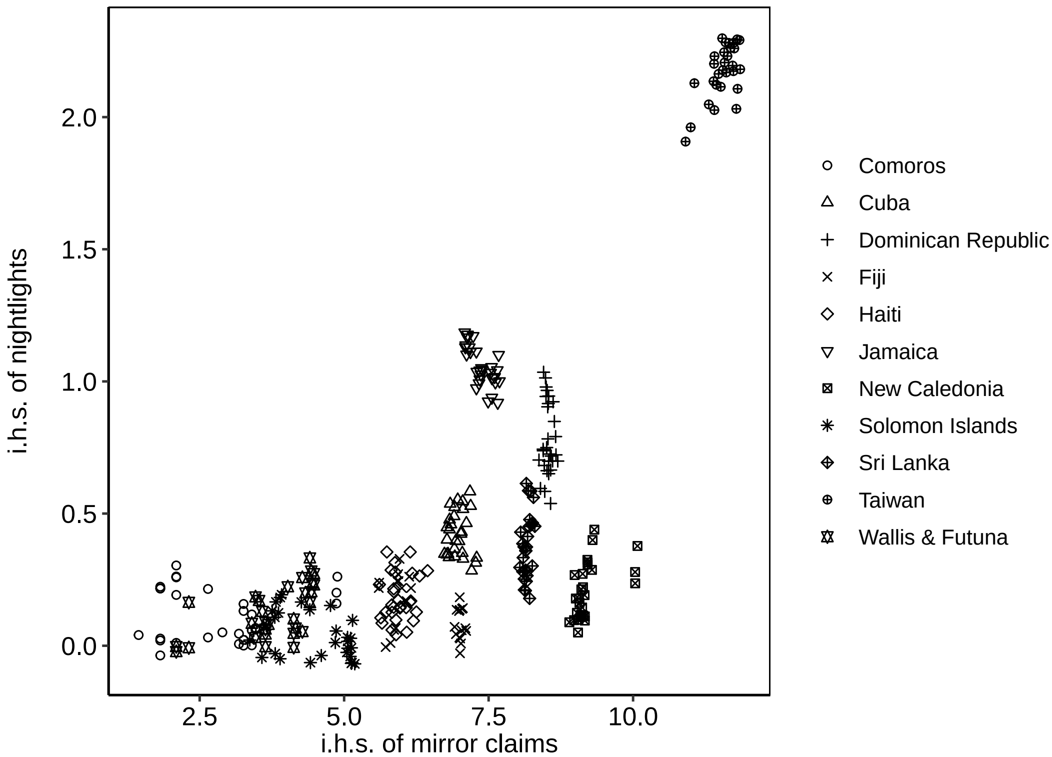
Notes: Shows absolute (panel a) and standardized (panel b) changes in incorporations relative to the average incorporation activity excluding weekends and local holidays. Data on public holidays is publicly available and used starting with 1990. Foreign holidays are public holidays in London, New York, and Tokyo that are normal workdays on the six islands plotted.

Figure 11: Direct correlations of nightlights and mirror claims

(a) OFC sample



(b) Non-OFC sample



Notes: Both figures plot the inverse hyperbolic sine of nightlights aggregated by quarter over the inverse hyperbolic sine of mirror claims reported by non-OFC economies. The sample is limited by the availability of offshore mirror claims. Panel (a) shows the OFC part of the sample, panel (b) shows the non-offshore part of the sample.

Online Appendix
to accompany
“The elusive banker: Using hurricanes to uncover (non-)activity in offshore financial centers”

Jakob Miethe

Includes:

A.1. Data

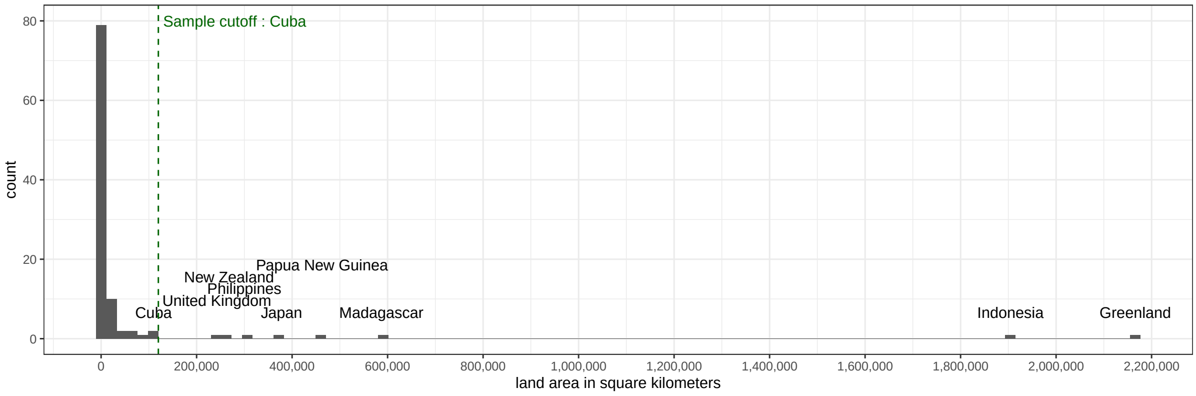
A.1.1. Sample Choices

This Annex provides further information about the sample choices used in the main text. There are 104 island jurisdictions on the planet, ranging from military atolls such as the Spratly Islands to the United Kingdom and Greenland. To reduce this sample to one suitable for identification using hurricanes, first, only islands that are located in the Caribbean, the Pacific Ocean, and the Indian Ocean are used, where hurricanes can be expected.[1] Then, islands that exhibit one of the following characteristics are excluded from the sample: The jurisdiction does not have an iso3 code and thus no geospatial data,[2] it is landlocked (including to a larger island),[3] or it is uninhabited/a pure military base.[4] Next, two choices concerning large islands and island groups are needed. Indonesia, for example, is so large that a hurricane hitting it does not necessarily show up in national data. Fortunately, the area distribution of island economies has a clear cut-off point (see Figure A.1.1) with no island of an area between 109,238 square kilometers (Cuba) and more than double that size: 244,820 square kilometers (The United Kingdom). The sample is therefore cut at Cuba, dropping larger islands.[5] Finally, island groups that spread out over large spans of water are dropped. This choice is based on exclusive economic zones that include the water area between islands of the same island group. Again, there is a natural cut-off point (see Figure A.1.2) between the Solomon Islands (the largest island group still included with 1.5 million square kilometers of exclusive economic zone) and the Cook Islands (2 million square kilometers). After the decision rule on land area, this choice only excludes some very spread out island groups.[6]

Table A.1.1 shows all islands on the planet in the first column. If available, the second

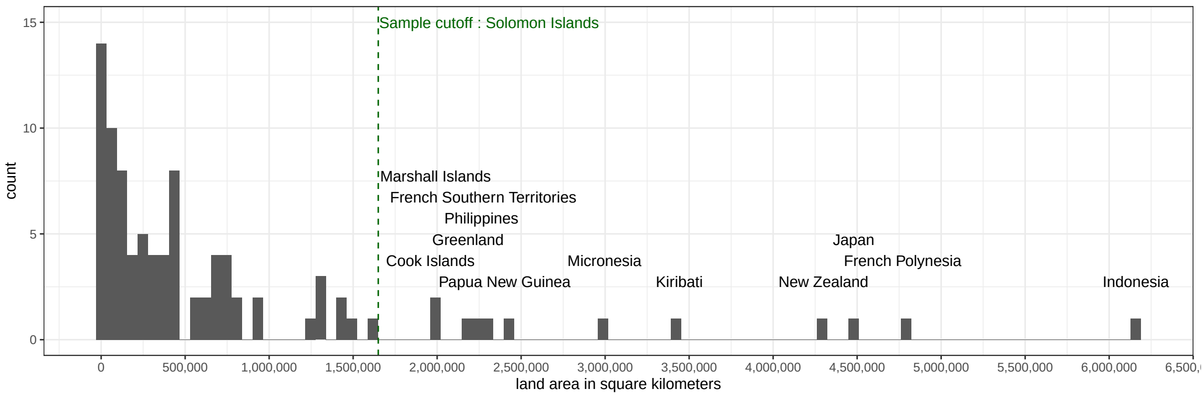
[1] This excludes islands such as Cyprus and Malta, or the Falkland Islands.
[2] This excludes islands such as the Easter Island or the Azores.
[3] This excludes countries like Brunei, Papua New Guinea, and East Timor.
[4] This excludes jurisdictions like the British Indian Ocean Territory, or the Spratly Islands.
[5] This excludes countries like New Zealand, Madagascar, or Japan.
[6] These are Micronesia, Kiribati, the French Southern Territories, the Marshall Islands, and the Cook Islands.

Figure A.1.1: Distribution of Island area and cutoff point



Notes: On the vertical axis, the histogram counts the number of islands of the area shown on the horizontal axis. Islands larger than Cuba are named in the graph. The vertical green dashed line shows the cutoff point at Cuba (109,238 square kilometers) which is the largest island in the sample. The next biggest island is the United Kingdom with 244,820 square kilometers.

