The impact of ownership transparency policies on illicit purchases of US property*

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Abstract

High value real estate is a popular destination for corrupt and criminal foreign assets, in part caused by limited oversight and lack of transparency in real estate transactions. In response to these concerns, the US Treasury began implementing a series of Geographic Targeting Orders (GTOs) in 2016, forcing corporate buyers making all-cash purchases in targeted counties to report the company's ultimate owner. To estimate the causal effect of GTOs on these types of transactions, we combine data on on millions of real estate transactions over the period 2014-2019 with a staggered difference-in-differences design. Our analysis suggests the absence of an aggregate effect of the GTOs on corporate all-cash purchases in targeted counties, as well as little evidence of substitution into other types of purchases. We contend that the lack of overt enforcement and validation of the ownership requirements failed to create a sufficient deterrent effect to drive out participation in the sector by illicit actors.

*Authors listed in alphabetical order. Equal authorship is implied. Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at http://www.zillow.com/ztrax. The results and opinions are those of the author(s) and do not reflect the position of Zillow Group. All remaining errors are our own.

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1 Introduction

Much of the scholarship on eradicating corruption in developing countries centers around fixing domestic institutions and the incentive structures that enable the abuse of public office for private gain (Olken and Pande, 2012; Ferraz and Finan, 2011). Yet a slew of recent investigations, such as the Panama Papers, the FinCEN files, and the most recent Pandora Papers, highlight the role that political and financial institutions in richer countries play in facilitating corrupt, criminal, and kleptocratic activity abroad.[1] The ability of bad actors to stash their illicit earnings abroad incentivizes the underlying criminal activity, deprives origin countries of important sources of revenue (Reuter, 2012; Gray et al., 2014), and produces a range of deleterious effects in the recipient markets (Badarinza and Ramadorai, 2018; De Simone, 2015).

Real estate markets in rich countries, in particular, are a popular target for money laundering (FATF, 2007). Real estate assets offer many attractive features for money launderers, including the ability to store large amounts of cash without a clear mechanism to determine the actual market value of the asset. Moreover, in many markets real estate companies and their agents are not covered by the same anti-money laundering (AML) provisions that govern banks, leading to less scrutiny of the source of their clients' wealth. While it is difficult to determine precisely how much illicit money makes its way into foreign property markets, the amounts observed in prosecuted money laundering cases (a significant underestimate) suggest it is sizable. A recent study noted that $2.5 billion were laundered through US real estate between 2015 and 2020 (GFI, 2021).

Lax oversight of the real estate sector is compounded by the fact that a majority of jurisdictions impose weak reporting requirements on legal entities. Rather than owning a property in one's own name, buyers of real estate can shield their identity using shell companies - firms that do not engage in any substantive economic activity. In recent years, shell companies have been a key conduit for corrupt politicians from countries such as Malaysia, Congo, and Ukraine to buy luxury real estate in the US and other advanced economies (Gabriel, 2018; White, 2020). For example, an analysis of London properties connected to owners under investigation for cor-

[1]"The Panama Papers: The largest investigation in journalism history exposes a shadow financial system that benefits the world's most rich and powerful.," International Consortium of Investigative Journalists, October 2020, https://www.icij.org/investigations/pandora-papers/. "The FinCen Files," International Consortium of Investigative Journalists, September 2020, www.icij.org/investigations/fincen-files. "The Pandora Papers: The largest https://www.overleaf.com/project/5fca916d7bb79481ed2589c1investigation in journalism history exposes a shadow financial system that benefits the world's most rich and powerful." International Consortium of Investigative Journalists, October 2021, https://www.icij.org/investigations/pandora-papers/.

ruption revealed that over 75% of the properties were purchased using a company base in an offshore jurisdiction with high levels of financial secrecy (De Simone, 2015).[2] In the hope of reducing the ability for corrupt and criminal actors to hide behind shell companies when engaging in economic activity, policymakers have begun introducing laws to require transparency around "beneficial ownership," requiring companies to disclosure who ultimately owns, benefits from, or controls them.

In this paper we analyze an institutional change designed to curb illicit financial flows into high-value real estate markets in the US. In January 2016, FinCEN, a bureau within the US Department of Treasury tasked with combating money laundering, announced the implementation of Geographic Targeting Orders (GTOs) in two counties: Miami-Dade and Manhattan. These orders required title insurance companies to collect information on the beneficial owners of any legal entities purchasing real estate using only cash ('corporate all-cash purchases') whenever the sales price exceeded a threshold defined at the county level. Though the information on beneficial owners was only made available to law enforcement authorities (and not the general public), the strengthened transparency was designed to increase the probability that someone purchasing real estate with laundered money would be detected. From 2016-2018, the GTOs were extended to an additional twenty counties, with the monetary thresholds also lowered to expand the breadth of property transactions covered by the policy.

We estimate the causal effect of the GTO introduction on real estate transactions, under the assumption that an increased probability of detection should result in a deterrence effect, reducing the number of corporate all-cash purchases in the targeted markets. To do this we make use of Zillow's Transaction and Assessment Dataset (ZTrax), which covers nearly all residential real estate transactions in the United States over the past 10 years. We then exploit the staggered roll-out of the GTO policies in certain counties and above certain price thresholds to investigate whether the increased transparency leads market participants to adjust their behavior following either the announcement or implementation of a GTO in a given county.

In our main analysis, we estimate the effect of GTO policies using a staggered difference-in-differences design. Given the multiple treatment periods and large differences in group sizes, we use the doubly robust estimator introduced by Callaway and Sant'Anna (2021b). Across a number of different model specifications, we find no evidence of a sustained effect of the

[2] Only 1.3% of properties in London are owned by companies based in offshore jurisdictions of this nature, suggesting that the preponderance of these firms in corruption cases is not an artefact of the nature of the UK property market.

GTO policy on the number of or total price volume of corporate all-cash purchases in targeted counties. We also see little difference in the patterns of corporate all-cash purchases versus a 'placebo' outcome that should not be affected by the policy: real estate purchases by individuals using mortgages. In addition to the staggered difference-in-differences design, we also estimate augmented synthetic control models as proposed by Ben-Michael, Feller, and Rothstein (2021). Again, we find no systematic evidence that GTO policies affected the number or total value of corporate all-cash purchases.

We then test whether, on the margin, the GTO program led potential buyers to either target properties just below the reporting threshold or otherwise manipulate the price so that purchases would not be reported. When considering the distribution of purchase prices in GTO-affected counties following the introduction of the policy, we find no evidence of bunching just below the reporting threshold. This stands in stark contrast to bunching in purchase prices observed in response to real estate transaction taxes in New York as reported in Kopczuk and Munroe (2015), a finding we replicate with the ZTrax data.

Finding no evidence of an average affect on corporate all-cash purchases, we dig in further to test whether the GTOs had a differential effect on all-cash purchases by legal entities 'most likely' to be used for these illicit transactions. Merging in business registry data from OpenCorporates,[3] we show that the GTOs did not lead to an overall decline in purchases by companies registered by mass formation agents, all-cash purchases by companies registered in so-called secrecy jurisdictions (Delaware, Nevada, and Wyoming), or by newly incorporated companies. We also do not find any evidence that buyers in GTO-covered counties attempted to evade the rules by substituting into other purchasing strategies such as using trusts (which were not covered by transparency requirement) or mortgages from foreign or *bad* banks. Across our different estimations, we do find some suggestive evidence that the initial GTO in Miami and Manhattan may have affected behavior in those real estate markets. In the last section, we, therefore, dig deeper to investigate whether this first GTO in March 2016 produced any substantial results by zooming in on the geographic areas 'most likely' to have been attractive to money launderers: Manhattan and Miami. At both the county and zipcode levels, we see no significant evidence that corporate all-cash purchases fell after the orders were introduced.

Taken together, our analysis suggests the absence of an aggregate effect of the GTOs on corporate all-cash purchases in the targeted counties. Our findings sharply differ from those

[3] https://opencorporates.com/

presented by Hundtofte and Rantala (2018). Using the same underlying data but a shorter time period, Hundtofte and Rantala (2018, 1) conclude that "all-cash purchases by corporations fall by approximately 70%". As we show in our paper, this finding is likely due to an erroneous change in the identification of corporate buyer types. Once corrected, there is little evidence in the data that the GTOs had any aggregate effect on real estate market behavior.

In this paper, we make several contributions to the empirical literature on anti-corruption efforts and transparency. We are undertaking one of the first studies of an intervention specifically designed to counter illicit flows in property markets. Despite the fact that both regulators and civil society have raised significant concerns about the abuse of this industry, few efforts have been made to empirically evaluate the impact of policies intended to drive out illicit money (Transparency International, 2017). In addition to Hundtofte and Rantala (2018), one of the few studies that examines how policy can affect the laundering of wealth through property markets is Agarwal, Chia, and Sing (2020). The authors find that in Singapore, cross-border cash limits and enhanced real estate agent regulations lead to a reduction in the price of properties purchased by persons linked to offshore shell companies.

More generally, this paper contributes to a nascent literature on the impact of AML policies on various sectors of the economy. To date, most work in this area has focused on the impact AML regimes have had both on and through the banking sector (Slutzky, Villamizar-Villegas, and Williams, 2020; Agca, Slutzky, and Zeume, 2021) or the aggregate impact of international AML watch-lists (Morse, 2019).

Our work also adds to a growing literature on how policies aimed at revealing ultimate beneficial ownership can drive illicit wealth out of markets. This includes research on the large, negative impact that tax transparency initiatives have on various forms of offshore wealth (Casi, Spengel, and Stage, 2020; Menkhoff and Miethe, 2019; Beer, Coelho, and Leduc, 2019; O'Reilly, Ramirez, and Stemmer, 2019) and a number of studies showing that removing the presumption of anonymity can force those who previously evaded detection to come clean (Bethmann and Kvasnicka, 2016; Londoño-Vélez and Ávila-Mahecha, 2021).

Finally, we build upon an existing literature that examines the drivers and impacts of foreign demand for property. For example, Badarinza and Ramadorai (2018) find that fluctuations in economic and political risk abroad leads to changes in real estate prices in London and New York. Similarly, Gorback and Keys (2020) find that the introduction of foreign-buyer taxes by other countries induced an increase in Chinese housing investment in the US and a subsequent increase in housing prices. Other studies have found that foreign buyers typically buy at a premium and are drivers of faster house price growth (Sá, 2016; Cvijanović and Spaenjers, 2020).

2 Context

2.1 Motivation

The US is considered by many to be a popular destination for illicit finance. In an analysis of grand corruption cases comprising $56 billion, a World Bank study found that corporations and bank accounts were more likely to be established in the US than any other jurisdiction (de Willebois et al., 2011). This is partly driven by the attractiveness of investing in the US economy (Forbes, 2010), but also the perception that despite the US's role in enforcing AML standards around the world, its own banks and corporate service providers' compliance with those standards is lacking (Findley, Nielson, and Sharman, 2014).

Up until 2021, setting up a US-based shell company was relatively simple, allowing individuals to easily conduct business with a significant degree of anonymity. Registering a company in the US is not only inexpensive and quick, but there have been no requirements that the beneficial, or true, owners of the corporation be reported to authorities at the time of registration. In some states, this corporate registration process requires less information from an individual than what is needed to obtain a library card (Global Financial Integrity, 2019). People from around the world can hire a corporation service provider to set up a US-based shell company and then use nominee officers, directors, and stockholders to shield the names of true beneficiaries from public record (Network, 2006).[4] Law enforcement officials often complain about their investigations going cold when shell companies appear in the money trail.[5]

Real estate markets appear to be an attractive target for money launders, especially in the US, given the corporate secrecy conferred. Data on the predominance of real estate in money laundering cases is somewhat scarce, but according to a survey of legal professionals around the world by the Financial Action Task Force (FATF), an international standard-setter for AML policies, real estate makes up approximately 30% of criminal assets confiscated in around 20 countries (FATF, 2013a). Investigative journalists have highlighted a large number of potentially-corrupt

[4]The passage of the Corporate Transparency Act (2021) should undo this anonymity through the creation of a centralized database of the beneficial owners of all companies registered in the US. How and when this new law will be implemented is still under deliberation at the time of writing.

[5]Barlyn, Suzanne. "Special Report: How Delaware kept America safe for corporate secrecy". *Reuters*, August 24, 2016. https://www.reuters.com/article/us-usa-delaware-bullock-specialreport/special-report-how-delaware-kept-america-safe-for-corporate-secrecy-idUSKCN10Z1OH

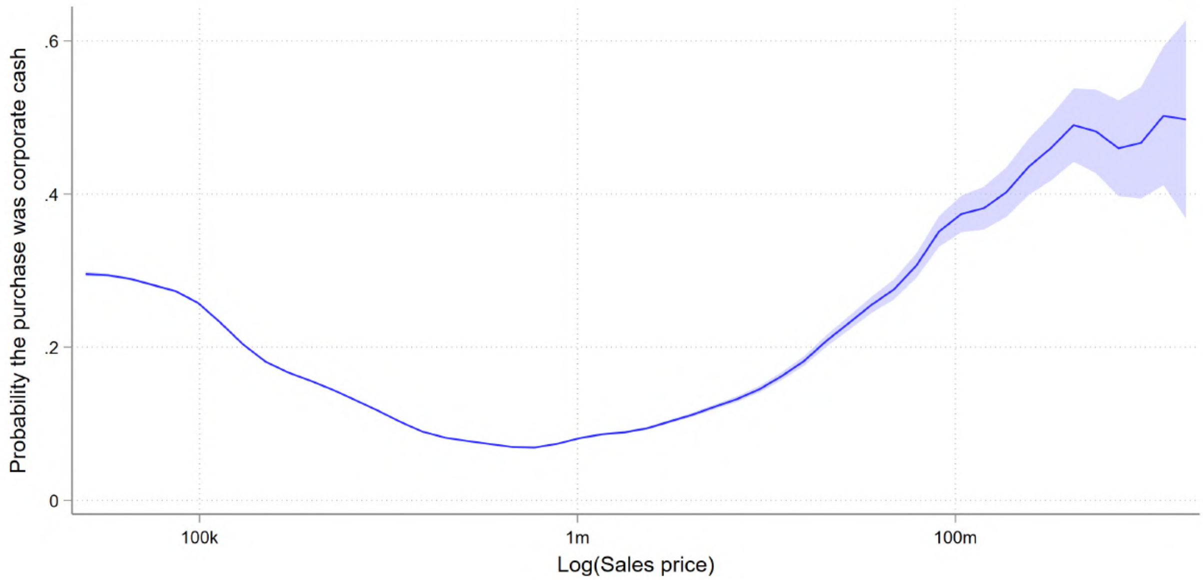
individuals investing in US property, ranging from the CEO of Equatorial Guinea's state-owned oil company to the individuals behind the Malaysian 1MDB scandal (Martini, 2017; Massoko, Orphanides, and Jones, 2021). Other criminal organizations such as drug cartels, the Italian mafia, and groups propagating Ponzi schemes have been caught moving money into US real estate using anonymous shell companies (GW, 2020; Wieder, Dasgupta, and Wang, 2021).

The attractiveness of real estate markets to illicit finance is in part due to factors inherent to the sector. The purchase of a high value property provides a 'one and done' method of laundering a large amount of money, either to store it in a steadily appreciating asset or to resell again, thus generating proceeds which are subsequently viewed as clean. This is compounded by the fact that in countries such as the US, many of the non-financial parties involved in the real estate market, such as brokers and lawyers, are not subject to AML regulation and are thus not required to give the buyers nor the sellers of property much in the way of scrutiny (FATF, 2007; Martini, 2017).

Although real estate professionals are not required by law to conduct due diligence on their clients, financial institutions are. Consequently, illicit property purchases are more likely to be spotted if they involve banks or similar institutions. Under the Bank Secrecy Act (BSA), financial institutions must monitor their transactions and report any suspicious activity to FinCEN in the form of Suspicious Activity Reports (SARs), exposing buyers that rely on external financing to more scrutiny. Since 2012, FinCEN has applied these requirements not only to ordinary banks but also to non-bank mortgage companies and brokers. Thus, even for purchases made using anonymous shell companies, because of due diligence requirements, any lender must still gather information on the beneficial owner(s) of the shell company.

Prior to the GTO program, the use of a shell company could still grant the buyer a reasonable degree of anonymity if the purchase was made 'in cash,' as none of the parties in the transaction would be subject to FinCEN's reporting requirements. This loophole led to concerns that corporate all-cash purchases have become an attractive means of investing in US property while maintaining anonymity. While it is impossible to know the share of corporate cash purchases that are illicit, the practice is particularly prevalent with regards to luxury properties. A recent investigation by the New York Times found that almost half of the properties priced at $5 million and above were bought using shell companies (Story, 2015). Our own estimates using Zillow's data confirms a similar pattern: as illustrated in Figure 1, corporate all-cash buyers seem to dominate both the very low and and the very high end of the property market, with around 40% of

Figure 1: Cash purchases by companies become more common in the luxury property market



Notes: Figure shows local polynomial estimate of the probability a purchase is both (i) made by a corporation and (ii) is made without a mortgage, for residential purchases over $50k and below $250m. Estimates made over every single purchase captured by the ZTrax database between 2010 and 2015 for the for 21 counties ultimately targeted by the GTO program (approximately 8 million observations). 95% confidence intervals shown.

all purchases over $100 million involving these types of buyers.

2.2 The Geographic Targeting Order program

As a response to concerns that corporate cash transactions afford buyers a high degree of secrecy, FinCEN developed its Geographic Targeting Order (GTO) program. On January 13th, 2016, FinCEN announced its first two GTOs for Miami and Manhattan, which were set to come into effect on March 1st of that year and last for 180 days. The order applied to all transactions in which a legal entity purchased a residential property, without external financing, in either cash currency or using a check. Only properties purchased above a set price ($1 million for Miami and $3 million for Manhattan) were covered.

Importantly, the GTOs created a reporting requirement for title insurance companies, which applied to any title insurance company involved with a reportable transaction. The companies were required to collect identifying information on the person representing the legal entity in the transaction as well as information on any and all beneficial owners (those with more than 25% control over the legal entity). FinCEN reportedly chose title insurance companies because nearly every buyer purchases title insurance (GAO, 2020).

The initial order was set to expire after 180 days, but after four months, FinCEN announced a second GTO which covered a further 12 counties. To date, FinCEN has renewed its GTO program eight times, eventually expanding the reach of the program to counties in California, Texas, Hawaii, Nevada, Washington, Massachusetts, and Illinois. Over the first two and a half years of the program, FinCEN applied different price thresholds for its reporting requirements, but eventually reduced the threshold to $300,000 for all targeted counties. Figure 2 displays the timing of when each county was introduced to the GTO program as well as the price threshold that was applied.

In addition to expanding the geographic and price coverage of the GTOs, FinCEN slowly expanded the set of monetary instruments that would be covered. As mentioned above, the first GTO covered cashier's, certified, and traveler's checks as well as cash currency. In August 2016, the scope was expanded to personal and business checks, then to all wire transfers in September 2018, and finally to virtual currencies in November 2018. While FinCEN has not updated the scope of the GTOs in any manner since November 2018, it has continued to renew the program every six months.

To date, FinCEN has issued eight public GTOs. However, reports from both title insurance companies and the Miami Herald indicate that FinCEN also issued a confidential GTO directly to title insurers in April 2018, to be implemented in the subsequent month, that lowered the reporting threshold to $300,000 (Hall and Nehamas, 2018) and expanded the range of the GTO to "five more metropolitan areas" (Bethencourt, 2018). If these reports are correct, then the terms of FinCEN's publicly-released GTO from November 2018 were actually introduced six months earlier. In our analysis of the impact of the GTOs, we test whether this confidential GTO had a separate impact from the publicly-released GTOs.

Despite only covering twenty-two counties in total,[6] the GTO program targeted both a significant share of the US population and its overall housing market. In 2015, the counties covered by the program represented about 25% of the country's population, about half of the national volume of real estate sales and more than 65% of all corporate all-cash purchases. They also represent a sizable share of potentially-illicit behavior. GTO counties were the origin of roughly 29% of all suspicious activity reports filed by banks to FinCEN in 2015. Half of the GTO counties are also designated by FinCEN as "High Intensity Financial Crime Areas" (HIFCAs), a special designation for areas where authorities estimate that money laundering and financial crime is

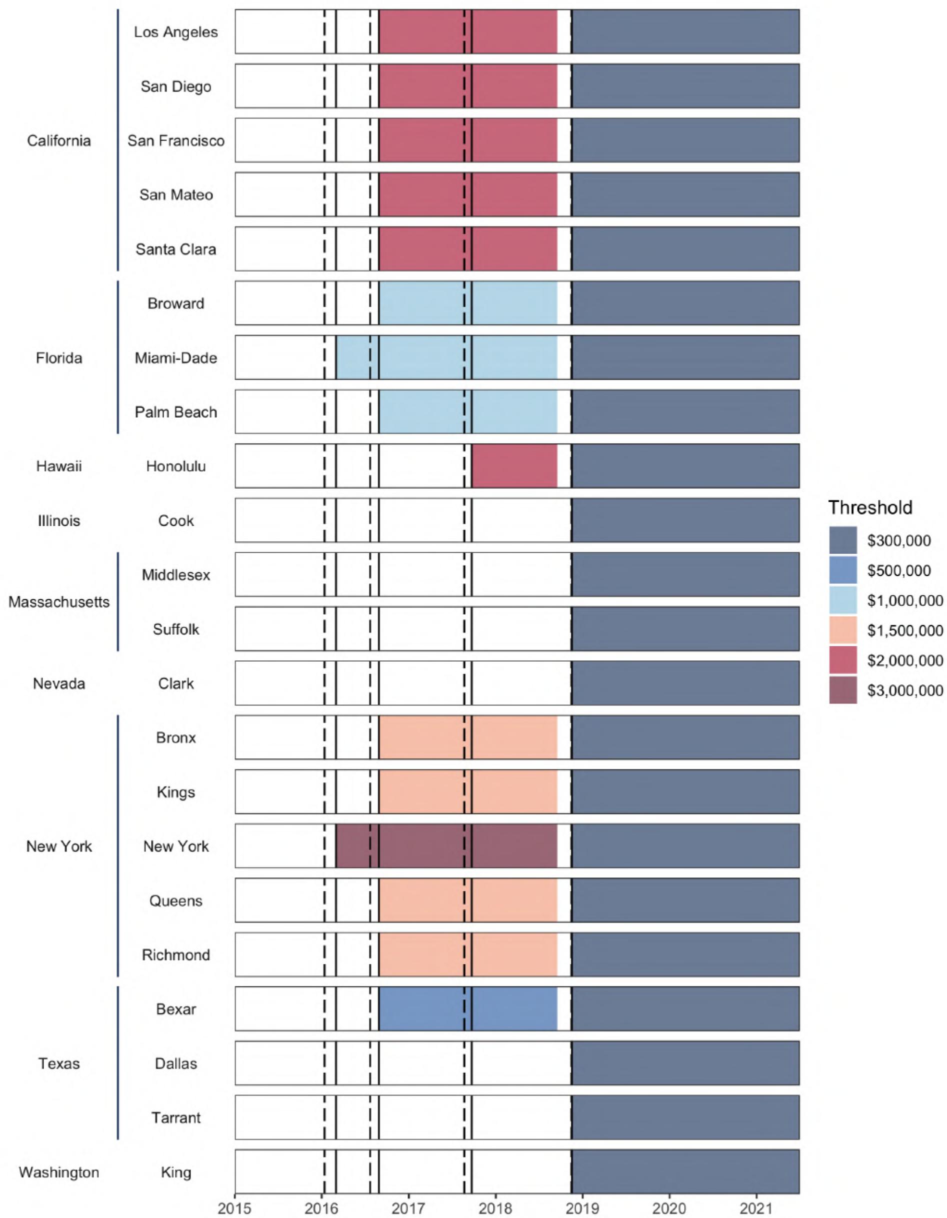
[6] As of 2016, there were 3,007 counties in the United States.

extensive. Out of the fifty-seven money laundering cases which involved the purchase of real estate detailed in a recent report, nearly 60% involved properties in GTO counties (GFI, 2021).

While there have occasionally been calls to expand the GTO program to other counties or to expand its scope to other types of transactions such as commercial property, beyond Hundtofte and Rantala (2018) there has been little analysis of the impact of the program. When interviewed by the Government Accountability Office (GAO), FinCEN reported that - as of mid 2019 - 37% of transactions reported through the GTO program involved a person who was also subject to a suspicious activity report (SAR), a report that banks file with FinCEN when they suspect a transaction may be facilitating money laundering (GAO, 2020). However, because it is unknown what percentage of SARs actually reflect an illicit transaction,[7] this overlap may just reflect the tendency for banks to file SARs on similar types of transactions (large cash transfers involving corporate buyers), rather than any underlying criminality.

[7] Recent research suggests that banks in the US may file SARs 'defensively,' to avoid being punished for failing to report an illicit transaction, leading to a low signal-to-noise ratio (Unger and Van Waarden, 2009).

Figure 2: Public revisions to the Real Estate Geographic Targeting Orders (GTO), 2016–2020



Note: Figure shows the timing of public GTO announcements (dotted vertical line) by FinCEN and their implementation (solid vertical line) for each of the twenty-two counties ultimately included in the program. Not shown is the confidential GTO announced in April 2018 and implemented in May 2018, which ran until the next public GTO announcement in November 2018. Sources: FinCEN GTO announcements and GAO (2020)

3 Data and empirical framework

Our primary goal in this analysis is to identify whether the introduction of the GTO program led to a decline in the types of transactions it was targeting: corporate, all-cash purchases made at prices at or higher than the thresholds set by FinCEN. Because the policy was publicly announced and implemented, we would expect the initial GTOs to have created a significant deterrence effect. Those who would have used shell companies to buy property should substitute away into other types of behavior, for fear of detection by the authorities. Declines in economic activity have been observed following the introduction of similar information sharing regimes, ranging from beneficial ownership in partnership formation (GW, 2013) to offshore deposits (O'Reilly, Ramirez, and Stemmer, 2019).

In the rest of this section, we discuss the data we use to identify these transactions in the US property market as well as the econometric approach we take to estimate the impact of the GTOs on these types of transactions.

3.1 ZTrax residential property data

Our primary data source on real estate transactions is Zillow's Transaction and Assessment Dataset (ZTrax) (ZTRAX, 2021). Zillow aggregates data from public records at the county level using a variety of different private data providers (which are indicated in the data).[8] The ZTrax data includes separately-collected information on both deed transfers (ZTrans) and property assessments (ZAsmt), which are linked by unique identifiers.

To create our sample, we start with universe of more than 32.6 million deed transfers included in the *ZTrans* data for the period 2010-2019. We keep only direct sales, removing other types of deed transfers, such as foreclosures, second mortgages, or transfers between family members. Since GTOs do not apply to commercial properties, we then restrict our data to only include sales of residential properties.[9]

[8]Each row has an indicator for the provider that entered the data, but Zillow does not provide a dictionary to decipher the actual companies. Below we describe both how using the different data providers affects the measurement of several key variables and how we account for resulting differences.

[9]For more details on the creation of our sample, see Section A.1 in the Appendix. ZTrax is primarily a dataset of residential property transactions, preventing us from analyzing commercial properties as placebo or substitution outcomes.

3.2 Identifying and coding corporate buyers, all-cash purchases, and the use of title companies

GTOs only target real estate transactions where the buyer includes legal entities (such as corporations, partnerships, or LLCs, but excluding trusts) and that are "all-cash" (financed without the use of a loan). To investigate the effect of the GTO implementation, we first need to correctly identify whether a buyer in a transaction is an individual, corporate entity, or trust.

We use a string-matching procedure to identify whether legal entities were used to purchase properties.[10] In brief, we first build a list of common 'noise words' that are used by Secretary of State offices at the US state level to identify different types of corporations, trusts, government agencies, and religious organizations.[11] We use these keywords to code the type of buyer for all buyer names listed on the deed transfer documents associated with a transaction.Specifically, we code a transaction as having a corporate buyer if any of the *BuyerNonIndividualName* fields for a transaction contain one of the relevant *corporate keywords*, and zero otherwise. For the period we consider, corporations were the buyers of 4.5 million (13.9%) out of the total 32.6 million transactions recorded in the ZTrax single-property residential data set. Trusts were responsible for 1.1 million transactions, or 3.4% of the total.[12]

Next, we identify transactions that are all-cash. Zillow has its own field which indicates whether a mortgage was attached to the deed transfer. However, starting in 2018, it appears that some of Zillow's data providers began entering mortgages in a separate record in the ZTrax data set. We code a transaction as being a mortgage transaction if (i) Zillow lists it as including a positive or non-missing loan amount or (ii) ZTrax indicates that a mortgage was issued for the same property on the same date as the purchase in question. If neither a mortgage nor a loan amount is listed, we code the transaction as being all-cash. Using this new measure, we estimate that 14.3 million (44%) of all transactions were purchased using only cash.

3.3 Data availability and selection of counties

Unfortunately, ZTrax does not always contain comprehensive data for all transactions in all US counties. In the US, county governments are responsible for collecting data on property sales and assessments. Because of differences across state and local laws, data on transactions or sales

[10] For details on the string-matching procedure, see Section A.3 in the Appendix

[11] See the keyword dictionary in Appendix Table A.2. In most states, these departments are responsible for registering business.

[12] Zillow identifies buyers using the *BuyerNonIndividualName* or *BuyerIndividualFullName* for non-natural and natural persons, respectively. We code all buyers that are named in the *BuyerIndividualFullName* field as *Individual Buyers*.

prices are not consistently available to the public and consequently the data brokers used by Zillow to source the ZTrax dataset. For example, some counties do not publicly release data on sales prices, mortgages, and/or the use of title companies.[13] This means that some counties have better coverage (e.g. more transactions with nonmissing sales prices) than others. We thus trim our sample to remove counties that do not have a minimum level of data coverage - keeping those that have sufficient data on sales transactions and prices.

For a county to remain in the sample we use for our main analysis, we require that it meet the following three conditions. First, the county must have transaction data available for every year of our analysis period of 2015-2019. Second, sales price should not be missing for more than 25% of a county's transactions in any given year. Finally, mortgage data should not be missing for no more than 10% of that county's transactions.

Lastly, to ensure our control counties are more comparable to the counties targeted by GTOs, we only include counties with at least $2,500$ recorded sales with sales prices per year.[14] These thresholds are based off of Figure A.6 in the Online Appendix, which shows density plots for the same four indicators across all 451 counties ($2,255$ county-years: 451×5). The red-shaded curves are for Non-GTO Counties (the vast majority of the observations), and the blue-shaded curves are for GTO Counties (96 observations).

Table 1 shows summary statistics across the three different samples: (1) the full data set and (2) our data set according to the thresholds applied above.[15] As an additional robustness check, we also use samples that only include counties with no minimum sales or $5,000$ sales recorded in each and every year from 2015-2019. This removes smaller counties that are less comparable to the large, urban counties which were subject to GTOs.

3.4 Empirical Framework

Our main dependent variable of interest is the number of *all-cash corporate purchases* in county i at time t. Using shell corporations with anonymous beneficial ownership is one way to hide the true owner behind property purchases. The explicit goal of the GTOs was to make such anonymous

[13]Our approach here differs from Hundtofte and Rantala (2018), who make decisions on including data on the state level. However because real estate data are collected by county governments, significant within-state variation in missingness exists.

[14]We do not restrict our sample based on title company data availability. As discussed below, for some robustness checks, we vary our restriction on the minimum number of sales, ranging from setting no minimum or increasing the minimum number of yearly recorded sales with price information to $5,000$.

[15]Hundtofte and Rantala (2018) also restrict their sample based on the availability of *BuyerDescriptionStndCode* data in order to identify corporations and trusts. Instead, we use the variable *BuyerNonIndividualName* to classify buyers, which is available for all transactions in all states.

Table 1: Summary Statistics across Samples

	Full Dataset	Our Sample	GTO	Non-GTO
Num. States	51	40	8	40
Num. Counties	2,772	1,439	19	1,420
Sums				
Num. Purchases	32,642,210	25,032,783	3,397,732	21,635,051
Volume (bil. $)	6,779.3	6,404	1,593.82	4,810.18
All-Cash Volume (bil. $)	2,114.39	1,797.73	509.64	1,288.09
Num. Corporate All-Cash Purchases	3,628,943	2,717,429	446,334	2,271,095
Corporate All-Cash Transaction Volume (bil. $)	439.15	400.87	149.52	251.35
Num. Individual Mortgage Purchases	17,023,512	13,697,896	1,821,256	11,876,640
Individual Mortgage Transaction Volume (bil. $)	4,458.52	4,402.09	995.55	3,406.54
Means (county-month)				
Num. Purchases	196.3	289.9	2,980.5	253.9
Volume (bil. $)	0.04	0.07	1.4	0.06
Num. Corporate All-Cash Purchases	21.8	31.5	391.5	26.7
Corporate All-Cash Volume (bil. $)	0	0	0.13	0
Num. Individual Mortgage Purchases	102.4	158.7	1,597.6	139.4
Individual Mortgage Volume (bil. $)	0.03	0.05	0.87	0.04

Note: This table gives summary statistics for the samples we analyze in the paper. The left panel distinguishes between the full ZTrax data and the sample we use after applying the missingness thresholds. The right panel divides our sample into transactions occurring in either GTO-covered counties or not. Data are for the years 2015-2019 inclusive, and a county assumes GTO status if it was ever treated during the period. All volume figures are in billion USD.

purchases more difficult. If the GTOs had the intended effect, we should, therefore, observe a decrease in corporate all-cash purchases in counties subject to GTO policies. This should be especially true for high priced properties (properties transactions above $3 million were subject to the GTO requirements in all GTO counties).

Conversely, the GTOs should not lead to changes in the behavior of individual real estate buyers (real persons) using mortgage financing, as these are self-identifying persons already subject to AML due diligence procedures. We, therefore, create a second *placebo* outcome: number and price volume of *individual mortgage purchases*. Given the design of the GTO policy, we should see no changes in individual mortgage purchases associated with the GTO announcements. There are a number of months (or quarters) where the outcome measures are zero (periods in which there were no sales). Therefore, both of our main outcome measures are transformed using the inverse hyperbolic sign (IHS) function.

To estimate the effect of GTOs on county-level real estate markets, we estimate difference-in-differences models as our primary specification. We use the GTO announcement date for a respective county as the treatment date. The canonical approach when treatment is staggered has been to estimate a two-way fixed effects specification, of the following form:

$$S_{it} = \beta T_{it} + \gamma \mathbf{X_{it}} + \mu_i + \alpha_t + \eta_{it} \tag{1}$$

Where S_{it} are the IHS transformed total number sales (or in some specifications, the volume) within a category (e.g. corporate cash) in county i at time t (either month or quarter). The vector $\mathbf{X_{it}}$ is a set of time varying characteristics, μ_i are county fixed effects and α_t are period fixed effects.