Figure A.1.2: Distribution of Exclusive Economic Zones and cutoff point



Notes: On the vertical axis, the histogram counts the number of islands of the EEZ size shown on the horizontal axis. Islands with an EEZ larger than that of the Solomon Islands are named in the graph. The vertical green dashed line shows the cutoff point at the Solomon Islands (1,589,477 square kilometers) which is the largest EEZ island in the sample. The next biggest EEZ is that of the Cook Islands with 1,960,027 square kilometers.

column shows the iso3 character code accepted by the United Nations, followed by the area in km^2. The next seven columns show reasons for exclusion from the sample indicating if the island is not in hurricane prone oceans (column 4), has no iso3 code recognized by the UN (column 5), is landlocked (column 6), uninhabited (column 7), larger than Cuba (column 8 with details of land area shown in figure A.1.1), or has a larger exclusive economic zone (EEZ) than the Solomon Islands (column 9 with details of EEZ distribution of all islands shown in figure A.1.2). The two figures A.1.1 and A.1.2 indicate with dashed lines where the sample was cut. The largest country still included is indicated in green in those figures.

Table A.1.1: Sample Exclusion Choices

country	iso3c	area in km^2	other oceans	no iso3	landlocked	uninhabited	area > Cuba	eez > Solomon Isl.	in sample
American Samoa	ASM	199							1
Anguilla	AIA	91							1
Antigua & Barbuda	ATG	440							1
Aruba	ABW	180							1
Bahamas	BHS	13,878							1
Barbados	BRB	431							1
Bermuda	BMU	53							1
British Virgin Islands	VGB	151							1
Caribbean Netherlands	BES	328							1
Cayman Islands	CYM	259							1
Christmas Island	CXR	135							1
Cocos (Keeling) Islands	CCK	14							1
Comoros	COM	1,659							1
Cuba	CUB	109,884							1
Curaçao	CUW	444							1
Dominica	DMA	750							1
Dominican Republic	DOM	48,442							1
Fiji	FJI	18,333							1
Grenada	GRD	348							1
Guadeloupe	GLP	1,628							1
Guam	GUM	549							1
Haiti	HTI	27,750							1
Jamaica	JAM	10,992							1
Maldives	MDV	298							1

Martinique	MTQ	1,128							1
Mauritius	MUS	1,040							1
Mayotte	MYT	374							1
Montserrat	MSR	201							1
Nauru	NRU	21							1
New Caledonia	NCL	18,575							1
Niue	NIU	261							1
Norfolk Island	NFK	35							1
Northern Mariana Islands	MNP	477							1
Palau	PLW	458							1
Pitcairn Islands	PCN	47							1
Puerto Rico	PRI	13,800							1
Réunion	REU	2,512							1
Saint Martin (French part)	MAF	53							1
Samoa	WSM	2,842							1
Seychelles	SYC	459							1
Sint Maarten	SXM	34							1
Solomon Islands	SLB	28,399							1
Sri Lanka	LKA	65,610							1
St. Barthélemy	BLM	24							1
St. Kitts & Nevis	KNA	261							1
St. Lucia	LCA	617							1
St. Vincent & Grenadines	VCT	389							1
Taiwan	TWN	36,193							1
Tokelau	TKL	10							1
Tonga	TON	747							1
Trinidad & Tobago	TTO	5,131							1
Turks & Caicos Islands	TCA	417							1
Tuvalu	TUV	26							1
U.S. Virgin Islands	VIR	346							1
Vanuatu	VUT	12,199							1
Wallis & Futuna	WLF	142							1
Akrotiri and Dhekelia		254	1	1					
Aland	ALA	1,580	1						
Azores		2,351	1	1					
Bahrain	BHR	750			1				
Baker Island		2		1					
British Indian Ocean Territory	IOT	60				1			
Brunei	BRN	5,765			1				
Canary Islands		7,492	1	1					
Cape Verde	CPV	4,033	1						
Clipperton Island	XCL	9				1			
Cook Islands	COK	236						1	
Cyprus	CYP	9,251	1						
Easter Island		164		1					
Falkland Islands	FLK	12,173	1						
Faroe Islands	FRO	1,399	1						
French Polynesia	PYF	4,167						1	
French Southern Territories	ATF	7,676				1		1	
Greenland	GRL	2,166,086	1				1	1	
Guernsey	GGY	78	1						

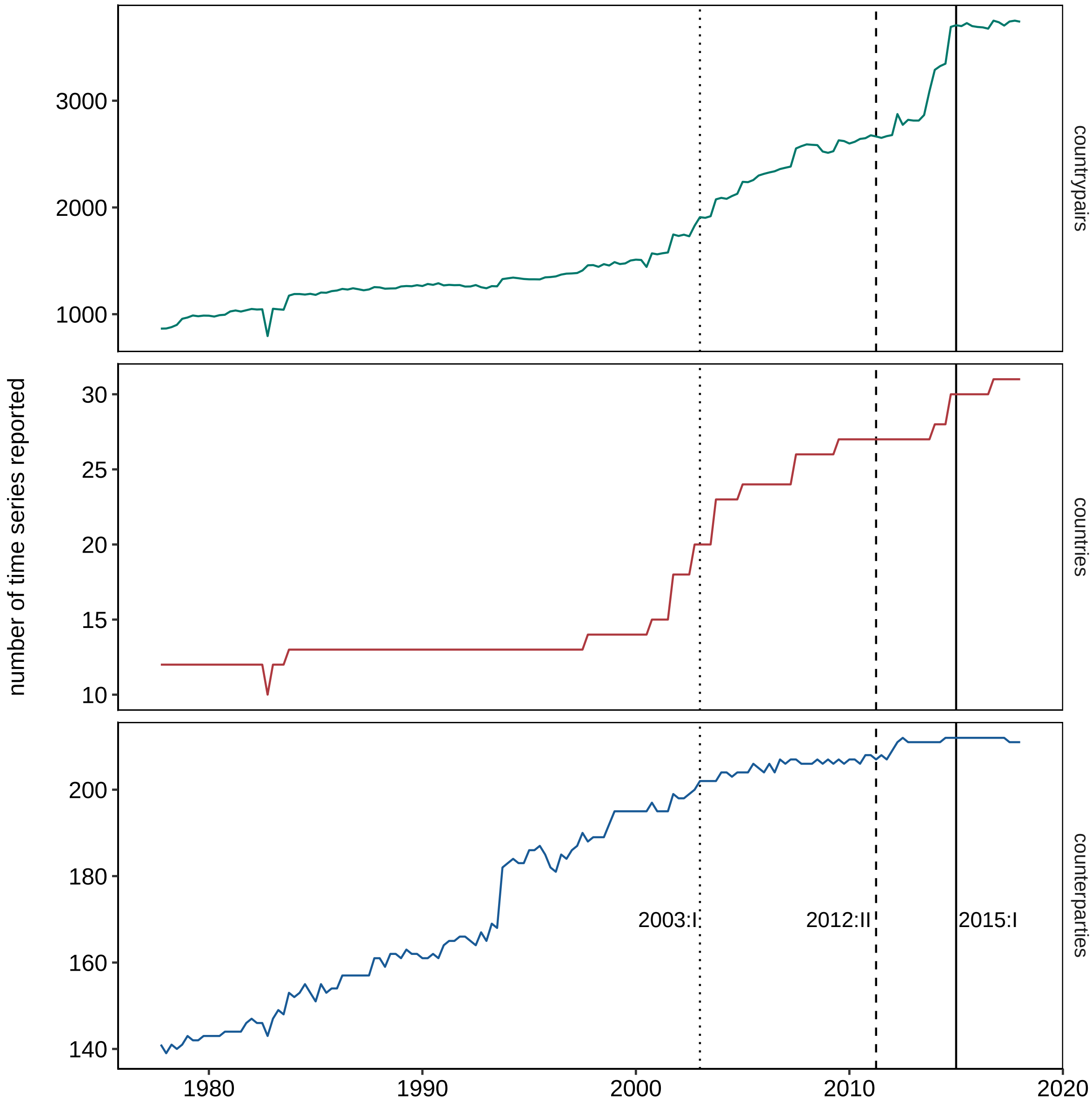
Heard & McDonald Islands	HMD	368				1		
Hong Kong SAR China	HKG	1,106			1			
Iceland	ISL	103,000	1					
Indonesia	IDN	1,904,569					1	1
Ireland	IRL	84,421	1					
Isle of Man	IMN	572	1					
Japan	JPN	377,915					1	1
Jersey	JEY	119	1					
Kiribati	KIR	811						1
Macao SAR China	MAC	115			1			
Madagascar	MDG	587,041					1	
Malta	MLT	316	1					
Marshall Islands	MHL	181						1
Micronesia	FSM	702						1
Navassa Island		5		1		1		
New Zealand	NZL	268,021					1	1
Papua New Guinea	PNG	462,840			1		1	1
Paracel Islands		8		1		1		
Philippines	PHL	300,000					1	1
Singapore	SGP	721			1			
South Georgia & South Sandwich Islands	SGS	3,903	1			1		
Spratly Islands		2		1		1		
St. Helena	SHN	122	1					
St. Pierre & Miquelon	SPM	242	1					
Svalbard & Jan Mayen	SJM	61,359	1					
São Tomé & Príncipe	STP	1,001	1					
Timor-Leste	TLS	15,006			1			
United Kingdom	GBR	242,495	1				1	
United States Minor Outlying Islands (the)	UMI	34				1		

A.1.2. BIS balancing and Descriptives

The Locational Banking Statistics (LBS) used in the main text are derived from aggregated reports of reporting countries against a large number of counterparties. The coverage of this dataset changes along both dimensions. A continuous increase is visible over time as shown in Figure A.1.3. The top panel shows the total number of countrypairs available starting in 1977 with the earliest reports. The middle and bottom panels show the underlying developments on the country and counterparty dimension. The number of countrypairs almost doubles between the earliest balanced series (starting in 2003q1, vertical dotted line) and the data used in the main text (starting in 2012q2, vertical dashed line). However, as shown in Figure A.1.4, this increase neither changes the level nor the time dynamic of total reported mirror claims against one counterparty drastically. The large OECD countries that report the highest positions start reporting early in the sample and the large number of countrypairs where data becomes available late in the sample (the vertical dashed line in Figure A.1.3 shows the panel available for balancing in 2015q1) report relatively small positions that follow similar trends.