There are two chief limitations to this approach. The first is that the announcements and implementation of GTOs are both staggered: there are several distinct periods in which counties are exposed to the policy. A recent, but growing literature on difference-in-differences designs has shown that both two-way fixed effects estimates and event-study designs exhibit a number of problems when the treatment is staggered and treatment effects are heterogeneous (Sun and Abraham, 2020; Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021b; Baker, Larcker, and Wang, 2021). To account for these concerns, we primarily adopt the doubly robust estimation method introduced by Callaway and Sant'Anna (2021b) and implemented in the *did* package in *R* (Callaway and Sant'Anna, 2021a). One advantage of the Callaway and Sant'Anna (2021b) method (henceforth CSA) is that it allows for the inclusion of covariates and "covariate-specific trends across groups" (Callaway and Sant'Anna, 2021b).

The CSA approach assumes that treatment is irreversible, which holds in our case, given that once a county was under a GTO order, these were never removed, only widened. Our main data set includes 18 counties that at some point become subject to the geographic targeting order. As noted above, the GTOs were implemented in different counties (and for different price brackets) at three distinct time points: March 2016, August 2016, and November 2018.

As with the standard difference-in-differences design, the most fundamental assumption required for unbiased estimation is parallel trends, in our case extended to multiple treatment groups. In the *CSA* estimation the specific parallel trends assumption depends on what comparison group is most appropriate: whether units treated at later time points (*not-yet-treated*) would be appropriate comparison units for those treated in earlier time periods.[16] In our case, we believe that the *not-yet-treated* counties are likely the real estate markets most similar to early treated counties and are thus our best comparison group. Our main results, therefore, focus on the *not-yet-treated* group as the comparison.[17]

[16] See the discussion in Callaway and Sant'Anna (2021b, 5-6) regarding assumptions 4 & 5: "Assumption 4 states that, conditional on covariates, the average outcomes for the group first treated in period g and for the "never-treated" group would have followed parallel paths in the absence of treatment. Assumption 5 imposes conditional parallel trends between group g and groups that are "not-yet-treated" by time t + δ"

[17] Our results are largely unchanged if we rely on the less restrictive assumption and only use the *never-treated* units in the comparison group.

One problem for the parallel trends assumption in our case is how extraordinary the real estate markets are in counties that received GTO orders. For example, it is unlikely that the number or volume of real estate transactions (particularly corporate all-cash transactions) in Manhattan follow a similar trend to that of Dickinson, Iowa. In fact, for a number of counties, there is no trend to observe in corporate all-cash purchases, as it stays at zero throughout the whole study period. For our preferred specifications, we therefore include two pre-treatment covariates: county GDP in 2015 (log transformed) and the county-wide median sales price for 2015 (log transformed). In addition, we also show the results for a number of different samples and covariate combinations.

3.5 Different approaches to county and price bracket aggregation

For our primary analysis, we ask whether GTOs led to a decline in either the number or total dollar volume of corporate all-cash purchases in targeted counties. Because a treated unit in this analysis is a county, and treatment is an absorbing state, counties are considered treated whenever they are first subject to a GTO. This allows us to identify the initial impact of GTOs on the corporate cash segment of the entire market.

There are a couple of limitations to this approach. The first is that, as shown in Figure 2, different counties faced different price thresholds. For example, the first GTO applied to transactions above $1 million in Miami and transactions above $3 million in Manhattan. This affected around 7% of corporate, cash sales and 38% of corporate cash dollar volume in Miami, but 38% and 78% of the number and dollar volume of corporate cash transactions in Manhattan. Thus a county-wide analysis may obscure the full impact of the program because it includes transactions that were not being targeted, and does so differentially across counties.

The second limitation is the fact that the same counties faced different price thresholds over time: as in November 2018, the threshold was lowered to include transactions above $300,000 for all affected counties. Thus some markets may have been affected multiple times: when they were first subject to a GTO and then again when they faced a reduction in the threshold. In our county-level analysis, we will miss out on this second effect.

As stated above, for our main analysis, we aggregate our data to the county-month, ignoring any price thresholds, to estimate the aggregate impact on corporate cash purchases. Here the treatment indicator is coded 1 after the *first* GTO announcement for a given county. Then, to better identify the impact of GTOs on transactions that fall within the price range being targeted, we take two additional approaches to focus on transactions that are more likely to have been affected by the GTOs.

Alternative approach #1: aggregating within specific price ranges

Our first alternative approach is to aggregate sales into different 'price brackets' or bins reflecting all transactions for a range of prices. We first do this using $500,000 bins or 'price brackets', from $1 to $500,000, $501,000 to $1 million and so on. We code all transactions above $5 million into one bin. This allows us to analyze the data above different sales price cut-offs and establish comparisons in purchase patterns of similar properties before and after the GTOs were introduced. In addition to using $500k price brackets, we also estimate results using sales price brackets of widths that correspond closely to the GTO policy thresholds but are of different sizes: 1. ($0 to $0.3mil); 2.[$0.3mil to $0.5mil); 3. [$0.5mil to $1mil); 4. [$1mil to $1.5mil); 5. [$1.5mil to $2mil); 6. [$2mil to $3mil); 7. ≥$3mil.

For both these approaches, the unit of analysis is a county-bracket (e.g., purchases between $500,000 and $1 million in Broward County). A county-bracket is considered treated whenever a GTO is announced (or comes into effect) for that specific bracket. The implicit assumption behind these estimations is that untreated price brackets in treated counties (for example, transactions between $500k and $1 million in Miami-Dade) are valid controls for brackets that are currently treated. This means that there should be not spillovers between brackets: that deterrence effects do not push people who would have bought a $1.4 million dollar property in Miami-Dade into buying two $700k properties instead. In practice, through our other estimation strategies we do not find any evidence of this kind of behavior, and, if it did exist, it would bias our results towards finding a negative impact. This approach also assumes that there are no "chilling effects," that those buying property below the GTO threshold in targeted counties are not dissuaded from continuing to do so. However, we would expect general chilling effects to manifest in our primary analysis, and we find little evidence of this.

There are significant trade-offs when it comes to aggregating the data into different sales price brackets. One problem with the county-bracket aggregation is that the counties differ substantially on the number of transactions that take place across the different price-brackets, thus introducing implicit differential weights. This is particularly problematic for total volumes based on summing purchase prices, which naturally are much larger in higher brackets. To best account for these difficulties, we estimate our main models on several different data sets and aggregation levels. We primarily focus on the county-month analysis.

Alternative approach #2: trimming out low value transactions

Additionally, we will also present results for the number of purchases from regression models where we drop all transactions below different price thresholds before aggregating corporate-cash purchases for every month within a county. We try this two different ways: first dropping all transactions at each subsequent $500k threshold (e.g. dropping all of those below, $500k, $1m, $1.5m) and also following the specific thresholds set by the various GTOs ($300k, $1m, and so on). By trimming out low-value transactions, we compare high value transactions across counties that are treated by GTOs to counties that are not treated by GTOs.

3.6 Final data aggregation and treatment dates

To aggregate transactions by month or quarter, we follow Zillow's guidelines,[18] and use a transaction's document date (or if missing recording date) to code the month or quarter in which the transaction took place. We then collapse the transaction-level data to either the county-month level, or the county-month-price bracket level for the period 2015-2019. Prior to collapsing, we trim out extreme price values.[19]

We focus on two main outcomes for our main analysis: either the count or the dollar total (the 'price volume') of all purchases at the county-month (or county-month-price bracket) level. We calculate this outcome separately for both corporate all-cash purchases (which were targeted by the GTOs) and purchases by individuals using mortgages, which we use as a placebo check.

As Figure 2 shows, we observe three primary GTO announcements in our sample over the time period analyzed: January 2016, July 2016, and November 2018.[20] In general, the GTOs (or announced changes) go into effect within less than a month of the announcement. Only the first GTO exhibits a two months lag between announcement (January 13, 2016) and when the policy goes into effect (March 1, 2016).

[18] See: `https://www.zillow.com/research/ztrax/ztrax-faqs/`.

[19] Close inspection reveals that some sales price values are highly likely to be entered incorrectly. Prior to our final aggregation, we therefore code the sales price as missing for transactions where the recorded price is zero or if the sales price is outside the *county specific* 0.25th or 99.75th percentile in sales prices. We do not drop these transactions with missing sales prices from the data entirely, but instead run additional robustness checks including them in the sample as transactions without sales price.

[20] In addition, Honolulu, HI is subject to the GTO starting in August 2017. Due to data missingness, however, we do not include Hawaii in our sample. Also, as indicated above, a confidential GTO was implemented in May of 2018, which we check for robustness.

For our main analysis we use the date of the announcement as the relevant treatment date. We believe using the announcement date to code treatment is preferable to using the implementation date for theoretical and methodological reasons. First, we might expect a behavior change in anticipation to the policy. In particular, given the uncertainty with respect to closing dates in real estate, it is likely that behavior changes immediately in response to the announcement. Second, using event study graphs will allow us to detect potential immediate effects to the policy announcement, as well as potential effects that only occur after the policy comes into effect. We, therefore, see the announcement date as the more conservative choice in terms of treatment timing.

4 Results

4.1 Impact of the GTOs on corporate all-cash purchases

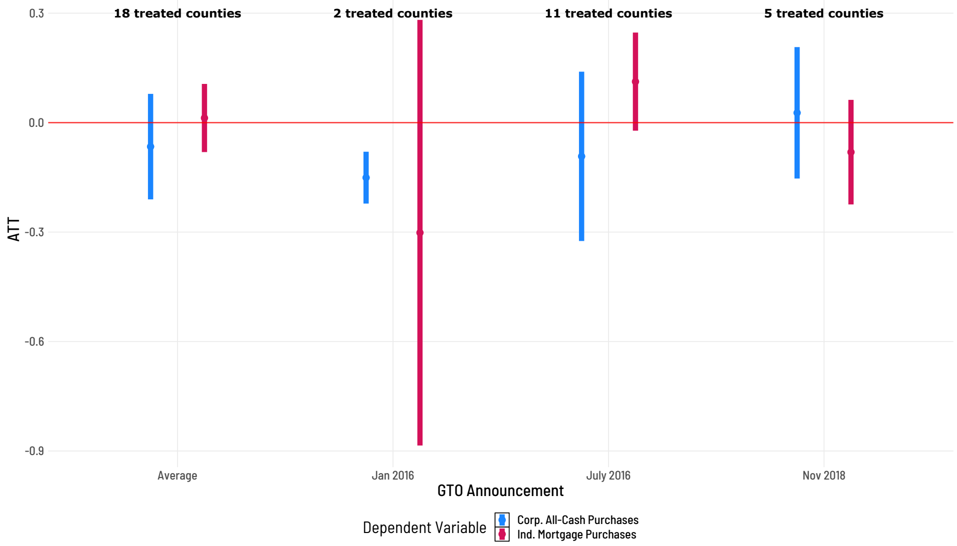
Figure 3 shows the results from our primary model specification estimated at the county-month model for the main outcome of interest (*corporate all-cash purchases* - blue) and the placebo (*individual mortgage purchases* - red). The top plot, Figure 3(a), shows the average treatment effect on the treated (ATT) for each of the three public GTO announcement dates and averaged across all groups.[21] Given that we would expect an immediate response to the introduction of the GTOs, we limit the calculation of the ATT to 12 months post-treatment. The aggregate ATT across all three GTOs for corporate all-cash purchases is -0.066. The effect is not statistically different from zero, with the 95% confidence interval ranging from -0.21 to 0.07. We do observe some heterogeneity in the estimated treatment effect across the different GTO groups. We find a negative and significant effect of the first GTO, which covered Miami-Dade and Manhattan counties. It is important to note, however, that the estimated effect is only based on two observations and should be interpreted with caution. For our placebo outcome, the number of individual mortgage purchases, the average ATT is 0.013, with the 95% confidence interval ranging from -0.09 to 0.11. Surprisingly, the estimated ATT for the first GTO is substantially more negative for the placebo outcome but with a very large confidence interval (and insignificant).

Figure 3(b) shows the dynamic event-study estimates, the "average effect of participating in the treatment over the first e' periods of exposure to the treatment" (Callaway and Sant'Anna, 2021b, 12). Again, we show effects calculated for 12 months pre- and post-treatment. The dynamic event study coefficients show no clear effect of the GTO policies on the number of cor-

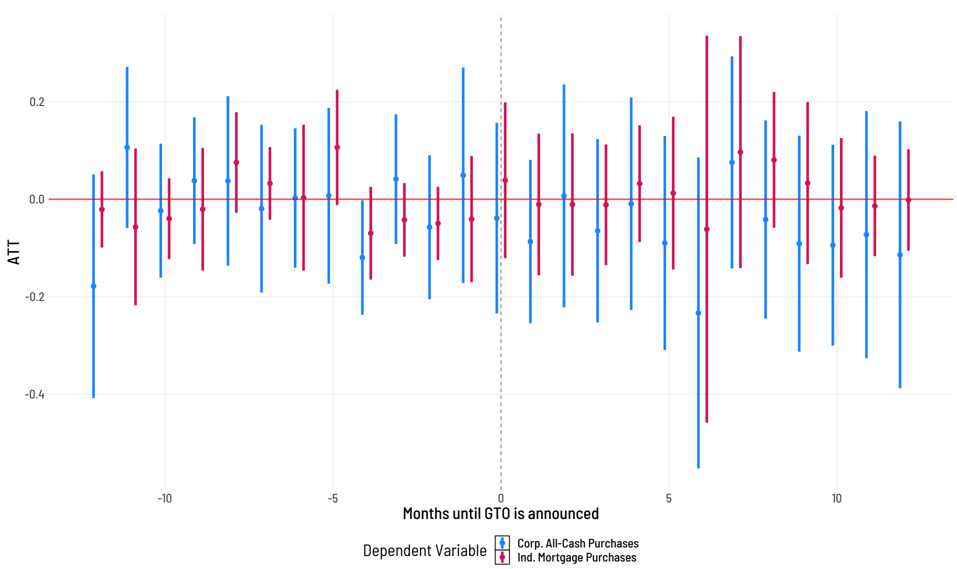
[21] Denoted Θ^{O}_{Sel} in Callaway and Sant'Anna (2021b)

Figure 3: Overall impact of public GTO announcements on number of corporate-cash purchases (CSA estimation)

(a) Average and group-specific ATT estimates



(b) Event-study estimates



Notes: Figure 3(a) shows estimates of the average treatment effect on the treated (ATT) for the GTO announcement, aggregating all the group-specific effects together (average) and group-specific ATT estimates for the January 2016, July 2016 and November 2018 announcements, respectively. The outcome of interest is the inverse hyperbolic sine of the number of monthly corporate cash purchases (blue) and individual mortgage purchases (red). All estimates calculated using Callaway and Sant'Anna's (2021b) doubly robust estimation method. 95% confidence intervals shown. Sample includes 18 GTO counties. Group ATTs are calculated based on 12 pre- and post-treatment months. Figure 3(b) shows the event time estimates for both dependent variables, averaging over all three GTO announcements.

porate all-cash purchases in the following twelve months. As shown in Figure 3(b), some of the event time estimates prior to the GTO announcements are above and below zero, we do not observe systematic pre-treatment effects and nothing that indicates clear pre-trends. In Figure B.7 in the Appendix, we present the dynamic event time ATTs for the maximum pre-treatment exposure with all three GTO groups used for estimation.

We then turn to our alternate methods of aggregating the data, comparing the county-level analysis above to one where we aggregate (and assign treatment) to specific price brackets. In Figure 4(a) we show the average group ATT for the number of corporate all-cash purchases and individual mortgage purchases for the model above (county-month, no brackets) and the same models estimated on the $500k or GTO-threshold county-price bracket-month data. Table B.8 in the Appendix shows the full results for the estimations based on price-bracket aggregations. As described above, these alternative approaches focus on the impact of the GTOs on purchases within the specific price brackets that were targeted by the policy.

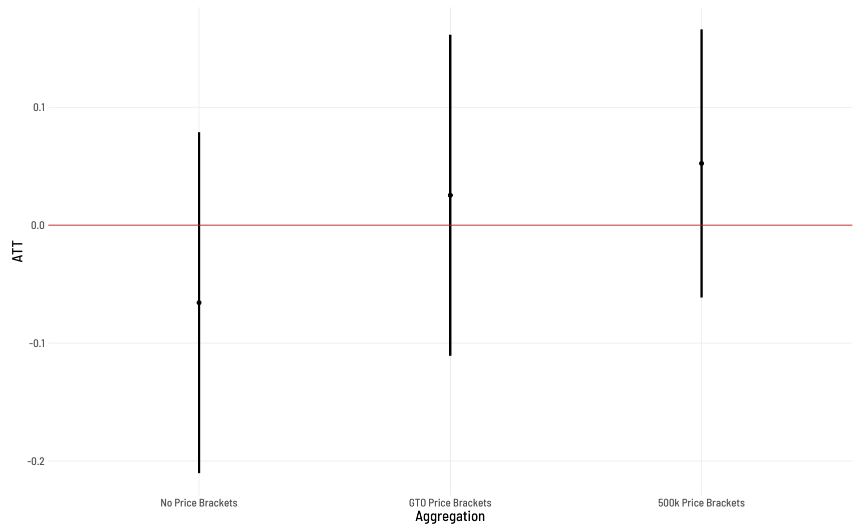
As one can see, the group specific effects vary across the different aggregations. For both models estimated on data aggregated at the price-bracket, the overall group ATT is actually estimated to be positive, though both are statistically insignificant. The estimate based on the $500k brackets are slightly larger compared to the GTO threshold brackets. There is no evidence in either of these models, however, that the GTO announcements led to a significant decrease in corporate all-cash purchases.