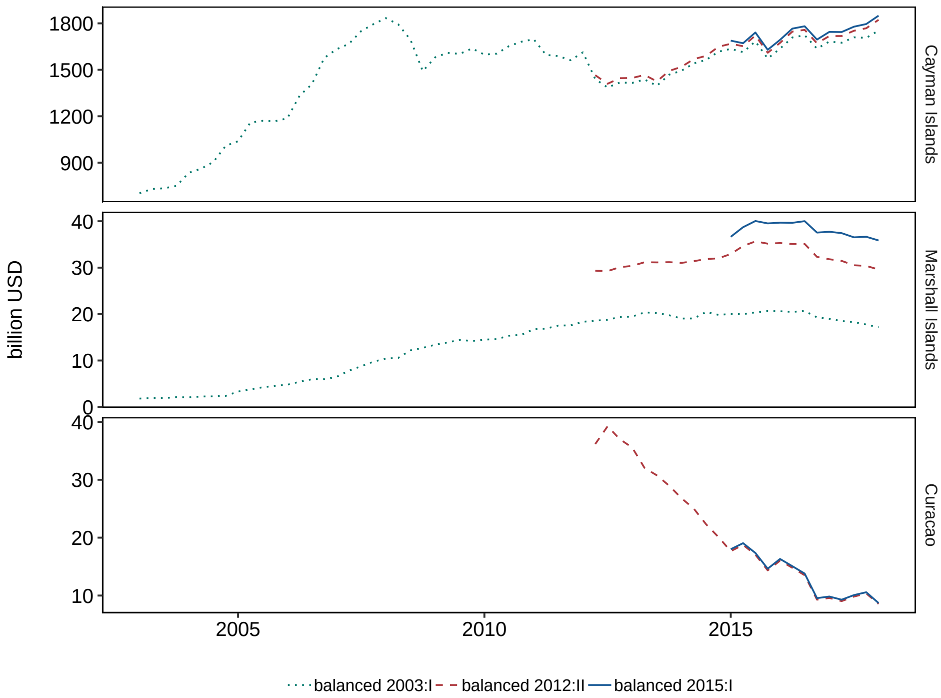
Figure A.1.4 plots national mirror claims reported against three exemplary islands to highlight the effect of different balancing choices. The green dotted line shows a sample balanced in 2003q1 to check if reporting increased substantially before 2012q2. The red dashed line shows a sample balanced in 2012q2 where nightlight becomes available. These are the series used in this study. The solid blue line shows a sample balanced in 2015q1. These series allow some initial observations. The financially largest OFC in the sample, the Cayman Islands (top panel), exhibits increasing claims over time, as do most OFCs. The largest OECD countries already report claims against this country in 2003, meaning that the three series do not deviate much and that both the level and the dynamics are well captured by the series balanced in the second quarter of 2012. The Marshall Islands have received much less scrutiny and coverage is still increasing as more and more countries start reporting data against them. This is evident in the level shift between the three series. Still, the time dynamics especially of the 2015q1 series seem

Figure A.1.3: LBS time series availability



Notes: The three panels show the availability of bilateral time series on international claims against all counterparties in the BIS' locational banking statistics. Observations are counted on the vertical axis when reports are available. The top panel shows total available countrypairs. The middle panel shows the number of reporting countries that report bilaterally (excluding those countries that only report against all countries aggregated). The bottom panel shows the total number of counterparties bilaterally reported against. The three vertical lines indicate the times at which balanced series are created for Figure A.1.4 below: 2003q1, 2012q2 and 2015q1. The series balanced in 2012q2 are used in the main text.

Figure A.1.4: Balanced mirror claims of three exemplary countries



Notes: Shows three versions of balancing the countrypairs from which mirror claims are constructed: one starting with the sample available in 2003q1 (green, dotted), one starting in 2012q2 (red, dashed, used in the main text), and one starting in 2015q1 (blue, solid). The vertical axis reports the total claims reported against the respective country by all reporting countries combined.

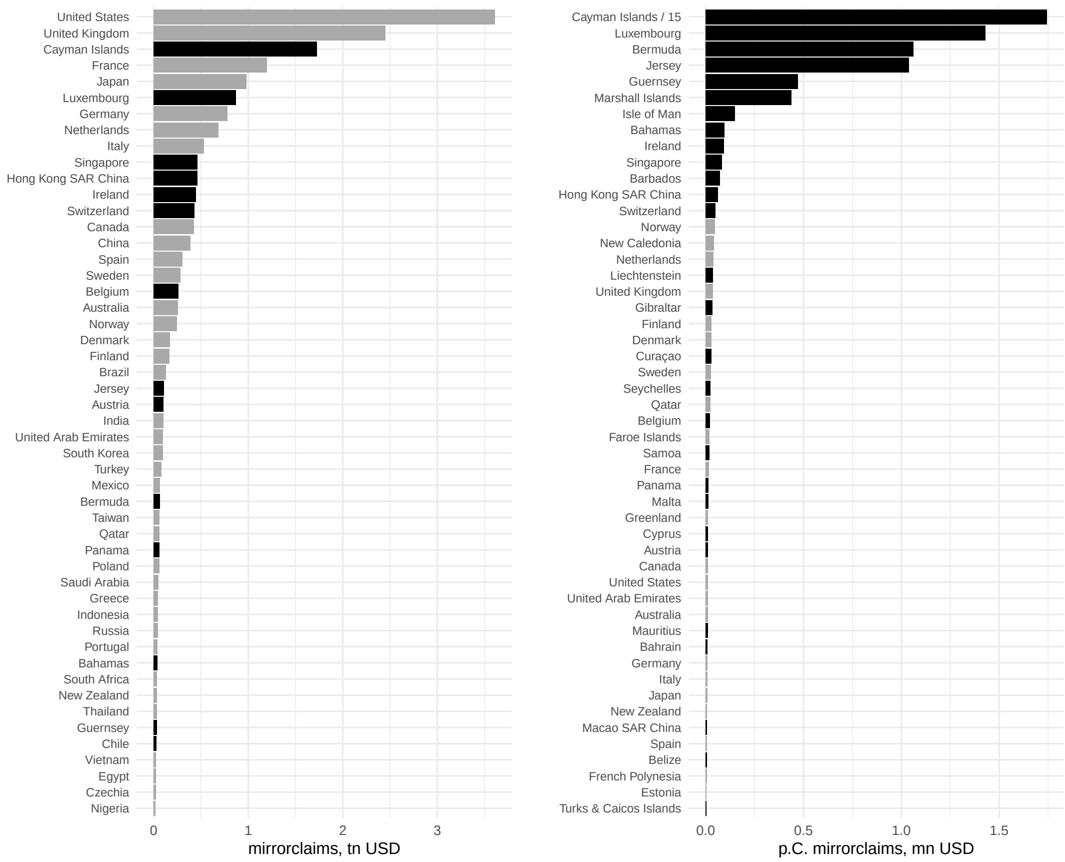
well captured in the 2012q2 series used. Some OFCs, such as Curacao (bottom panel) exhibit decreasing deposits over time. Since Curacao split from Sint Maarten and Bonaire (formerly the Netherlands Antilles) in 2010, the 2003q1 series cannot be compared here, but the 2012q2 and the 2015q1 series are closely aligned. They do show, however, how fickle international financial positions can be for OFCs with mirrorclaims dropping from around 40 billion USD in 2012 to only two billion USD in 2017. Figure A.1.4 also shows that OFCs vary substantially in their ability to attract international bank funding over time.