We then turn to our second alternative approach: estimating the effect of the GTOs at the county level, but restricting our aggregation to purchases at different price levels. Figure 4(b) shows the average group ATT for models estimated on data aggregated at different minimum sales prices. Each point and line shows the average group ATT and 95% confidence interval for a given sample. The leftmost estimate is based on the full sample, including all transactions, even those with missing sales prices. For the next estimate we drop transactions with missing sales prices. Moving to the right, we then subset further, only including transactions with sales prices above $500k, then increasing the threshold for inclusion by $500k for each model. Overall, there is once again no clear evidence that the GTOs led to a significant decrease in corporate all-cash purchases. Figure B.8 in the Appendix shows the same overall group average ATTs but for the data subsets by the different GTO thresholds. The results are quite similar.

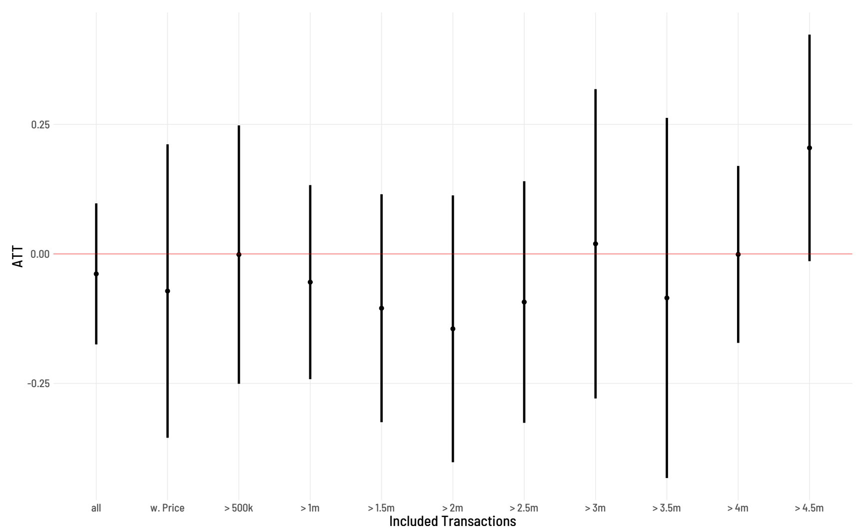
If the GTOs were effective in reducing money laundering in luxury real estate, we might expect to observe a stronger effect in higher valued properties. As one can easily see in Figure 4(b),

Figure 4: Overall impact of public GTO announcements on the number of corporate all-cash purchases across different estimation strategies (CSA estimations)

(a) County-level treatment versus county-price bracket level treatment



(b) County-level aggregation versus county-level aggregation keeping only high-value transactions at different thresholds.



Notes: Figure 4(a) shows estimates of the average treatment effect on the treated (ATT) across the three GTO announcements for the models estimated on data with different aggregation levels. The leftmost estimate is the average group ATT and its 95% confidence interval for the without price brackets. Next are the estimates based on the model with GTO threshold price brackets and the $500,000 price brackets. None of the results provide evidence that the GTOs had significant effects on corporate all cash purchases. Figure 4(b) shows the average group ATT and its 95% confidence interval for the non-bracket aggregation when we vary the sample by increasing a minimum sales price above which transactions are included in the aggregation. Again, there is no clear evidence of a negative effect of the GTOs. All estimates calculated using Callaway & Sant'Ana's (2021b) doubly robust estimation method. 95% confidence intervals shown. Sample includes 18 GTO counties. Group ATTs are calculated based on 12 pre- and post-treatment months.

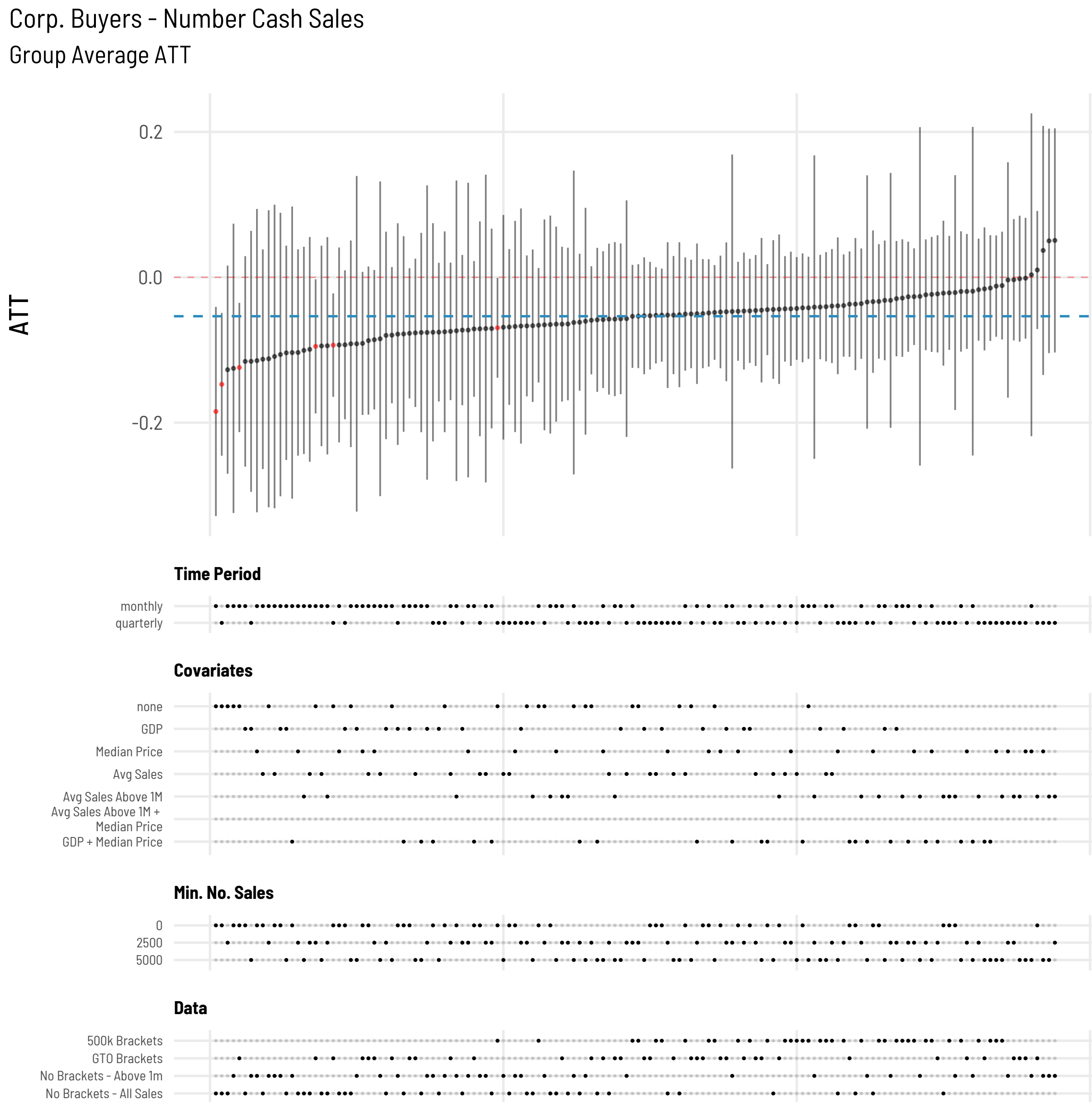
however, we do not observe any clear pattern in the estimated effect size. In fact, the estimated effect is positive, i.e., opposite the expected direction, for the highest valued transactions.

Figure 5 shows the average group specific ATT and its 95% confidence interval for a number of different specifications in models with the IHS-transformed number of corporate all-cash purchases as the dependent variable. We vary the model specifications across a number of parameter combinations. Specifically, we estimate the doubly robust CSA method on different combinations of several sets of pre-treatment covariates, quarterly or monthly data, different inclusion thresholds for minimum county sales, as well as the county-month data with all transactions, only transactions above $1 million, the GTO price bracket, and the $500,000 price bracket data. Points are ordered based on the average group ATT estimate and shown in red if the 95% confidence interval does not include zero. The median average group ATT is -0.05, similar to our main estimate above, shown as the blue dashed horizontal line. While the estimated ATTs vary, they do so in a clear manner and most are quite similar. Only six out of the 144 estimates are statistically significant. All six estimates that are significant are from models without any included pre-treatment covariates and all but one are from models without a minimum number of sales threshold, i.e., these are the specifications we believe are least appropriate.

Table B.5 in the Appendix shows the results for our main model without sales price brackets but aggregated at quarterly time intervals. Overall the results are quite similar; we do not find strong evidence of a substantial effect of the GTO policies. Furthermore, as noted above, there is some reporting that the last GTO announcement was secretly made in April 2018 and only publicly announced in November. As a robustness check, we therefore estimate our main models with the treatment re-coded such that the last GTO is announced in April instead of November. The results are presented in Table B.6 in the Appendix. There is no evidence that the secret announcement masked the impact of the GTO in our main models. In Table B.7 in the Appendix, we present the results from our main models when using only the never-treated counties as comparison groups in the CSA models. Again, the results are quite similar to those presented above.

Figures B.10 and B.11 show the overall group average ATT for the number of purchases and price volume models when we exclude one treated county at a time. Similarly, Figures B.12 and B.13 show the group average ATT when each of the three treatment groups are being excluded from the model. As one can easily see, the point estimate and confidence interval around the estimate changes little when specific counties or whole treatment groups are excluded. Lastly,

Figure 5: Specification Curve for Average ATT in County-Month Models (CSA estimation)



Notes: Figure shows the average group specific ATT and its 95% confidence interval for a number of different sample specifications with corporate all-cash purchases as the dependent variable. In particular, we vary the model specifications across a number of parameters. We estimate the same models with different minimum sales thresholds, different data aggregations (no brackets, no brackets with only transactions above $1 million, GTO price brackets, and $500k brackets), with different pre-treatment covariates included in the estimation, and alternating between quarterly and monthly aggregations. Points are ordered based on the average group ATT estimate and shown in red if the 95% confidence interval does not cover zero. The median average group ATT is -0.05, similar to our main estimate above, shown as the blue dashed horizontal line. While the estimated ATTs vary, they do so in a clear manner and most are quite similar. Only six out of the 144 estimates are statistically significant, and all six are from models without any included pre-treatment covariates, all but one of the significant estimates are based on no minimum sales data threshold.

Figure B.9 in the Appendix shows the dynamic event study estimates for our main outcomes of interest as estimated by the augmented synthetic control method introduced by Ben-Michael, Feller, and Rothstein (2021). Again, there is no clear evidence of a strong overall effect of the GTO on corporate all-cash purchases.

4.2 The impact of GTOs on corporate cash transactions more likely to be illicit

One limitation of our main analysis is the noisiness of the data, driven by heterogeneity in corporate cash transactions both across counties and across time. This leaves the possibility that our inability to pin down precise effects is driven in part because changes in a small number of illicit transactions are being obscured by large fluctuations in perfectly legal corporate crash transactions. In this section we attempt to focus on transactions that are more likely to have involved illicit money, under the assumption that these will be more responsive to the introduction of GTO reporting requirements.

We first consider purchases made by companies that are more likely to be anonymous shell corporations: those that do not engage in any sort of economic activity aside from facilitating transactions and buying and holding assets. To better identify transactions that involve shell companies (and are thus more likely to have involved illicit money), we consider purchases by companies with the following characteristics:

1. Created using formation agents, which are corporate service providers that specialize in setting up shell companies and have been associated with criminal and corruption cases in the past (Goodrich, Cowdock, and Simeone, 2019).

2. Companies incorporated shortly prior to the real estate transaction taking place, under the assumption that the company was only created to facilitate the transaction or hold the real estate.

3. Companies formed in US states that, due to local legislation, have allowed for anonymous ownership in the past and are associated with illicit finance.[22]

To create corporation types, we merge the universe of corporate buyers in the ZTRAX data

[22]We define these states as Delaware, Nevada, and Wyoming. These three states were identified by both Financial Action Task force and FinCEN as being prone to abuse, due to their lax position towards anonymous shell companies (Network, 2006; FATF, 2013b). A former special agent for the US Treasury also singled the three states out in 2013 as being "nearly synonymous with underground financing" (Cassara, 2013). With the passing of the 2021 Corporate Transparency Act, all anonymous ownership of domestic companies will (in theory) be eliminated.

with the universe of firms in the Open Corporates data set.[23] We apply the same standardizing algorithm to the legal entity names from both the ZTrax and the OpenCorporates, and then perform exact string matching based on name and state.[24] This process was able to assign OpenCorporates unique identifiers to 12.7 million of the 17.8 million legal entities appearing as buyers in the Ztrax dataset from 2010-2019. We then create the four types of corporations mentioned above by coding fields from the OpenCorporates data. This requires that we assume that the match rate between the ZTrax data and OpenCorporates is orthogonal to the introduction of the GTOs, which we believe is quite reasonable.

Companies were coded as using formation agents if that agent had registered at least 1,000 companies in the OpenCorporates dataset. We compared company incorporation dates to the real estate transaction dates to code whether the company was formed just prior to the property purchase. Specifically, we code companies as 'newly' incorporated if they are registered fewer than 183 days (six months) prior to the transaction date. Finally, we use data on the state of registration to determine whether a company was located in a secrecy jurisdiction.Next, we aggregate all-cash purchases and total all-cash purchase price volume for each of the four company types at the county-month level.

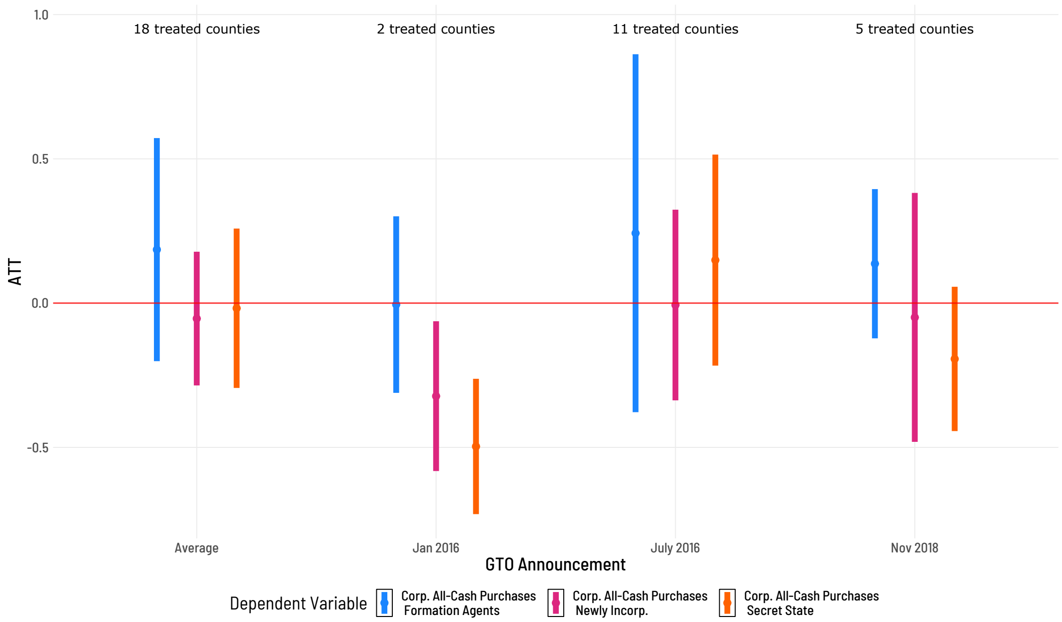
Tables B.9 and B.10 in the Appendix show the main results – the average group ATT and GTO specific ATTs – for the number of purchases and price volume, respectively. There is no clear or consistent evidence that GTO policies changed all-cash real estate transactions involving corporations registered using formation agents. In fact, the overall estimate is positive. The average group ATT for corporate all-cash transactions by newly incorporated corporations or those from secretive states is negative but both small and insignificant. However, we find a large, significant effect for the first GTO: an approximately 30% decline in purchases using newly-incorporated companies and a roughly 50% reduction in those involving US states with high degrees of corporate secrecy

Overall, given that all-cash transactions from these types of corporations are the *most likely cases* for detecting any effect of GTOs, we interpret these results again as cautious evidence that the GTOs are unlikely to have had large aggregate impacts on illicit monetary flows into real estate. Again, however, there is some evidence that the first GTO may have made an impact,

[23]OpenCorporates aggregates data from the business registries of 49 US states (plus the District of Company) into a unified standard. Illinois does not make its business registry accessible public. https://opencorporates.com/registers

[24]At the state level, legal entities register with unique names.

Figure 6: Overall impact of public GTO announcements on more 'suspicious' purchases (CSA estimation)



Notes: Figure shows estimates of the average treatment effect on the treated (ATT) for the GTO announcement, aggregating all the group-specific effects together (average) and group-specific ATT estimates for the January 2016, July 2016 and November 2018 announcements, respectively. The outcome of interest are the inverse hyperbolic sine of the number of transactions we identify as particularly suspicious. The is no evidence of a significant overall decline in these types of transactions, though the estimates for the first GTO suggest that corporate all-cash purchases by newly registered companies or those registered in secretive states may have declined. Though note that these estimates are only based on two observations (Miami & Manhattan). All estimates calculated using Callaway & Sant'Ana's (2021b) doubly robust estimation method. 95% confidence intervals shown. Sample includes 18 GTO counties. Group ATTs are calculated based on 12 pre- and post-treatment months.

though it is important to keep in mind that the group only includes two treated counties (Miami-Dade and Manhattan). We will investigate these two counties in more detail in Section 4.4.

4.3 Evasion of GTO reporting through substitution into other forms of purchases or lower price brackets

4.3.1 Evasion through other types of entities or by avoiding the use of title companies

In our main specifications we have not found a clear or consistent effect of GTO policies on all-cash corporate purchases. An additional observable implication of an impact of the GTOs might be buyers adjusting their behavior in order to avoid being subject to the GTOs. For example, anonymous buyers might stop using corporations to hide their identities or shift away from all-cash purchases. Similarly, since the policies apply at specific price thresholds, purchase prices might be adjusted. In this section, we investigate these possible substitution effects. First, we test whether GTOs led to an increase in all-cash purchases by buyers using trusts instead of corporations, as trusts were not covered by any of the GTOs. Second, one possibility of avoiding the reporting of transactions to FINCEN is using mortgages instead of all-cash purchases.

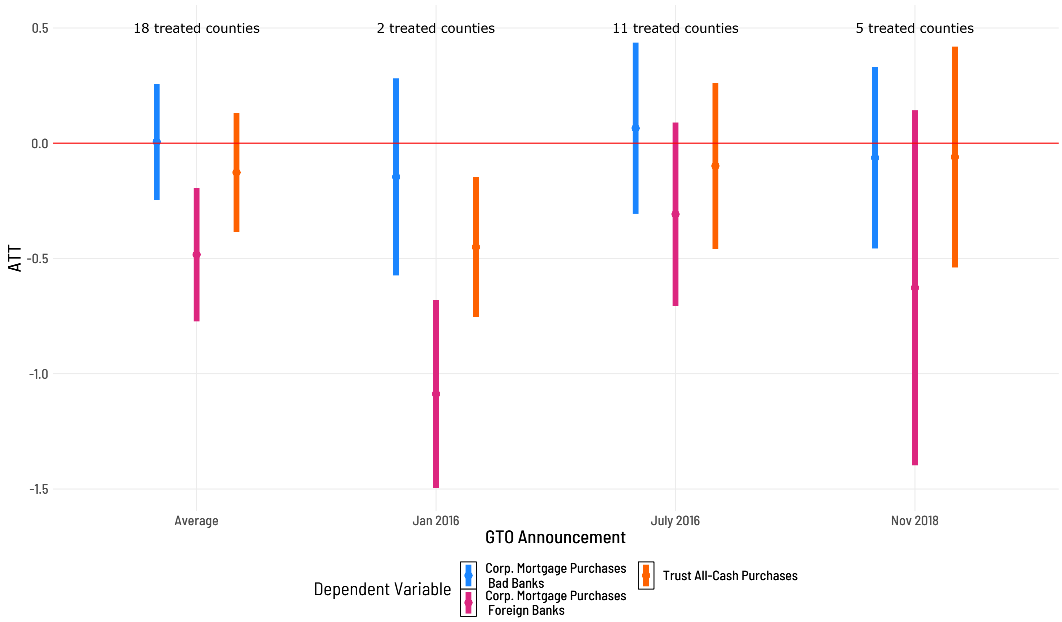
Even though banks are required to conduct due diligence checks in their customers, buyers may target those banks with the weakest levels of compliance, relying on short-term mortgages with the main goal of avoiding all-cash declaration. We, therefore, estimate whether corporate buyers moved to using mortgages from banks with a higher risk of compliance failure or foreign banks. We define banks with a higher risk of compliance failure (bad banks, for short) as those that have been subject to an enforcement action by a US financial regulator since the year 2000. We describe in Section B.3 in the Appendix how we construct this list.

Tables B.11 and B.12 show the main results with respect to substitution patterns for both the number of purchases and the price volume, respectively. Figure 7 shows average group ATTs and GTO announcement specific ATTs for the total number of purchases (IHS) for all-cash purchases by trusts, corporate purchases with mortgages from bad banks, and lastly corporate purchases with mortgages from foreign banks. We do not see any evidence of a positive effect of the GTOs on any of the potential substitutes. The effects estimated are negative. Contrary to expectations, we find a negative and significant overall group ATT on corporate purchases using mortgages from foreign banks. We also find a negative effect of the first GTO on all-cash trust purchases, though recall this is only based on two counties (Miami-Dade and Manhattan).

Table B.12 shows the results from the same models with purchase price volume (IHS) as the dependent variables. As the table shows, there is some evidence that total price volume of purchases using *bad banks* increased. Once again, we only find a negative significant overall group ATT for corporate purchases with mortgages from foreign banks. As with number of purchases, this indicates that purchases using mortgages from foreign banks actually decreased in response to the GTO, opposite to what one would expect. All other 95% confidence intervals for the average group ATT and GTO announcement specific ATTs include zero.

The GTO policies require title insurance companies to report all-cash transactions with legal entity buyers to FinCEN. One potential strategy to avoid reporting under the GTO would be to bypass title companies. Although nearly all institutional lenders mandate individual borrowers take out title insurance policies to protect against financial losses, title insurance for all-cash buyers is voluntary. To identify whether GTO implementation led to changes in the use of title

Figure 7: Overall impact of public GTO announcements on potential substitutes (CSA estimation)

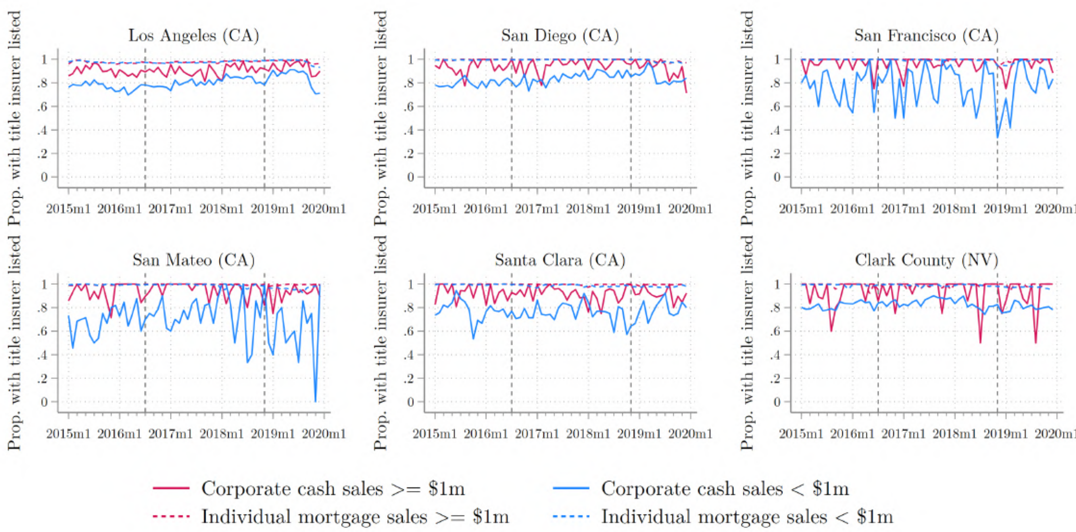


Notes: Figure shows estimates of the average treatment effect on the treated (ATT) for the GTO announcement, aggregating all the group-specific effects together (average) and group-specific ATT estimates for the January 2016, July 2016 and November 2018 announcements, respectively. The outcome of interest are the inverse hyperbolic sine of the number of transactions we identify as potential substitutions to corporate all cash purchases. If the GTO policies had the expected effects, we would expect an increase in purchases of these *substitution* categories, i.e., positive effect estimates. We do not find any positive ATTs, in fact we observe a negative and significant average group ATT for corporate purchases using mortgages from foreign banks. We also find a negative effect of the first GTO on all-cash purchases using trusts. All other ATT estimates are quite close to zero and the 95% intervals for the all of the ATTs cover zero. All estimates calculated using Callaway & Sant'Ana's (2021b) doubly robust estimation method. 95% confidence intervals shown. Sample includes 18 GTO counties. Group ATTs are calculated based on 12 pre- and post-treatment months.

companies or attorneys, we measure whether title companies (or alternatives) were used in a given transaction. To do so, we code a binary indicator if the ZTrax variable *TitleCompanyName* was missing or had a value of "None Available", Zillow's indicator for the use of title insurance.

Unfortunately, full data on title companies is missing for most counties in the ZTrax dataset; we observe this through the substantial number of transactions where institutional lenders provide mortgages but no title company is reported (as is mandated). Therefore, we can only examine changes in the use of title companies in six counties covered by the GTOs: Los Angeles, San Diego, San Francisco, San Mateo, Santa Clara, and Clark County. This small sample size prevents the estimation of our preferred difference-in-differences models; instead we plot the raw data in Figure 8 to look for potential breaks around the introduction of GTO policies. In none of the six

Figure 8: Title company coverage for six counties affected by Geographic Targeting Orders



Notes: Figure shows the percentage of property sales where title companies were used in six counties that were subject to GTOs in July 2016 and November 2018 (dotted lines show the months of announcement). The dotted lines (both with near 100% title company usage) track individual mortgage purchases above (red) and below (blue) $1 million. The solid lines track corporate all-cash purchases above and below the same thresholds

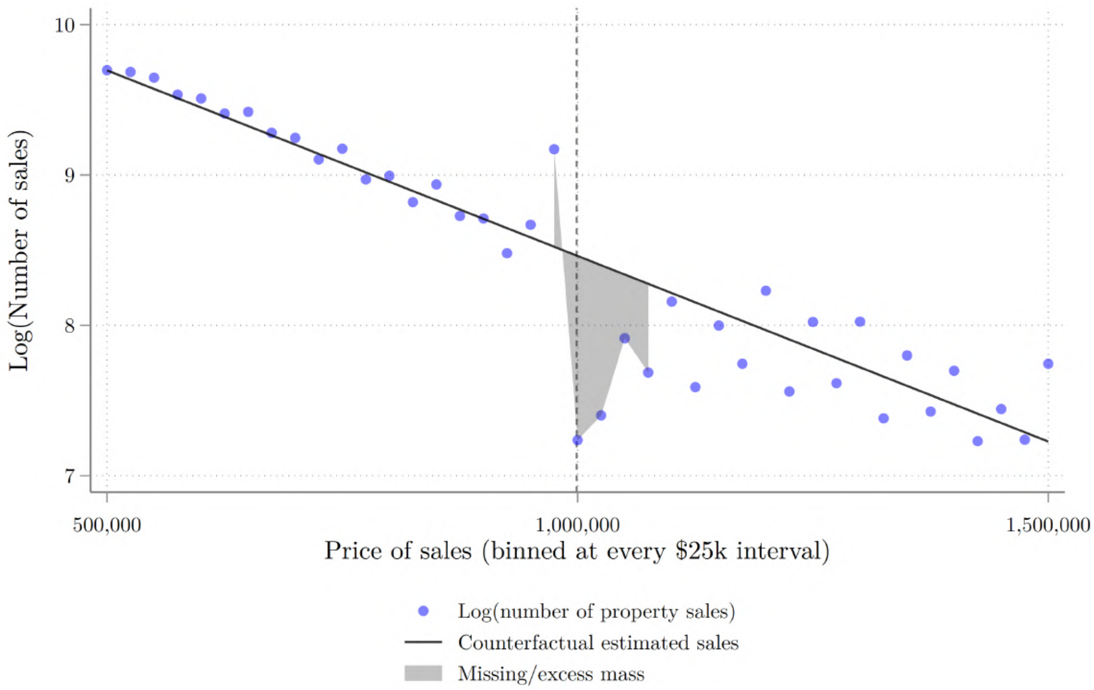
counties do we see a drop in the use of title companies following the GTOs, either for corporate all-cash purchases above or below $1 million thresholds. Although the lack of data for the entire group of affected counties prevents us from definitely stating that corporate buyers evaded the GTOs by refusing to engage title companies, what we can observe from available data suggests little such evasive behavior.

4.3.2 Testing for evasion into lower price brackets by examining bunching behavior

As we have described above, Geographic Targeting Orders do not only increase the probability that a purchase funded through illicit means will be detected, but they also create a price threshold over which that probability increases discontinuously. If a company purchases a luxury apartment in Miami in mid-2017 for $1,000,001, in cash with title insurance, then the insurance company will be required to report beneficial ownership information to FinCEN. But if that same purchase is made for $999,999 then no report is made.

If GTOs create a credible threat of detection, those still wishing to purchase in affected markets may instead opt to purchase a property just below the reporting cutoff. This could both create excess demand for properties that are close in price to the cutoff, but also create incentives for buyers to negotiate a lower official purchase price, by either conceding on other dimensions

Figure 9: The impact of the mansion tax on price bunching in NYC purchases



Note: Figure shows the log of all recorded purchases in ZTrax in New York City between 2010-2020, aggregated at every $25k interval. The black line shows the counterfactual density from a regression of the following form: $Log(sales)_p = \beta B_p + \sum_{b=975k}^{1075k} D_b + e_p$, where the number of sales is regressed linearly on the binned price, with indicator variables for the bins that excess bunching and missing mass is observed. The counterfactual density is then estimated using the estimate of β, thus excluding the influence of the bunched/lower density bins.

(e.g. the timing of the purchase) or by making unrecorded side payments to the seller.

There is an extensive literature that examines "bunching" behavior in respond to discontinuous changes in payoffs. Particularly relevant to this study are a number of papers showing that notches - discontinuous changes in transaction taxes- lead to bunching below the relevant cutoff (Kopczuk and Munroe, 2015; Slemrod, Weber, and Shan, 2017; Best and Kleven, 2018).

For example, Kopczuk and Munroe (2015) find that the "mansion tax," a 1% transaction tax levied on all residential property sales valued at $1m or more in New York State and New Jersey, leads to substantial bunching of the reported price, with an excess number of properties being sold just below the threshold, as well as a missing mass of fewer properties being sold just above it. Even though Kopczuk and Munroe (2015) examine transactions taking place between 2003-2011, we find the effect of the mansion tax is still evident today in the ZTrax data covering New York City (See Figure 9).

Do GTOs induce those who want to make a purchase through a corporation in cash to bunch around the GTO threshold? To investigate this, we calculate the share of corporate cash purchases at each $25k price point (e.g. a purchase at $330,000 would be coded as $325,000). We then graph the share of purchases in each bin for each GTO threshold separately, for counties where a given GTO threshold was active, only on the dates when it was active.

We graph these results in Figure 10. For comparison, we also show the density for all corporate cash purchases on dates prior to the date the GTO came into effect. If bunching behavior was prevalent, it would manifest as excess mass to the left of the threshold, and missing mass to the right of threshold, particularly relative to purchases prior to the GTO coming into effect. Across all five thresholds we see no strong evidence of any bunching.

While this does not rule out the substitution of lower-value properties for higher value properties, it does suggest that, on the margin, buyers looking to avoid the scrutiny generated by GTOs are not doing so by manipulating the purchase price in the area around the threshold.

4.4 Digging into potential hotspots: Miami-Dade and Manhattan

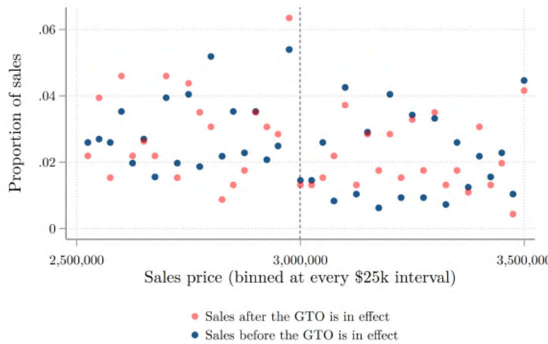
While there is no strong aggregate evidence that there was a decline in corporate cash purchases in GTO-affected counties, Figure 3 does suggest there may have been a modest decline at the time of the first GTO announcement in Miami and Manhattan - with the former being the county driving most of the observed negative effect. Similarly, Figure 7 suggests there may also have been a decline in purchases made through newly-incorporated shell companies incorporated in US states with a high degree of secrecy.

To explore this possibility descriptively, we present purchase volumes for the first two GTO countries, Miami-Dade and Manhattan, in Figure 11. Each figure shows the total dollar volume of all corporate cash purchases above the GTO threshold (in red) versus below the threshold (in blue). We also graph individual mortgages purchases above and below the threshold (using lighter versions of both colors), our placebo outcome of choice since mortgages were not subject to any additional scrutiny under the GTO program.

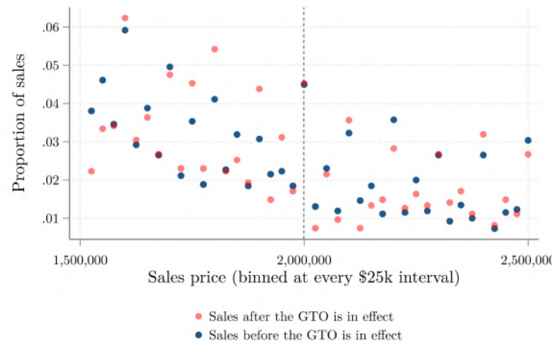
In Miami-Dade, corporate cash and individual mortgage volumes roughly track each other (in their respective price brackets) until the announcement and the implementation of the first GTO in January and March, 2016, respectively. Corporate cash volumes decline substantially in the months following the announcement of the first GTO, a decline that is not observed in any of the comparison groups. This suggest that there may have been a small, albeit temporary, decline in Miami around the announcement of the first GTO.