OFC dominance in BIS claims: While the outsized positions of OFCs generated through profit shifting and tax evasion are well documented in the literature, much less is known about the financial services sector. They are equally dominant in positions in the financial sector, however. Figure A.1.5 shows the top 50 mirrorclaim countries based on the data used in the main text (balanced in 2012q2, claims against all counterparties, reported by all reporting non-OFCs). Panel (a) shows absolute values in trillion USD. Even here, small OFCs are visible within the top 20 countries. Panel (b) turns to per capita values where the top 13 countries are classified as OFCs. Population data is taken from the World Bank, filled in with CIA World Factbook data where necessary. Note that the value for the Cayman Islands in per capita terms has been divided by 15 for visibility of the other countries. Here, only 22 of the top 50 countries are non-OFCs.

Figure A.1.5: Top 50 target countries for mirror claims (2018)

(a) total mirror claims

(b) per capita



Notes: Shows the top 50 destinations for international mirror claims in trillion USD (panel a) and per capita in million USD (panel b). Bars shaded in black show OFCs, non-OFCs are shaded in grey.

A.2. Further results on hurricane impacts

This Annex shows further results for nightlight impacts. First, results on the impact of hurricanes Irma and Maria in September 2017 in the Caribbean are presented as a case study before results on regional data are presented. Figures A.2.1 and A.2.2 plot a part of the Caribbean at different points in time. Visible in shaded areas are the British and the US Virgin Islands. The spatial polygons of the country boundaries, plotted in grey, are only added for the British Virgin Islands. The nightlight intensity inside that area would then be used to calculate monthly statistics. The top panel shows the map in August 2017, the bottom panel in October 2017. Hurricanes Irma and Maria hit the British Virgin Islands in September 2017 and the drop in nightlight intensity after these hurricanes is visible between the two maps.

Figure A.2.1: Nightlights in the British Virgin Islands pre Irma & Maria

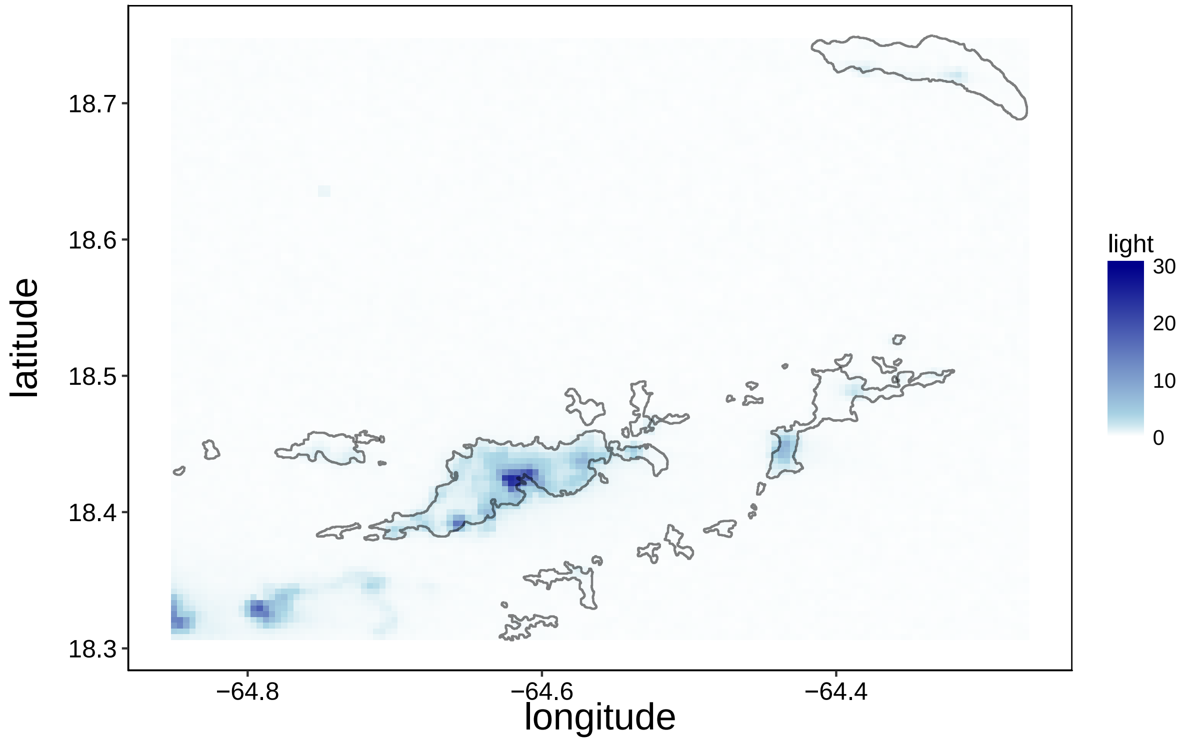
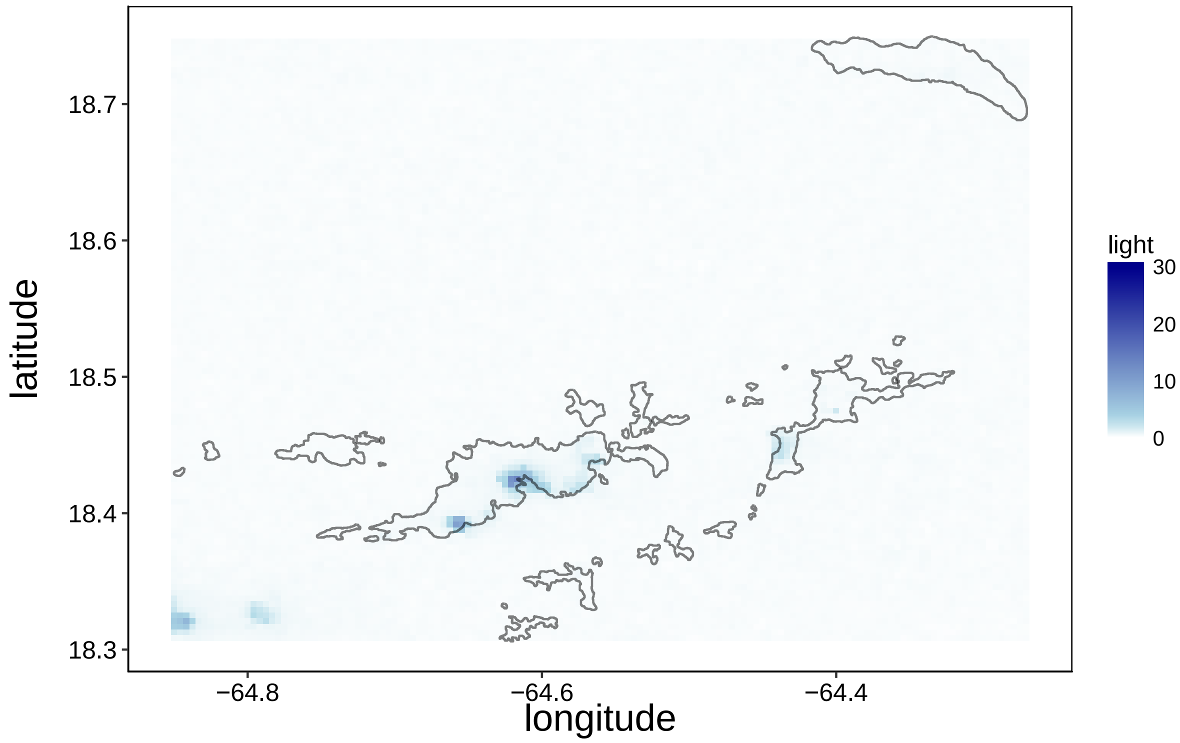


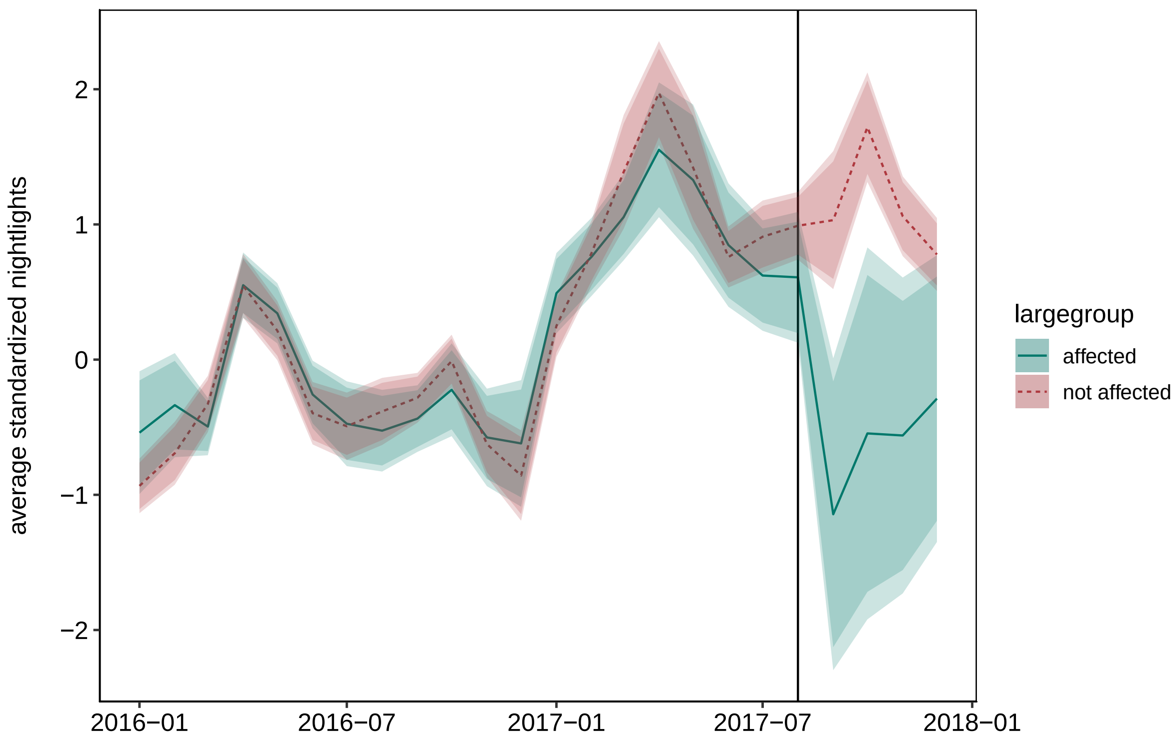
Figure A.2.2: **Nightlights in the British Virgin Islands post Irma & Maria**