Figure 10: (Non)evidence of bunching of corporate-cash purchases around the GTO thresholds

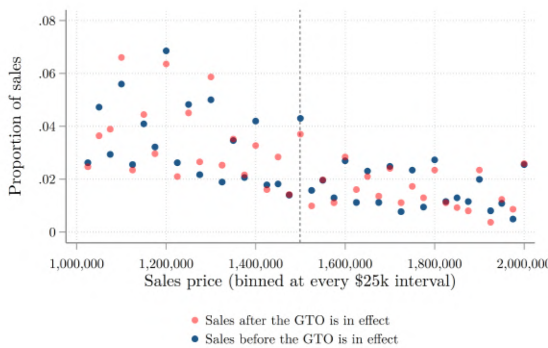
(a) \$3m



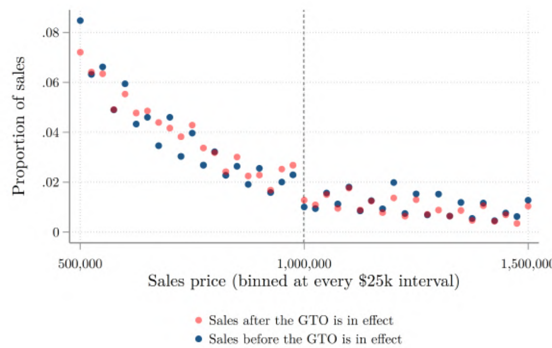
(b) \$2m



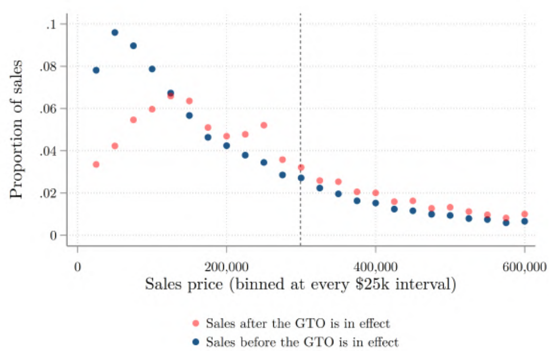
(c) \$1.5m



(d) \$1m



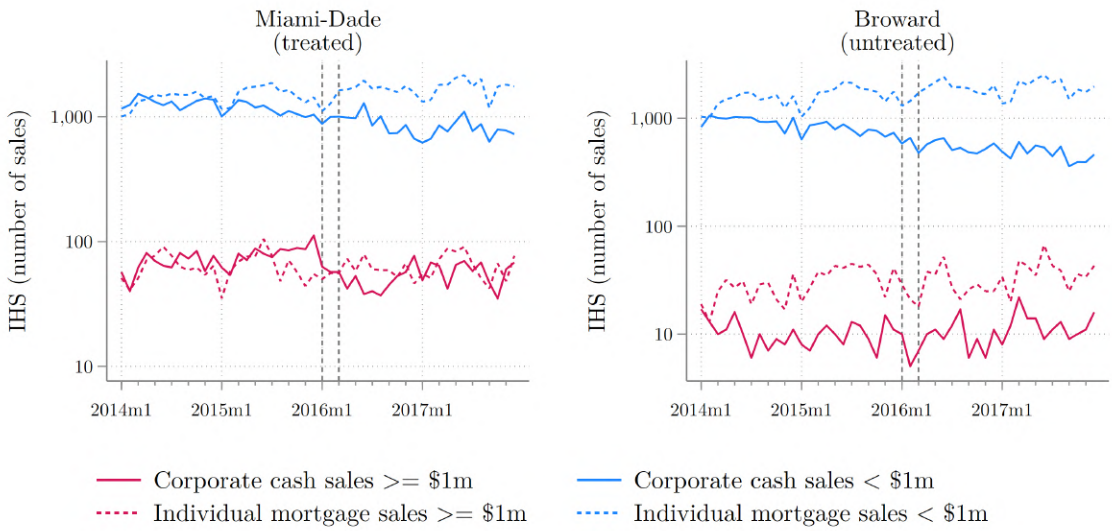
(e) \$300k



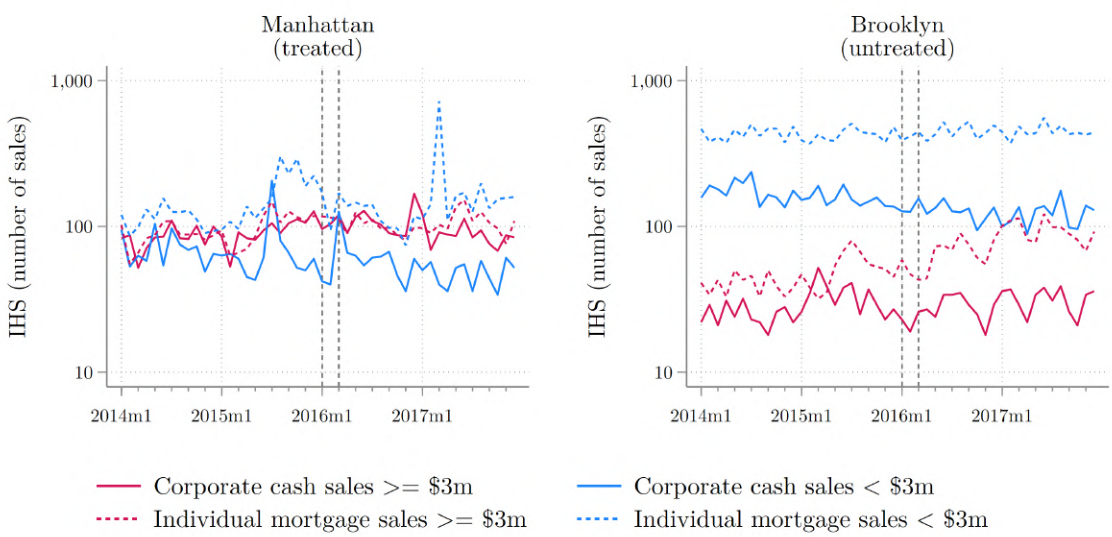
Notes: Each subfigure shows the density of corporate-cash purchases around the GTO reporting threshold for every \$25k price bin, for all for all counties affected by that GTO threshold both for (in red) the entire period following the introduction of the GTO (until the end of 2019) and (in black) the entire period from 2010 until the GTO came into effect. For both of these, all transactions outside the observed window are dropped (to account for long term shifts in the overall density) The counties used in each figure are as follows: \$3m (Manhattan), \$2m (Los Angeles, San Diego, San Francisco, San Mateo, Santa Clara), \$1.5m (Brooklyn, Queens, Bronx, Staten Island), \$1m (Miami-Dade, Broward, Palm Beach), \$300k (All GTO counties). For the \$300k threshold, we have dropped the period where the 'secret' GTO was in effect (May 21,2018 until November 17, 2018). For Manhattan and Miami, the GTO threshold was defined as all purchases "in excess" of the threshold. For these we assign all purchases at the threshold to the bin immediately below.

Figure 11: The number of property purchases in Miami-Dade and Manhattan, relative to the introduction of the first Geographic Targeting Order, compared to two adjacent counties

(a) Florida



(b) New York City



Notes: Figure shows the number of property purchases in the two counties that were subject to a GTO in March, 2016 (lines show the months of announcement and implementation) relative to two adjacent counties that were subject to a GTO later, in August 2016.

To investigate this further, we use a synthetic control approach to estimating the impact of the first GTO on Miami-Dade and Manhattan separately: using the total number of all corporate-cash purchases above $1 million in the former case and all corporate cash above $3 million in the latter case. The relative uniqueness of these two counties in the luxury property market creates a challenge for the standard synthetic control approach: because both counties are at the top of the national distribution of corporate cash purchases, there is no combination of non-GTO 'control' counties will create a well-match synthetic Miami or Manhattan.

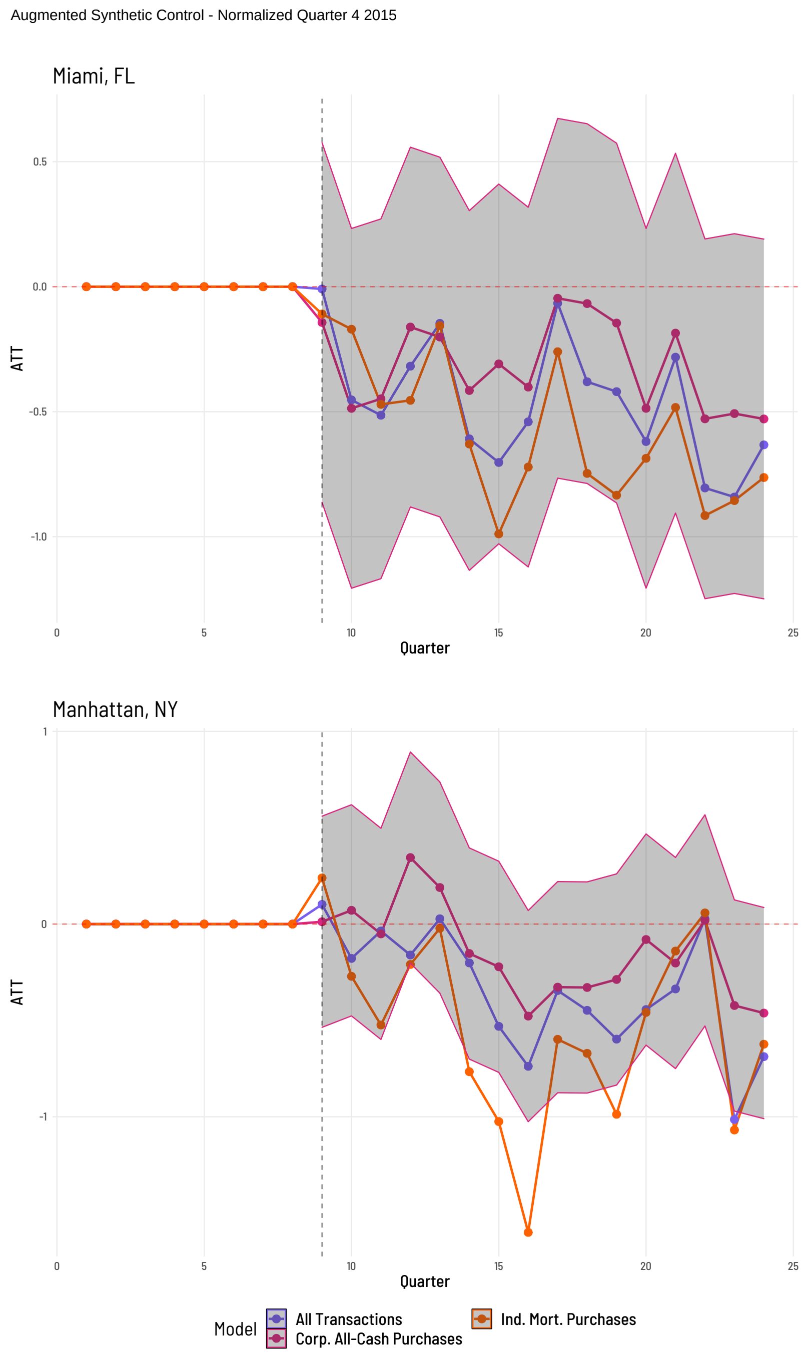
To overcome this, we do two things. First, we define the outcome variable as being the number of corporate cash purchases relative to the last pre-treatment period (December 2015). For example, in the Miami-Dade estimation, if a county has 50 corporate cash purchases above $1m in December 2015 and 100 in January, 2016, the outcome is 2. We thus drop all counties that have zero purchases above the threshold in December 2015. To minimize the number of control counties lost due to this method, we calculate all of our outcomes at the quarterly level. Second, to account for an imperfect fit, we use the bias-correction methods of the augmented synthetic control approach detailed in Ben-Michael, Feller, and Rothstein (2021) (more detail on this approach is provided in Appendix Section C). Finally, for each estimation we only keep the treatment county (Manhattan or Miami-Dade, respectively) and all non-GTO counties, so as to not use control counties that subsequently become treated.

The main results are shown in Figure 12. While we observe declines in corporate all-cash purchases in both counties following the announcement of the first GTO, the decline only happens immediately in Miami-Dade. Even though there is a noticeable decline in purchases above $1m relative to synthetic Miami, we observe similar declines in individual mortgages purchases, and in all purchases, respectively. We see a similar long term pattern in Manhattan. Our conclusion from these results is that the negative effect observed for this group (Miami-Dade and Manhattan) in our main analysis is likely driven by idiosyncratic shocks that hit these property markets around the same time as the GTO came into effect, rather than an impact of the GTOs on the types of transactions that were being targeted.

4.5 Did GTOs lead to a general chilling effect on illicit transactions?

Absent strong evidence that the GTOs led to a stark change in opaque transactions in the real estate market, we consider the possibility that GTOs led to a general decline in illicit transactions in affected counties. This might be the case because there was a small decline in illicit property transactions that our previous analysis was unable to identify or because the GTOs led those wanting to launder money to avoid targeting counties for fear of additional scrutiny by law enforcement.

Figure 12: The impact of the first Geographic Targeting Order on high value purchases in Miami-Dade and Manhatan (Augmented Synthetic Control results)



Notes: Figure shows results of augmented synthetic control estimation (Ben-Michael, Feller, and Rothstein, 2021) of the impact of the first Geogaphic Targeting Order on (top figure) the relative number of all purchases above \$1m, corporate-cash purchases above \$1m, and individual mortgages purchases above \$1m and (bottom figure) the same for Manhattan. 95% confidence intervals are shown only for corporate all-cash purchases.

To study this, we turn to data on suspicious activity reports (SARs), the reports filed by banks when they detect a transaction or activity by one of their customers that they deem is worth reporting to FinCEN. There are no broad criteria as to what constitutes suspicious activity: banks are left to their own devices to determine what might qualify. For example, a customer who appears to be deliberately avoid automatic cash deposit reporting requirements by making multiple deposits below the threshold might lead a bank employee to file a SAR.

On their own, SARs are not necessarily a good proxy or underlying levels of money laundering. This is because the incentives that banks face to file SARS have changed over time. Because filing a SAR allows banks to essentially 'pass the buck' on to law enforcement to follow up on suspicious activity, there are significant incentives to over-file, what is often referred to as "defensive filing" (Unger and Van Waarden, 2009). Thus an increase in SARs from one year to the next could reflect a change in underlying criminal activity or just a change in the pressure financial institutions face from regulators to report. However, because the GTO program did not affect banks directly (by targeting only title companies and dealing only with property transactions), we would not have expected banks to change their behavior in response to it. If the GTO program led to a decrease in illicit money moving in and around targeted counties, we would expect the number of SARs filed by banks transacting with those counties to decline.[25]

We use data taken directly from FinCEN's public portal, which detailed the number of monthly SARs filed by each US county for the period 2014-2021. According to FinCEN, banks indicate in a SAR *where* the suspicious activity took place.[26] So while SARs should reflect changes in the flow of illicit money moving in and out of a given county, it would not comprehensively capture all flows connected to a given county. For example, if a buyer in Horry County, South Carolina purchases a property in Miami-Dade from a seller who owns a bank account in Shawnee County, Kansas, the location of the reported suspicious activity may be coded as Horry or Shawnee, rather than Miami-Dade. Thus SARs will be a partial geographic measure of suspicious activity, but not a perfect one.

We then estimate our main model, in which a county is considered treated when a GTO is first announced. We also estimate an alternative model in which we use the 'secret' GTO date in place of the third GTO. Finally, we estimate a third model in which counties covered by the

[25]However, at the same time, if the GTO program led buyers trying to launder illicit cash to rely more on the existing banking system, then the introduction of the GTO program could lead to an increase in SARs).

[26]In instances where this field was empty, the location of the SAR may revert to the location of the relevant bank branch or corporate office.

first two GTOs are not considered treated until the GTOs were amended to cover wire transfers. This is to investigate whether this potential loophole, once closed down, led to a decline in wire-transfers related to illicit property purchases and thus a decline in the number of transfers being flagged through suspicious activity reports.

Table 2 and Figure 13 display the main results from this exercise. Across none of the three specifications is there a significant impact of the introduction of the GTOs (or their revision) on the number of SARS being filed at the county level. The largest estimated effects are for the revision of the GTOs to include wire transfers, which led to an estimated increase in SARs of around 4%. However, this effect is not significant at standard levels of inference. As can be seen in Table 2, a few of the group-specific estimates, most notably for the introduction of the secret GTO - show significant effects of between 4-12%. However, examination of the event study coefficients for these groups reveals that these are long term effects which only manifest over a year after the introduction of the GTOs, indicating that these are unlikely to reflect a short term response to the program.

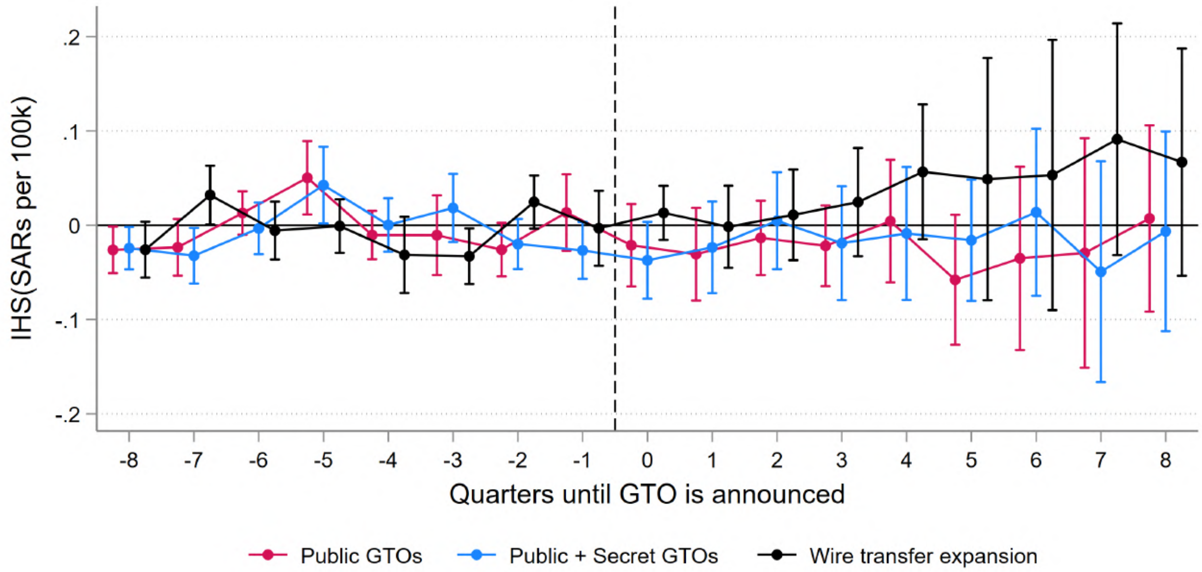
Taken together, the introduction of the GTO program does not appear to have had a perceptible impact on illicit activity in the targeted counties, as measured by suspicious activity reports.

4.6 Aggregate property market effects of the program

One often-cited concern over the use of local property by foreigners to launder money is that this behavior has the potential to drive up local property prices (De Simone, 2015). While there is evidence that other drivers of foreign investment in real estate (such as economic uncertainty or restrictions on foreign ownership in other markets) have an impact on local house prices (Badarinza and Ramadorai, 2018; Gorback and Keys, 2020) there has been little evidence that the flow of dirty money has the same impact.

In theory, if the GTOs had led to a significant decline in investment in the markets being targeted, we would expect this to manifest in a decline in property prices. To test this, we repeat our main analysis using Zillow's Home Value Index (ZHVI) as an outcome. Zillow's ZHVI is described as a seasonally-adjusted dollar value of the 'typical' home in a market. The index derived from averages of Zillow's Zestimate, the main estimate of a property's value from Zillow's valuation model. Zillow publishes monthly data on the ZHVI separately for properties in the 'top tier' (those priced at and above the 66th percentile for a given market), the 'middle

Figure 13: Event study estimates of the impact of GTOs on Suspicious Activity Reports (SARs) filed by banks



Notes: estimates are (dynamic ATT) estimates of the impact of public GTO announcements on the inverse hyperbolic sign of the number of suspicious activity reports (SAR)s filed per 100,000 people at the county level to FinCEN between 2014 and the end of 2019 (only shown for the window of two years leading to or following the announcement of a GTO). Event time is in months relative to the announcement date All estimates calculated using Callaway & Sant'Ana's (2021b) doubly robust estimation method with the median 2015 price and GDP-per capita included as controls (results correspond to columns (2), (4) and (6) of Table 2. 95% confidence intervals shown.

tier' (those priced between the 33rd and 66th percentile) and the 'bottom' tier, which is everything below the 33rd percentile. Across most of our markets, we would expect the first two and third GTOs to negatively affect primarily the top and mid-tiers respectively, but not the bottom tier of the market.

Figure 14 shows the event study estimates when we use the same model in our main analysis, but use the three tiers of the ZHVI as our outcome. While there is a short term decline in the ZHVI following the announcement of a new GTO in targeted markets, the negative point estimates are small (less than 1%), insignificant and transitory, lasting fewer than a few months. Furthermore, these small, insignificant declines are observed across all three tiers, indicating that the shift in the ZHVI observed at this time is not likely to be driven by the GTOs and themselves. The overall group-ATT for the three tiers is estimated at 0.014, 0.0185, and 0.0173) respectively, indicating that the long term impacts of the GTOs on house prices are positive and insignificant in the long run.

While this is yet another clue that the GTOs are not likely to have led to a sizable shift in

Table 2: Impact of GTO announcement on SARS

	Public GTOs		Public + Secret GTOs		GTOs extended to wire transfers	
	(1)	(2)	(3)	(4)	(5)	(6)
ATT across all groups	-0.00808	-0.00286	0.00841	-0.00118	0.0381	0.0456
	(0.0310)	(0.0338)	(0.0314)	(0.0344)	(0.0262)	(0.0344)
Q1 2016 (1st GTO)	0.00508	0.0273	0.00498	0.0155		
	(0.0249)	(0.0374)	(0.0250)	(0.0363)		
Q2 2016 (2nd GTO)	-0.0168	-0.0161	-0.0170	-0.0190		
	(0.0392)	(0.0440)	(0.0392)	(0.0449)		
Q3 2017 (1st wire transfer GTO)					0.0398	0.0467
					(0.0275)	(0.0384)
Q2 2018 (secret GTO)			0.123***	0.0618*		
			(0.0307)	(0.0339)		
Q4 2018 (3rd GTO)	0.0290	0.0402*			0.0290	0.0402*
	(0.0190)	(0.0220)			(0.0196)	(0.0232)
Controls?		X		X		X
# Counties	294	294	294	294	294	294
# Quarters	24	24	24	24	24	24
Observations	7056	7056	7056	7056	7056	7056

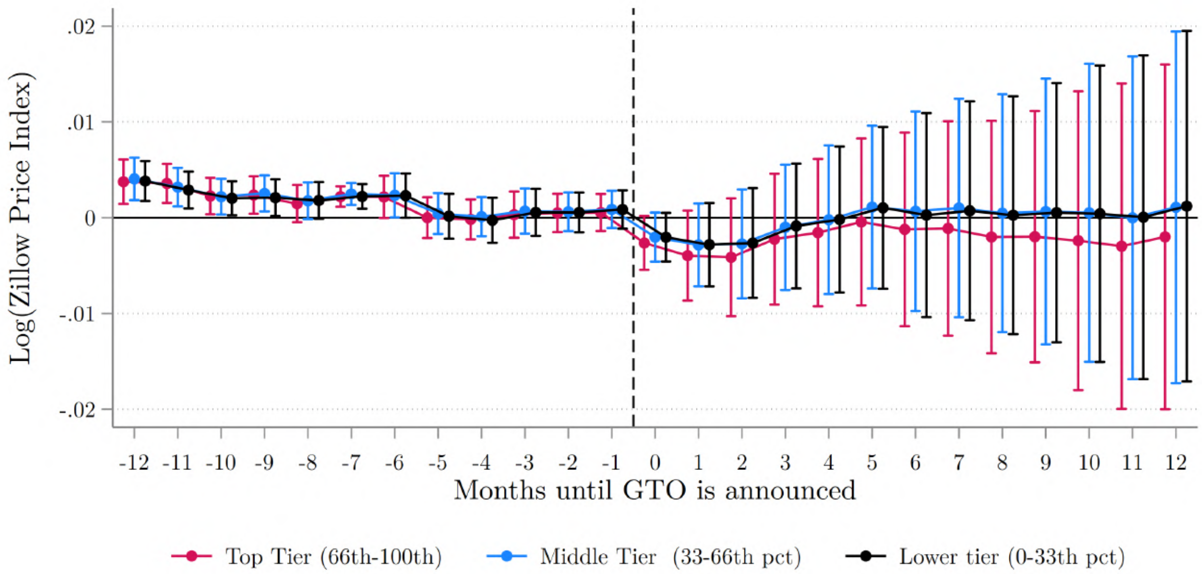
Notes: Outcome is the inverse hyperbolic sign of the number of SARS filed at the county level in a given quarter. **Public GTOs** uses the three main public GTOs as the relevant treatment dates. **Public + Secret** uses the date of the announcement of the secret GTO as the relavant treatment date for the third treatent group. **GTOs extended to wire transfers** uses August 2017 as the relevant treatment date for the first two groups (as this is the moment when the GTOs were expanded to cover wire transfers in these counties) and uses November 2018 as the relevant date for the final group (as this is when they were first subject to a GTO) Event-time is in quarters.
$^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$ Standard errors clustered at the GTO group-county level.

investment in investment in these markets, it is also an indication that these policies have not led to any substantive market impacts. Given that the reporting requirements of the GTOs may ultimately raise transaction costs in these markets (via the extra costs borne by title companies), it is reassuring that these costs do not appear to have manifested in changes in property values. To the extent that the GTO program still has value as an information-gathering exercise for law enforcement, the costs associated with it appear to be minimal.

5 Discussion

Overall the analysis has revealed that the GTOs had a small and statistically insignificant effect on corporate all-cash purchases in the targeted counties. This null effect does not appear to vary based on when, in which counties, or at what sales price threshold the orders were implemented. We also do not see substitution into other types of purchases not covered by the GTOs, for example, legal entities taking out loans from "bad" or foreign banks or trusts being used instead of other types of legal entities. Similarly, the GTOs did not drive down all-cash purchases by some of the corporations most associated with this type of money laundering in the United States: LLCs registered in so-called secrecy jurisdictions or by corporate service providers. Fi-

Figure 14: Event study estimates of the impact of public GTO announcements on Zillow's Home Value Index (ZHVI) at the county level



Notes: estimates are (dynamic ATT) estimates of the impact of public GTO announcements on the log of the Zillow Home Value Index from between the beginning of 2014 to the end of 2019 (only shown for the window of two years leading to or following the announcement of a GTO). Event time is in months relative to the announcement date All estimates calculated using Callaway & Sant'Ana's (2021b) doubly robust estimation method with the median 2015 price and GDP-per capita included as controls 95% confidence intervals shown.

nally, whereas other regulatory interventions (such as mansion taxes) generated bunching just under declared thresholds, we do not observe similar patterns with regards to the thresholds used by FinCEN. Taken together, we find little evidence that the GTOs reduced the amount of money being invested into the US real estate market through the corporate all-cash route.

5.1 Lack of illicit transactions or incomplete implementation?

We see two possible explanations for why the reform did not produce the intended, observable results. On one hand, the GTO laws could have been properly implemented and enforced, but the scale of money laundering into the luxury real estate market in the US might be substantially smaller than expected. By definition, money laundering is a very difficult problem to identify and measure. What we do know about the practice in the United States comes from anecdotal journalistic investigations and criminal prosecutions rather than comprehensive forensic analysis. It is possible that only a small fraction of corporate all-cash purchases actually involve suspicious, wealthy individuals who would not want to reveal their identity to federal authorities. The GTOs may have deterred such buyers from entering the targeted countries, but their number was not so large that their exit from the market would affect aggregate statistics about corporate all-cash purchases. We term this the *small scale* explanation.

On the other hand, we might hypothesize that the volume of money being laundered through the US luxury real estate market is substantial, but GTOs did not effectively deter bad actors from continuing to exploit the sector. We term this the *incomplete implementation* explanation, in that the implementation of the GTOs by FinCEN may have been insufficient to change corporate buyer behavior. Incomplete implementation can arise along three dimensions: compliance, verification, and enforcement.[27] First, corporate all-cash buyers may have refused to faithfully comply with the regulation, either by submitting false (or even failing to submit) beneficial ownership information as required. Such non-compliance deprived officials of actionable intelligence to pursue money launderers and undermined the efficacy of the GTOs.

Next, even if corporate all-cash buyers complied and submitted reports on their beneficial owners, law enforcement authorities may have failed to verify the information. Beneficial ownership reporting is a relatively recent form of transparency requirement. As countries worldwide begin to implement similar directives, journalists and academics have consistently uncovered failures by governments such as the United Kingdom and Luxembourg in ensuring the quality and accuracy of data submitted to officials (GW, 2013; Martini and Szakonyi, 2021). Corporate buyers may have realized that there were no mechanisms or penalties for submitting false information and continued their money laundering activity as before. Finally, law enforcement officials must both be able to identify and then hold money launderers accountable through criminal prosecution. Acquiring accurate information on beneficial owners aids in the former, but does not necessarily ensure that investigations will be conducted, arrests made, and suspicious assets seized.

Although testing between the two explanations is complicated by confidentiality surrounding official FinCEN data,[28] the preponderance of publicly available evidence suggests the lack of measurable effect of the GTOs is best explained by incomplete implementation. First, compliance with the GTO orders may be incomplete. Official figures given by FinCEN (as of August 15, 2019) cite 23,659 beneficial ownership reports submitted by title companies pursuant to the GTOs (GAO, 2020). Using the Ztrax dataset, we calculate that there were no fewer than 27,114 corporate

[27] Section 4.3 demonstrated that bad actors were not exploiting a fourth dimension, loopholes in the regulation, for example, by using trusts or taking out mortgages from shoddy lenders in order to avoid the transparency requirements of the GTOs.

[28] In March 2020, the authors submitted to FinCEN detailed questions and requests for data concerning the implementation of the GTO program, but as the time of the writing, no response has been received.

all-cash transactions occurring in GTO-covered counties during the same period, i.e. those that had sales prices above the price threshold in place at the time of the transaction. Because Texas is a non-disclosure state, ZTrax is missing data on covered transactions in Bexar, Dallas and Tarrant Counties. Therefore, we estimate that at best, GTO reports were submitted in no more than 87% of applicable cases; taking into consideration the missingness in Texas, the true number is likely to be significantly lower.

In 2020, the Government Accountability Office (GAO) likewise identified significant shortcomings in FinCEN's oversight of title companies, and in particular the lack of examination of the dozens of firms submitting GTO reports (GAO, 2020). FinCEN did evaluate compliance by a single title company, but only three years after the first directive was implemented in 2016. The other 28 companies did not undergo such scrutiny nor were the results of that evaluation publicly related. As written, the GTOs do not provide a clear mechanism for verifying that corporations are accurately reporting their beneficial owners. In conversations with this paper's authors, FinCEN officials did not elaborate on how the GTO beneficial ownership data was handled or analyzed, though perhaps because they were hesitant to share sensitive information about the tactics used. Worryingly, the GTO orders are also vague about the civil and criminal liability that these title companies face for not fully complying (McPherson, 2017).

Perhaps the biggest weakness in implementation relates to enforcement. FinCEN officials reported to the GAO in 2020 that "the GTO reports, in some cases, helped identify potential suspects of interest for further investigation and led to law enforcement referrals" (GAO, 2020, 13). Indeed, six enforcement agencies and two interagency task forces shared that GTO reports had been used in their investigations. Yet as of 2020, government officials were not aware of any cases where real assets were seized or forfeited based on the information contained in GTO reports. In fact, it took more than two years after the first GTO was issued in 2016 for FinCEN to systematically contact different law enforcement officials to share information (GAO, 2020). Independently we were also unable to find any criminal cases involving real properties where GTO reports were specifically cited in court documents or media reports. Without publicized asset seizures and forfeitures, it remains unclear whether bad actors believed the GTO program carried real punishments and thus would have changed their purchasing behavior.

The limited data shared by FinCEN about the GTO program also suggests that the scale of money laundering through US real estate has been and continues to be substantial, thereby helping excluding the "small scale" explanation. Of the 23,659 GTO reports submitted from 2016-

2019, 8,652 (37%) involved an individual who had been mentioned previously in a suspicious activity report (SAR), which financial institutions file with the US Department of Treatment when transactions of more than $10,000 are suspected of a connection to money laundering or other criminal activity. Although a SAR in and of itself is not proof of criminal activity, the mere fact that over a third of all GTO-covered (i.e. luxury) transactions involved such individuals *after* the program was implemented suggests that scale of suspicious activity in the sector is large. Likewise, the FBI indicated that "nearly 7 percent of the GTO reports identified individuals or entities connected to FBI's *ongoing* cases since the issuance of GTO in 2016" (italics added) (GAO, 2020).[29] Although the number of counties targeted by the GTOs was both small and selective, based on these official statistics, the volume of suspicious money flowing into the US real estate market was large enough to be detected using our empirical approach and the fine-grained ZTrax data.

6 Conclusions

Our analysis shows that across all targeted counties, the GTO orders had no aggregate effect on the number or volume of all-cash corporate transactions. Using a variety of estimation techniques, we see little evidence that would-be money launderers have been effectively deterred by the policy as to decrease their purchase of high-end properties in the United States. We do not observe a body of supporting evidence across a number of dimensions that corporate buying behavior changed due to this policy. In all likelihood, the lack of visible enforcement of the GTO orders, as signalled by criminal investigations and seizures of properties linked to criminal actor, best explains the limited effectiveness of the policy.

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[29] More recent reports by non-governmental investigators paint a similar picture of ongoing money laundering in the sector. For example, Global Financial Integrity found 56 known criminal cases involving money laundering in US real estate publicly reported from 2015-2021 that collectively were valued at least $2.3 billion (GFI, 2021). Over 80% of these cases used a corporate structure to hide the true owner, and 40% involved properties located in a GTO-covered county.