Notes: Shows nightlight intensity for the British Virgin Islands (center) and the US Virgin Islands (southwest) and the country polygon (in grey borders) for the British Virgin Islands only. The top panel shows nightlight intensity in August 2017, before hurricanes Irma and Maria hit the islands. The bottom panel shows the same area in October 2017 after these hurricanes. The mean of nightlight intensity inside a country polygon forms the basis of the monthly nightlight dataset used as a measure of local conditions. Radiance of nightlight is measured in units of $Wsr^{-1}cm^{-2}$, or watt per steradian per square centimeter multiplied by 10^9.

Using the monthly nightlight dataset outlined in the data section, Figure A.2.3 compares the development of average nightlight intensity of Caribbean islands around the dates of hurricanes Irma and Maria in September 2017 (vertical line). Hurricane Irma appeared on the 30th of August 2017 and hurricane Maria dissolved on the 30th of September 2017. Data are standardized for each island and then averaged for islands affected by the storms (green line) and non-affected islands (red line). Pre-trends show that both groups of islands fluctuate together very closely until the hurricane hits. After impact, the 90% (light shading) and 95% (dark shading) confidence bands show a significant drop in nightlight intensity on affected islands.

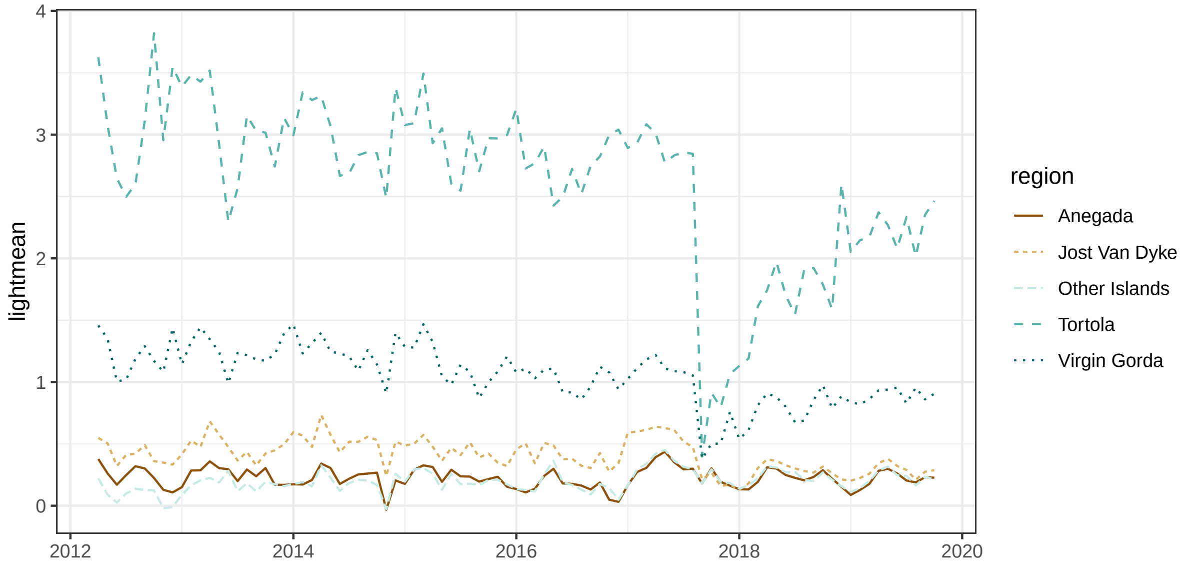
Figure A.2.3: Impacts of hurricanes Irma & Maria on nightlight intensity



Notes: The figure plots average nightlight intensity in a sample starting from January 2016 and continuing running till January 2018. The vertical line indicates September 2017 when hurricanes Irma and Maria hit the Caribbean. Countries are categorized into affected (green) and non-affected (red). All series are standardized at the country level to eliminate level effects before being averaged within the two groups.

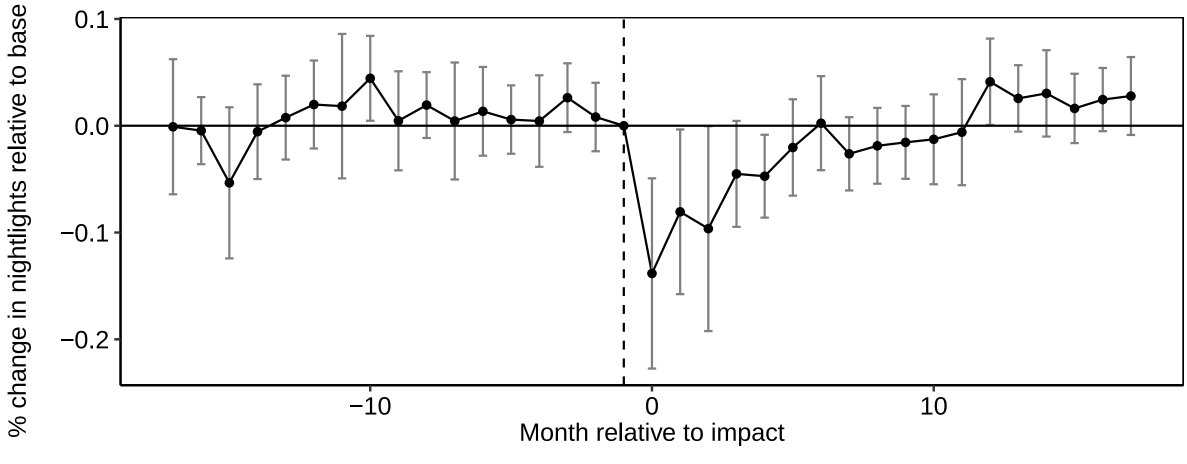
Turning to regional data Figure A.2.4 plots the mean of nightlight intensity for all regions of the British Virgin Islands. Hurricanes Irma and Maria are clearly visible here for all regions but impacts are especially strong for the capital Tortola. Moving to the entire sample, Figure A.2.5 repeats the main analysis of hurricane impacts on nightlight intensity for capital regions only. If no sub-national administrative entities exist, national data were used again. These results confirm the effects visible in the main analysis and show an immediate and sustained effect of hurricanes on local conditions.

Figure A.2.4: Regional time series for nightlights in the British Virgin Islands



Notes: Shows time series for the regional nightlight data of the British Virgin Islands. Tortola is the capital region and the drop at the end of 2017 happens at the time of hurricanes Irma and Maria.

Figure A.2.5: Hurricane impacts on nightlights in capital regions



Notes: Plots coefficients of the multiple event study with binned endpoints outlined in the main text for monthly data constructed for capital regions only. When no sub-national classification was available in country polygons, the entire island was used again. The baseline dummy left out of the regression is the month before the hurricane ($j = -1$) and 95% confidence intervals are plotted in grey based on heteroskedasticity and autocorrelation robust standard errors clustered at the country level.

A.3. Further robustness tests

OFC classifications: A potential concern with the main results on international bank positions is the classification of islands into OFCs and non-OFCs for the differing impact on financial service activity. Results in the main text use the union of OFCs in Gravelle (2015) and Johannesen and Zucman (2014), Table A.3.1 uses three different tax haven lists. As before, the hurricane dummy in these differences-in-differences specifications collects the five post-event dummies (j=1:5). Coefficients can therefore be quantitatively interpreted as the log point drop in mirror-claims compared to the six year-quarters before and including the hurricane. The first three columns show the OFC part of the sample, columns 4-6 the non-OFC part. Columns 1 and 4 show the effects of employing the tax haven list in Gravelle (2015). Columns 2 and 5 move to the tax haven list of Johannesen and Zucman (2014) and columns 3 and 6 to the older list of Hines and Rice (1994).

Results hold without qualifications for the first two lists. Only the last specification (non-OFCs using the older tax haven list) shows that the sample is not cleanly separated anymore. Although the main results still hold here. This is due to the fact that the Hines and Rice (1994) list was created 18 years before the sample in this study starts. At that time, many small island tax havens were not on the radar of policy makers and economists or did not engage in OFC activities yet. This list thus moves Aruba, Mauritius, Nauru, Niue, Samoa, Trinidad & Tobago, and the U.S. Virgin Islands into the non-haven part of the sample. In a sample starting in the second quarter 2012 when Mauritius for example was a major origin of international foreign direct investment into India and Africa, such a change should actually affect the results. While many more lists are available in the literature, they do not change the assignment in the sample used in this text. For example, the lists of Dharmapala (2008), Gravelle (2015), and OECD (2000) all categorize the same islands in the sample as OFCs, although they differ for other countries. A version of this table without binned endpoints is provided in the Annex (Table A.3.4) and confirms the results.

Table A.3.1: Robustness to different OFC categorizations: Mirrorclaims

	Dependent variable: i.h.s.(mirror claims)					
sample:		OFCs			non-OFCs	
tax-haven list:	Gr15	JZ14	HR94	Gr15	JZ14	HR94
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{hurricane}_{j=1:j=5}$	0.057	0.060	0.042	-0.317^{***}	-0.301^{***}	-0.234^{*}
	(0.093)	(0.093)	(0.077)	(0.094)	(0.078)	(0.121)
$\text{bin}_{j=\underline{j}:j=-6}$	0.035	−0.017	−0.008	−0.117	−0.071	-0.118^{*}
	(0.098)	(0.106)	(0.081)	(0.079)	(0.073)	(0.070)
$\text{bin}_{j=6:j=\bar{j}}$	−0.016	0.052	-0.173^{**}	−0.119	-0.179^{**}	−0.011
	(0.112)	(0.134)	(0.088)	(0.094)	(0.075)	(0.105)
country f.e.	Yes	Yes	Yes	Yes	Yes	Yes
year-qtr f.e.	Yes	Yes	Yes	Yes	Yes	Yes
Observations	519	462	346	346	403	519
R^2	0.171	0.217	0.231	0.173	0.193	0.129

Notes: Shows results of differences-in-differences specifications that change the assignment of islands into OFCs and non-OFCs based on lists in the literature. The first three columns show results for OFCs, the last three columns for non-OFCs. Columns 1 and 5 employ the list provided by Gravelle (2015). Columns 2 and 4 change this list to the one provided in Johannesen and Zucman (2014). Columns 3 and 6 finally use the older list of Hines and Rice (1994). The hurricane dummy collects coefficients of the first 5 quarters after a hurricane impact and both lower and upper bins are shown below. Effects therefore can be interpreted relative to the 6 quarters prior to and including a hurricane. *p<0.1; **p<0.05; ***p<0.01 based on heteroskedasticity and autocorrelation robust standard errors clustered at the country level.

No binned endpoints: Differences-in-differences versions of the main tables without binned endpoints are provided in Tables A.3.2 and A.3.3 and confirm the results of the tables provided in the main results with the exception of the slight positive effect of the falsification exercise on liabilities which turns insignificant. Table A.3.4 repeats the OFC classification exercises for the BIS results without binned endpoints.

Table A.3.2: Quantifying nightlight impacts: no binned endpoints

	Dependent variable:			
	log(nightlight intensity)		i.h.s.(nightlight intensity)	
	OFCs	non-OFCs	OFCs	non-OFCs
	(1)	(2)	(3)	(4)
$\text{hurricane}_{j=0:j=8}$	-0.217^{***}	-0.250^{***}	-0.111^{**}	-0.070^{**}
	(0.064)	(0.059)	(0.044)	(0.027)
country f.e.	Yes	Yes	Yes	Yes
year-qtr f.e.	Yes	Yes	Yes	Yes
Observations	2,145	2,189	2,187	2,349
R^2	0.163	0.277	0.174	0.275

Notes: Shows results of a difference in difference exercise with a dummy ($\text{hurricane}_{j=0:j=8}$) taking value 1 if there was a hurricane in the last nine year-months. All results are reported split-sample first showing the OFC part of the sample, then the non-OFC part of the sample. Columns 1 and 2 show results using the log of nightlight intensity, columns 3 and 4 using the inverse hyperbolic sine transformations. Without binned endpoints, effects can be interpreted relative to all non-hurricane periods. *p<0.1; **p<0.05; ***p<0.01 based on heteroskedasticity and autocorrelation robust standard errors clustered at the country level.

Table A.3.3: Further results using BIS data, no binned endpoints

dependent var.:	i.h.s.(mirror claims)			log(mirror claims)		i.h.s.(mirror liabs)	
sample:	OFCs (1)	non-OFCs (2)	all (3)	OFCs (4)	non-OFCs (5)	OFCs (6)	non-OFCs (7)
$\text{hurricane}_{j=1:j=5}$	0.008	-0.228^{***}		0.056	-0.228^{***}	0.064	0.053
	(0.083)	(0.082)		(0.108)	(0.082)	(0.075)	(0.047)
$\text{hurricane}_{j=1:j=5}$ OFC			-0.044				
			(0.072)				
$\text{hurricane}_{j=1:j=5}$ non-OFC			-0.152^{**}				
			(0.076)				
country f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
year-qtr f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	548	317	865	548	317	548	344
R^2	0.220	0.158	0.123	0.185	0.158	0.038	0.083
0.098							

Notes: Shows results of a difference in difference exercise without binned endpoints with a dummy ($\text{hurricane}_{j=1:j=5}$) taking value 1 if there was a hurricane in the last five year-quarters. Columns 1 to 3 show results on mirror claims for OFCs (1) and non-OFCs (2), as well as the entire sample (3) with an interaction term. Columns 4 and 5 repeat columns 1 and 2 but with log transformations instead of inverse hyperbolic sine transformations. Columns 6 and 7 report a falsification exercise showing results on all liabilities reported against islands in the sample. *p<0.1; **p<0.05; ***p<0.01 based on heteroskedasticity and autocorrelation robust standard errors clustered at the country level.

Table A.3.4: Robustness to different OFC categorizations: Mirrorclaims, no binned endpoints

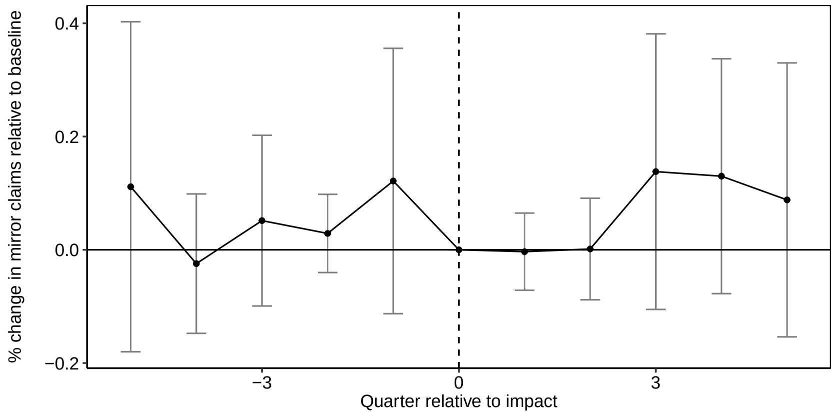
	Dependent variable: log(mirror claims)					
sample:		OFCs			non-OFCs	
tax-haven list:	Gr15	JZ14	HR94	Gr15	JZ14	HR94
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{hurricane}_{j=1:j=5}$	0.052	0.049	0.090	-0.214^{**}	-0.200^{***}	-0.168^{*}
	(0.110)	(0.117)	(0.083)	(0.085)	(0.064)	(0.098)
country f.e.	Yes	Yes	Yes	Yes	Yes	Yes
year-qtr f.e.	Yes	Yes	Yes	Yes	Yes	Yes
Observations	519	462	346	346	403	519
R^2	0.169	0.214	0.186	0.137	0.132	0.104

Notes: Shows results of differences-in-differences specifications that change the assignment of islands into OFCs and non-OFCs based on lists in the literature without using binned endpoints. The first three columns show results for OFCs, the last three columns for non-OFCs. Columns 1 and 5 employ the list provided by Gravelle (2015). Columns 2 and 4 change this list to the one provided in Johannesen and Zucman (2014). Columns 3 and 6 finally use the older list of Hines and Rice (1994). The hurricane dummy collects coefficients of the first 5 quarters after a hurricane impact. *p<0.1; **p<0.05; ***p<0.01 based on heteroskedasticity and autocorrelation robust standard errors clustered at the country level.

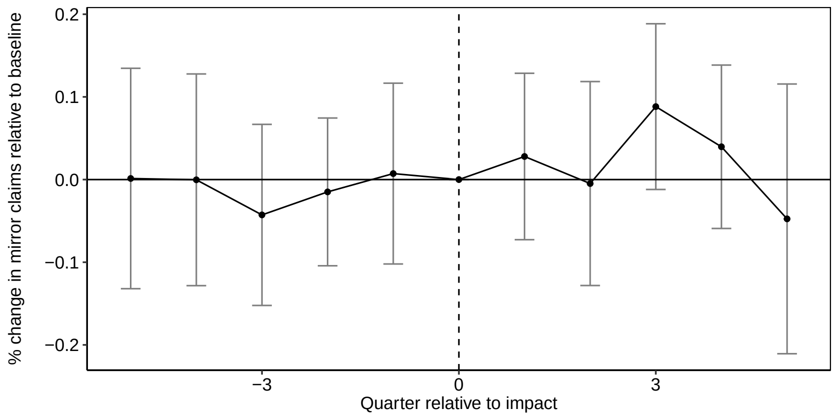
Falsification exercise using bank liabilities to non-banks: Figure A.3.1 provides event study specifications of the falsification exercise using bank liabilities separately for OFCs and non-OFCs. The non-OFC panel confirms the non-result discussed in the main text. As the OFC panel shows, the positive difference-in-difference result is driven by estimates with high standard errors three quarters after impact and should not be interpreted.

Figure A.3.1: Falsification using liabilities: Event Study

(a) OFC sample



(b) non-OFC sample



Notes: Plots coefficients of the multiple event study with binned endpoints outlined in the main text for quarterly data on passive deposits in non-OFC BIS reporting countries. The baseline dummy left out of the regression is the quarter of the hurricane ($j = 0$) and 95% confidence intervals are plotted in error bars based on heteroskedasticity and autocorrelation robust standard errors clustered at the country level. The top panel shows results for the OFC part of the sample, the bottom panel for the non-OFCs part of the sample.

Heterogenous treatment effects: As discussed in the main text, heterogenous treatment effects over time are intuitively not a major concern. To alleviate doubt, Table A.3.5 provides results of the Borusyak et al. (2024) estimator in a bootstrap exercise. Note that this specification is not directly comparable to the main text as the method has not been extended to multiple treatments yet. I therefore run a bootstrap exercise where I draw one hurricane at random for every island that experiences more than one. This exercise is repeated 100 times to allow for many possible combinations. Also, no binning takes place, instead, I compare the post and pre-periods used in the diff-and diff results in the main text (9 months for nightlights, 5 year-quarters for the BIS data). This reduces the sample and the identification of the time trend. I also eliminate the never treated control group. The fact that results are still qualitatively similar is reassuring. The transitory nature of the shock, the existence of never treated units, and the fact that hurricanes started and disappeared long before the sample period alleviates the problem of heterogenous treatment effects in my setting. The size of the coefficient on nightlight intensity is downward biased due to the presence of many values at zero and between zero and one in the inverse hyperbolic sine transformation as I show in more detail below.

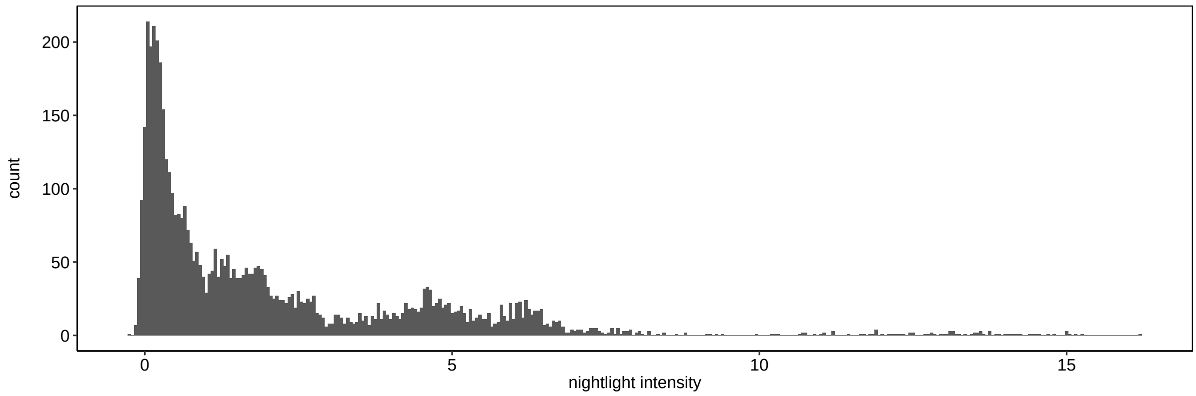
Table A.3.5: Robustness to Heterogenous Treatment Effects

dependent var.:	nightlights	mirror claims	
sample:	All (1)	non-OFCs (2)	OFCs (3)
$\text{hurricane}_{j=1:j=9m/5q}$	-0.091^{***}	-0.331^{***}	0.194
	(0.027)	(0.103)	(0.240)
country f.e.	Yes	Yes	Yes
year-qtr/month f.e.	Yes	Yes	Yes
Observations	2858	333	182

Notes: This table shows a robustness exercise using the estimator introduced in Borusyak et al. (2024) with the following adjustments to the main sample: Never treated islands are dropped, no end points are used (hence only data for the effects window is used), and single hurricanes per island are drawn in 100 bootstrap samples. The periods mirror the difference-in-difference results of the main text: +/-9 year-months for the nightlight data and +/-6 year-quarters for the BIS data. *p<0.1; **p<0.05; ***p<0.01 based on the standard errors of (Borusyak et al., 2024).

Values at and close to zero: To show the issue of numerical values around 0, Figure A.3.3 in the Annex plots the distribution of all nightlights in the sample. It shows a large number of small observations close to 0 which biases coefficients using the inverse hyperbolic sine transformation downwards. To show that this does not affect the interpretation of the event studies, Figure A.3.4 shows event study specifications for nightlights using the log, the inverse hyperbolic sine, and the level of nightlight intensity. In all of these cases, hurricane impacts are clearly visible. This confirms that, beyond the difficulties in interpreting coefficients of variables with many observations around 0, this methodological choice is not crucial for the results presented here. The main text therefore uses the more common log specification to interpret effect sizes.

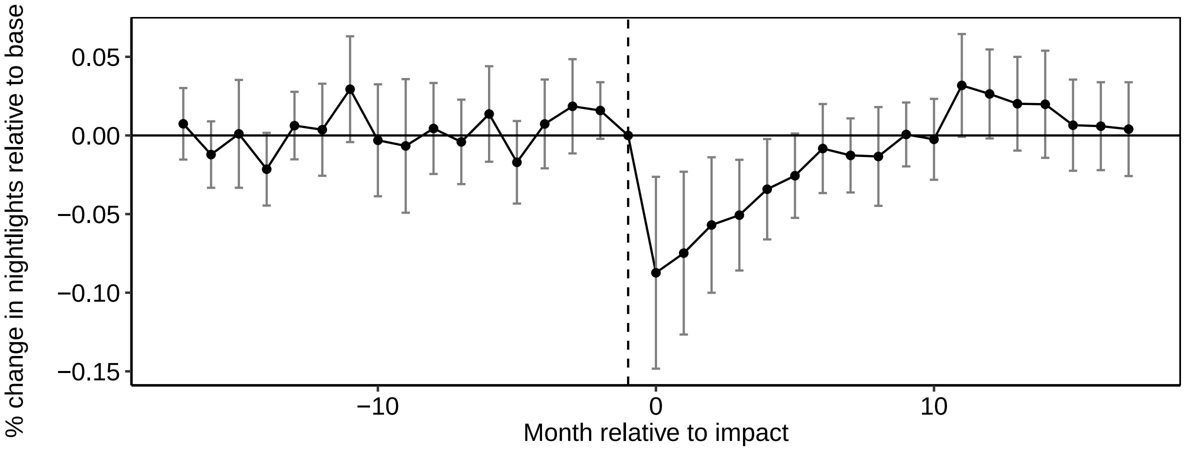
Figure A.3.3: Distribution of nightlight intensity



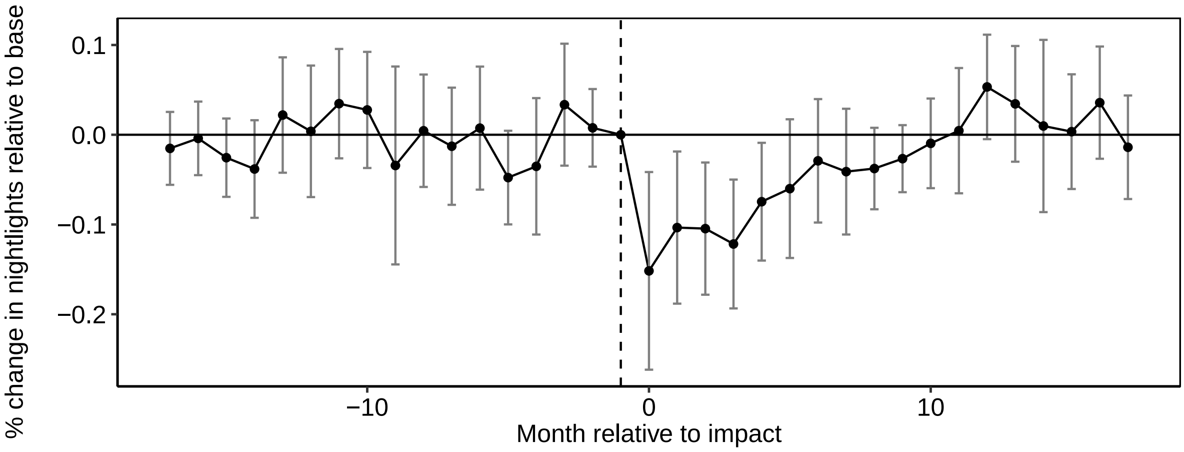
Notes: On the vertical axis, the histogram counts the number of observations that exhibit the nightlight intensity plotted on the horizontal axis.

Figure A.3.4: Event studies using i.h.s., log, and level specifications

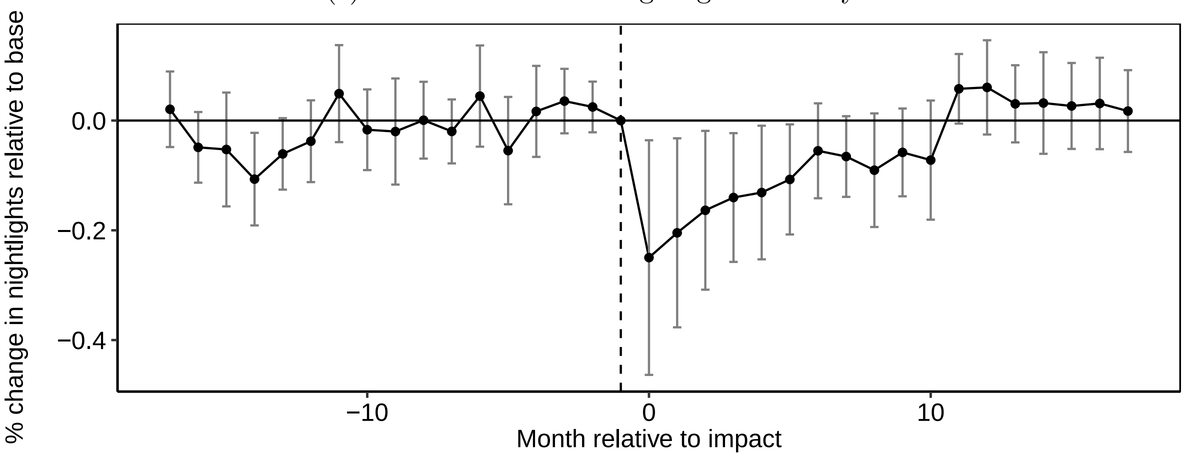
(a) Outcome variable: i.h.s(nightlight intensity)



(b) Outcome variable: log(nightlight intensity), sample reduced to positives



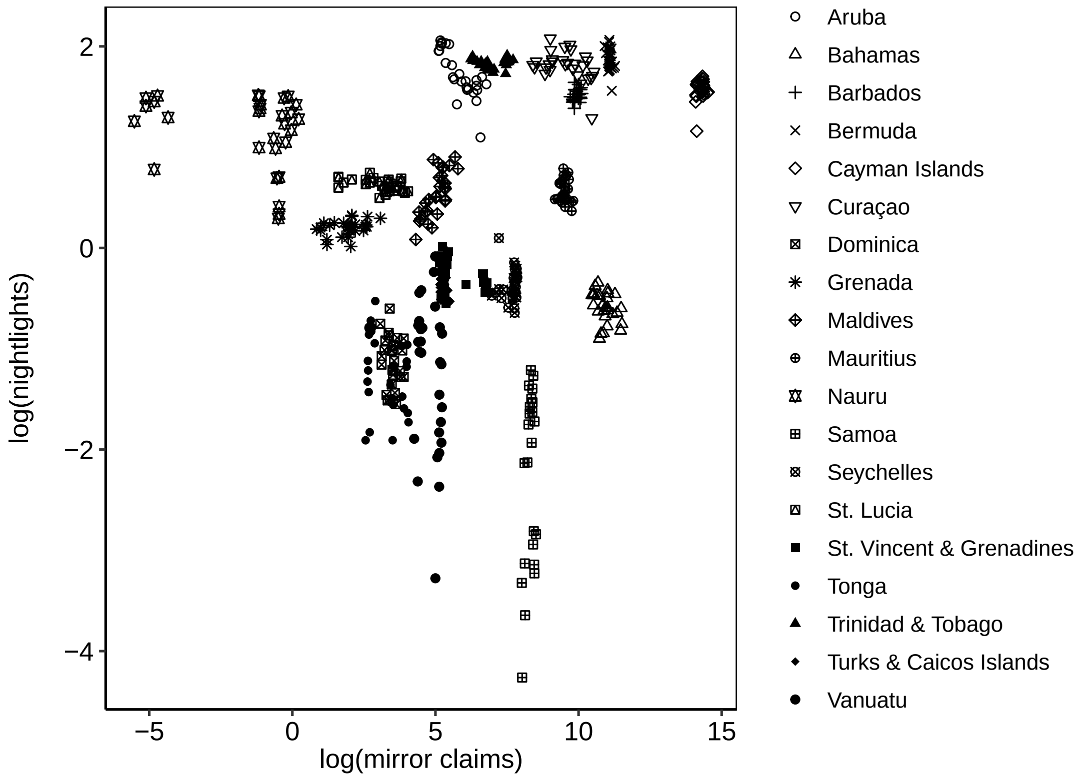
(c) Outcome variable: nightlight intensity



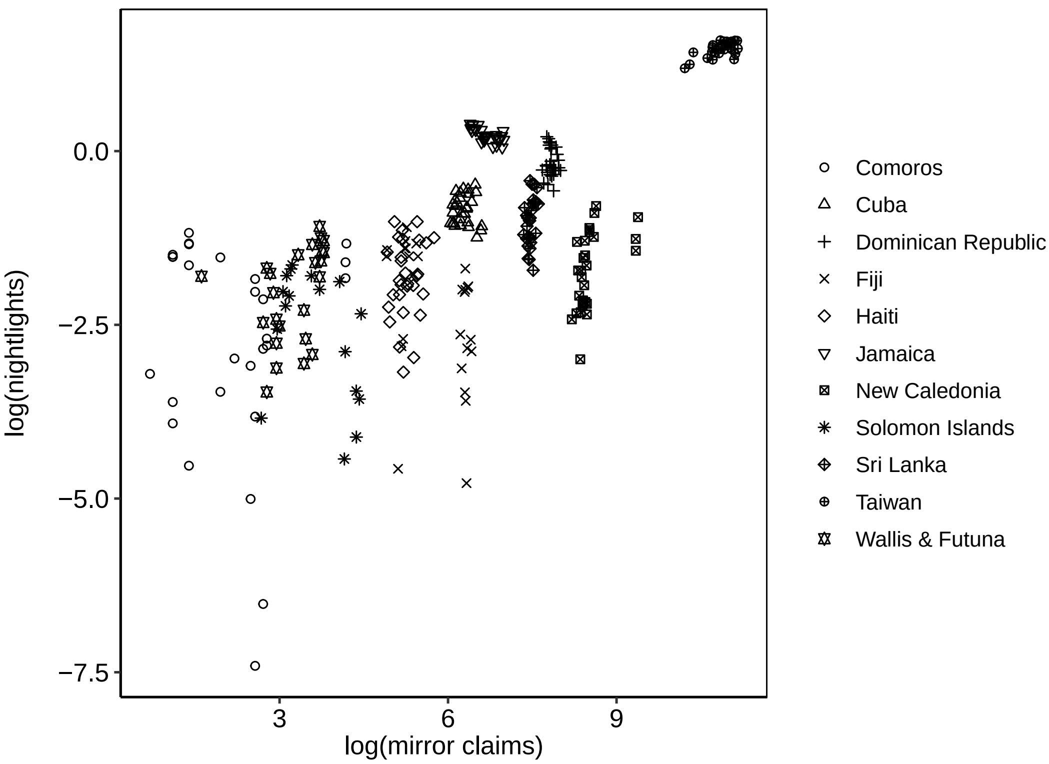
Notes: Shows the event study of the main text for hurricane impacts on nightlight intensity for the entire sample. The baseline dummy left out of the regression is the month before the hurricane ($j = -1$) and 95% confidence intervals are plotted in grey, based on heteroskedasticity and autocorrelation robust standard errors clustered at the country level. The top panel uses the inverse hyperbolic sine transformation that is used in the main text. The middle panel reduces the sample to countries without negative and 0 values for nightlight intensity and shows results of log-transformed data. The bottom panel finally uses the nightlight data in levels.

Figure A.3.6: Direct correlations of nightlights and mirror claims: Logs

(a) OFC sample



(b) Non-OFC sample



Notes: Both panels plot the log of nightlights over the log of the sum of international bank claims by all reporting non-OFC economies. The sample is limited by the availability of offshore mirror claims. Panel (a) shows the OFC part of the sample where no correlation is visible. Panel (b) shows the non-offshore part of the sample with a positive relationship of both variables.

A.4. Further extended results

Figure A.4.1: Absolute changes in incorporation activity

Notes: Shows the mean drop of incorporations on weekends, local holidays and holidays in Tokyo, New York, and London. All changes are compared to average incorporations on non-weekend workdays.