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Appendix

A Data cleaning

A.1 Data Set Creation

To create our sample, we start with all 32.4 million deed transfers from the period of 2015-2019. We code each transaction's date based on ZTrax's *DocumentDate* variable, or if missing, the *RecordingDate* variable. We next reduce this resulting transaction-level data to only include arms-length transactions. Specifically, we only include transactions where the transfer code provided by ZTrax (*dataclassstndcode*) takes values indicating Deed Transfer (D) and Deed with Concurrent Mortgage (H) (identified as values "U" and "J" for Hawaii).[30]

We use two codes provided by ZTrax to drop non-residential property transactions. ZTrax uses exact address matching to link property sales to the official county assessment record of the property. First, we code residential properties as those with the prefixed "RR" in a land use code that is included in the ZTrax data set (*AssessmentLandUseStndCode*).[31] Next, we identify additional residential properties based on the *PropertyUseStndCode* variable. Lastly, we link the historical assessment data (the ZAmst data set) to our transaction data. We then change any property transactions to residential if any *PropertyLandUseStndCode* in the property's assessment history indicates that the property is residential. In total, 2% of sales were missing both property use codes. We drop transactions that are missing all three property use codes or that can not be linked to the ZAsmt assessment data set (i.e. Ztrax's *ImportParcelId* is missing).

A.2 Issues with Hundtofte and Rantala (2018)'s coding of legal entities

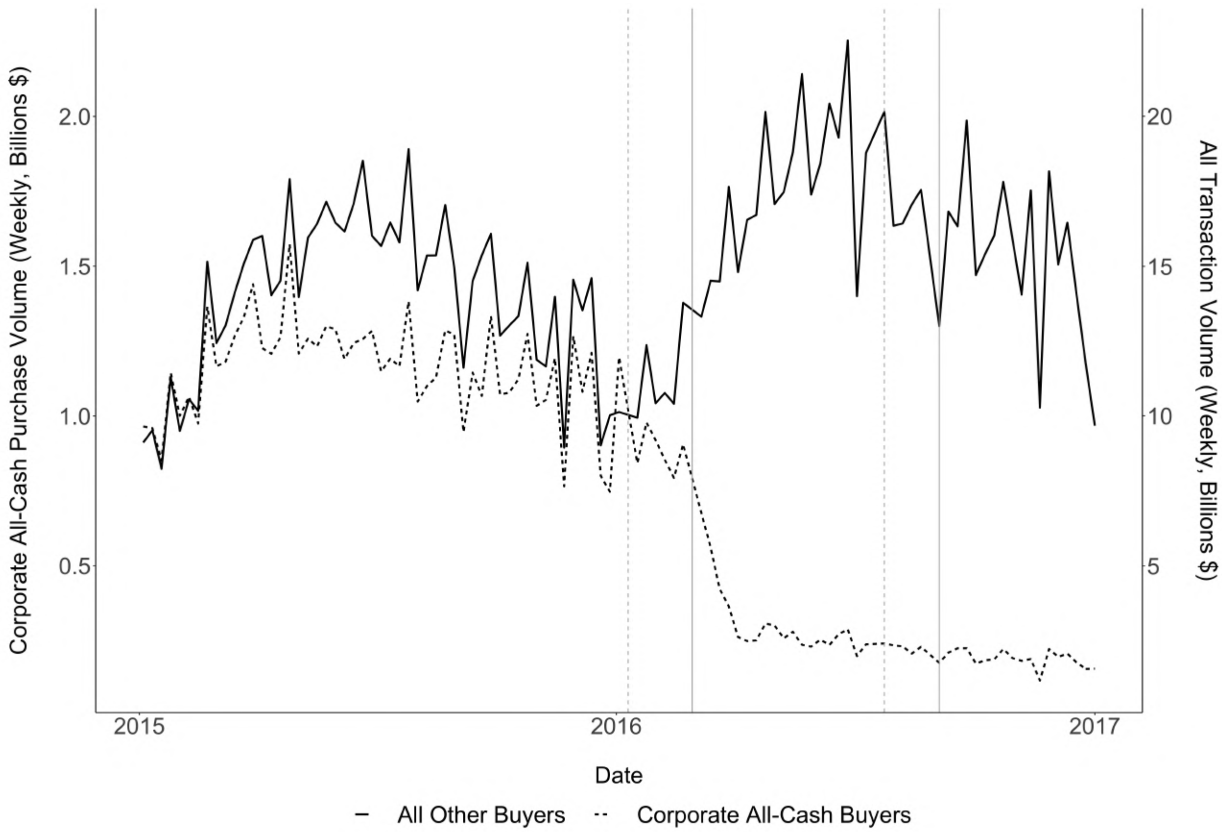
We argue that coding decisions made by Hundtofte and Rantala (2018) (hereafter H-R) mistakenly led to the exclusion of a significant portion of purchases by both corporations and trusts. In their coding scheme, H-R use a two-letter field from the raw ZTrax data called *DescriptionCode* that classifies transaction parties (buyers, sellers and borrowers) into common categories. The ZTrax documentation provides a dictionary of the 97 possible values that *DescriptionCode* can take (such as Company, Individual, Government, Trust, and so on). H-R identify corporate buy-

[30]This step removes foreclosures, second mortgages, and easements from the data set. We also remove all transfers between family members, as indicated by Zillow's *IntraFamilyTransferFlag* variable.

[31]This includes the following categories: Residential General, Single Family Residential, Rural Residence, Mobile Home, Townhouse, Cluster Home, Condominium, Cooperative, Row House, Planned Unit Development, Residential Common Area, Timeshare, Seasonal, Cabin, Vacation Residence, Bungalow, Zero Lot Line, Manufactured, Modular, Prefabricated Homes, Patio Home, Residential Parking Garage, Miscellaneous Improvement, Garden Home, Landominium, Inferred Single Family Residential.

ers when the *DescriptionCode* for the buyer is "CO" and trusts when the *DescriptionCode* is "TR". Using this coding scheme, we were able to replicate Figure 1 of H-R, as shown here in Figure A.1 here.

Figure A.1: Replication of Figure 1, Hundtofte and Rantala (2018)



Note: this Figure plots the total volume of purchases conducted by Corporate All-Cash Buyers (dotted line) and All Other Buyers in the 17 states (plus DC) included in H-R's analysis sample. The left y-axis maps onto the Corporate All-Cash Buyers, while the right y-axis maps onto All Other Buyers. The vertical gray lines indicate the announcement (dashed) and implementation (solid) of first set of GTOs implemented in the spring and summer 2016 in 14 counties. This Figure is a replication of Figure 1, Hundtofte and Rantala (2018).

We argue there are serious methodological issues with relying exclusively on these two *DescriptionCodes* to classify the purchases of interest. First, by our calculations, *DescriptionCode* is blank for roughly 7% of buyers and 24% sellers involved in 32.6 million transactions over the period of 2015-2019. Rather than try to assign *DescriptionCodes* where they are missing for buyers (for example using string matching techniques to code based on names), H-R adopt a listwise-deletion approach and remove all transactions where the *DescriptionCode* is blank.

However, the missingness in *DescriptionCode* is not random. First, almost all of the missingness relates to buyers that are not natural persons. We find that *DescriptionCode* is missing for 46% of buyers that are corporations,[32] and for 18% that are identified as trusts, while less than 1% for those that are natural persons. Excluding buyers from the analysis because their code is missing leads to severe underestimation of purchases by legal entities.

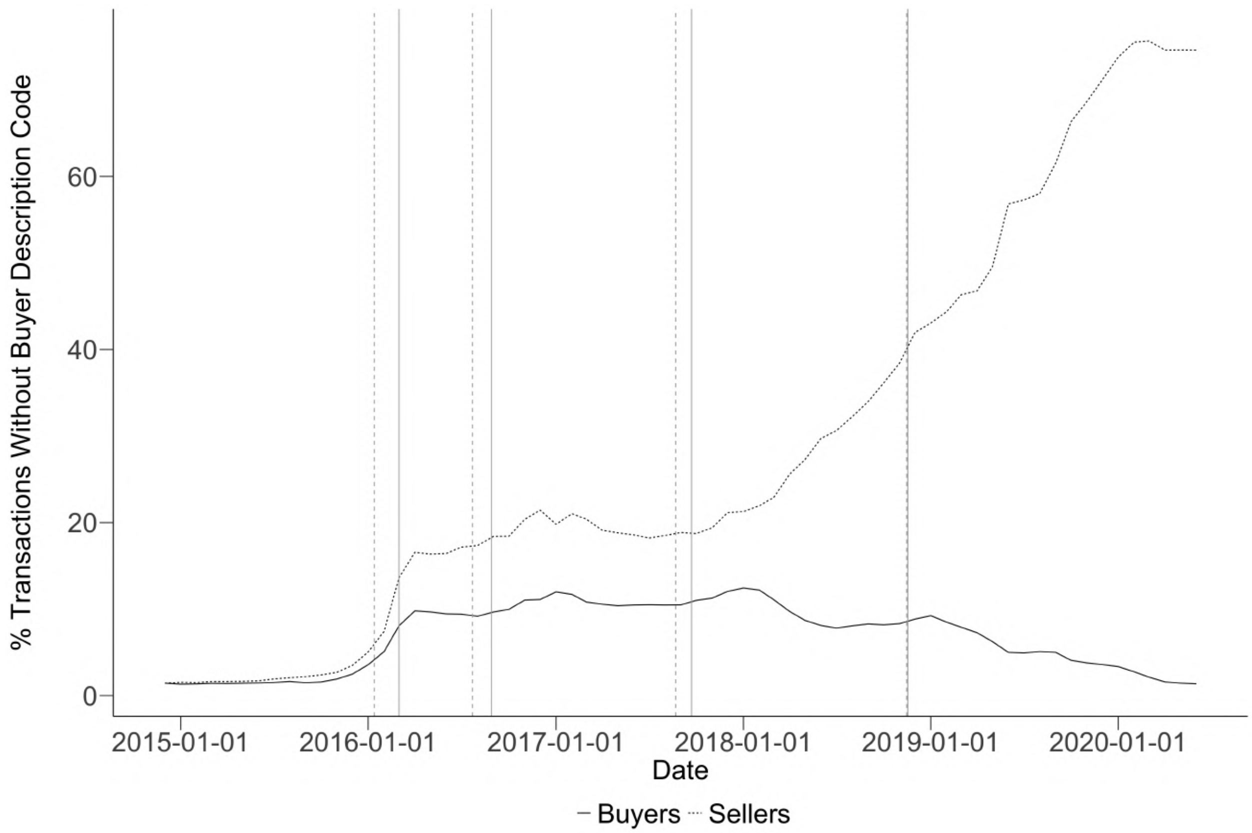
Second, the missingness in *DescriptionCode* increases significantly beginning in 2016. Figure A.2 plots the percentage of buyers and sellers, respectively, for whom *DescriptionCode* is missing. The light gray vertical lines indicate when a set of Geographic Targeting Orders is either announced (dashed) or implemented (solid). From mid-2016 to 2020, the percentage of buyers with missing *DescriptionCode* increases to and stabilizes at roughly 12%, while that for sellers increases consistently to over 70% by 2020. Because these *DescriptionCodes* are missing predominantly for corporations and trusts, removing the transactions from the analysis could create the false impression that the GTOs implemented during this period are driving down corporate transactions.

The trends in Figure A.2 could suggest that buyers were somehow strategically leaving blank their *DescriptionCode* in order to avoid having to comply with the GTOs. We argue, however, that the missingness is not a product of such evasion. First, *DescriptionCode* appears to be assigned to transaction parties during the creation of the ZTrax dataset, with no evidence suggesting it is an 'official' field filled out during the deed transfer. The ZTrax documentation does not give an explanation of how *DescriptionCodes* are assigned to transaction parties, but does suggest that string-matching on party name was used to create the 'CO' code. We were unable to get further clarification from Zillow about how the codes were created.

Second, even if parties to the transaction intentionally left these codes blank, there is no reason to believe that their absence would somehow mask their status as corporations and absolve them from the beneficial ownership transparency requirements. As Table A.3 shows below, the names of the legal entities that have missing codes clearly include indicators of corporate registration (LLC, Corporation, etc.). The GTO regulations make no reference to only applying based on how this somewhat arbitrary field was filled out. There is also substantial missingness in the *DescriptionCode* assigned to sellers, who have nothing to gain by leaving this field blank. The GTOs only required beneficial ownership transparency from the buyer in the transaction. If the missingness was indeed the result of strategic action, we should see greater missingness among buyers.

[32] See Appendix Section A.3 for the technique used to identify corporations.

Figure A.2: Missing Description Codes for Buyers and Sellers Over Time



Note: this Figure plots the percentage of transactions per month that are missing *DescriptionCode* for the involved buyers (solid line) and sellers (dashed line). The vertical gray lines indicate the announcement (dashed) and implementation (solid) of the GTOs from 2016-2018.

We also examine the correlates of this missingness empirically. Table A.1 shows results at the transaction-level where the outcome is whether the buyer (Columns 1-3) or seller (Columns 4-6) had a missing *DescriptionCode*. We might expect corporate all-cash purchases to be more likely to be missing a *DescriptionCode*. The classification of whether a transaction party was a corporation or trust uses the 'string-coding' method described in the main text, and again in more detail in Appendix Section A.3. The models confirm that transactions involving corporations and trusts are more likely to have their *DescriptionCode* missing. These point estimates are by far the largest. However, transactions involving all-cash corporate buyers (Column 3) are *less* likely to be missing their *DescriptionCode*. The price paid for the property also is not related to this missingness, suggesting that wealthier buyers are not declining to fill out this code to hide certain purchases.

Taken together, we interpret the missingness in the *DescriptionCode* field as not the product of

Table A.1: Correlates of Missingness in Description Codes

	Buyer Code Missing			Seller Code Missing		
	(1)	(2)	(3)	(4)	(5)	(6)
Buyer was Corporation	0.432***	0.433***	0.504***	-0.001	-0.0008	0.013***
	(0.011)	(0.011)	(0.013)	(0.002)	(0.001)	(0.003)
Buyer was Trust	0.137***	0.137***	0.134***	0.005***	0.005***	0.005***
	(0.006)	(0.006)	(0.006)	(0.002)	(0.002)	(0.002)
Seller was Corporation	0.009***	0.009***	0.009***	0.457***	0.457***	0.457***
	(0.001)	(0.001)	(0.001)	(0.011)	(0.011)	(0.011)
Seller was Trust	0.010***	0.010***	0.009***	0.137***	0.137***	0.137***
	(0.0007)	(0.0007)	(0.0007)	(0.007)	(0.007)	(0.007)
Commercial Property	0.008	0.008	-0.0009	0.029***	0.029***	0.028***
	(0.013)	(0.013)	(0.013)	(0.009)	(0.009)	(0.009)
Agricultural Property	-0.001	-0.001	-0.001	-0.002	-0.002	-0.002
	(0.002)	(0.002)	(0.002)	(0.004)	(0.004)	(0.004)
All-Cash Purchase	-0.001	-0.0009	0.009***	-0.016***	-0.015***	-0.013***
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)
No Title Company Used	0.006***	0.007***	0.007***	-0.012***	-0.012***	-0.012***
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
Attorney Used	0.005	0.006	0.006	-0.013***	-0.010***	-0.010***
	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)
Sales Price Present	0.004	-0.024	-0.026	0.005	-0.006	-0.006
	(0.003)	(43.1)	(43.1)	(0.005)	(57.2)	(57.1)
Year: 2016	0.078***			0.141***		
	(0.004)			(0.005)		
Year: 2017	0.098***			0.175***		
	(0.005)			(0.006)		
Year: 2018	0.099***			0.162***		
	(0.005)			(0.007)		
Year: 2019	0.106***			0.171***		
	(0.006)			(0.007)		
Year: 2020	0.104***			0.165***		
	(0.005)			(0.007)		
Sales Bracket: $0.5-1$m	-0.008*	0.020	0.024	-0.002	0.008	0.009
	(0.004)	(43.1)	(43.1)	(0.004)	(57.2)	(57.1)
Sales Bracket: $1.5-2$m	-0.001	0.026	0.028	-0.002	0.008	0.009
	(0.002)	(43.1)	(43.1)	(0.002)	(57.2)	(57.1)
Sales Bracket: $0-0.3$m	-0.011***	0.017	0.020	-0.020***	-0.009	-0.009
	(0.003)	(43.1)	(43.1)	(0.005)	(57.2)	(57.1)
Sales Bracket: $0.3-0.5$ million	-0.012***	0.016	0.020	-0.006	0.004	0.005
	(0.003)	(43.1)	(43.1)	(0.005)	(57.2)	(57.1)
Sales Bracket: Above $3 million	0.0002	0.028	0.026	0.0004	0.010	0.010
	(0.015)	(43.1)	(43.1)	(0.016)	(57.2)	(57.1)
Sales Bracket: $1-1.5$m million	-0.003	0.025	0.027	-6.923×10^{-5}	0.010	0.010
	(0.003)	(43.1)	(43.1)	(0.003)	(57.2)	(57.1)
Sales Bracket: $2-3$ million		0.028	0.029		0.010	0.010
		(43.1)	(43.1)		(57.2)	(57.1)
Buyer was Corporation × All-Cash Purchase			-0.095***			-0.018***
			(0.007)			(0.003)
R^2	0.39789	0.399967	0.402623	0.69277	0.69497	0.695002
Observations	34,698,550	34,698,550	34,698,550	34,698,550	34,698,550	34,698,550
County fixed effects	✓	✓	✓	✓	✓	✓
Data Vendor fixed effects	✓	✓	✓	✓	✓	✓
Month fixed effects		✓	✓		✓	✓

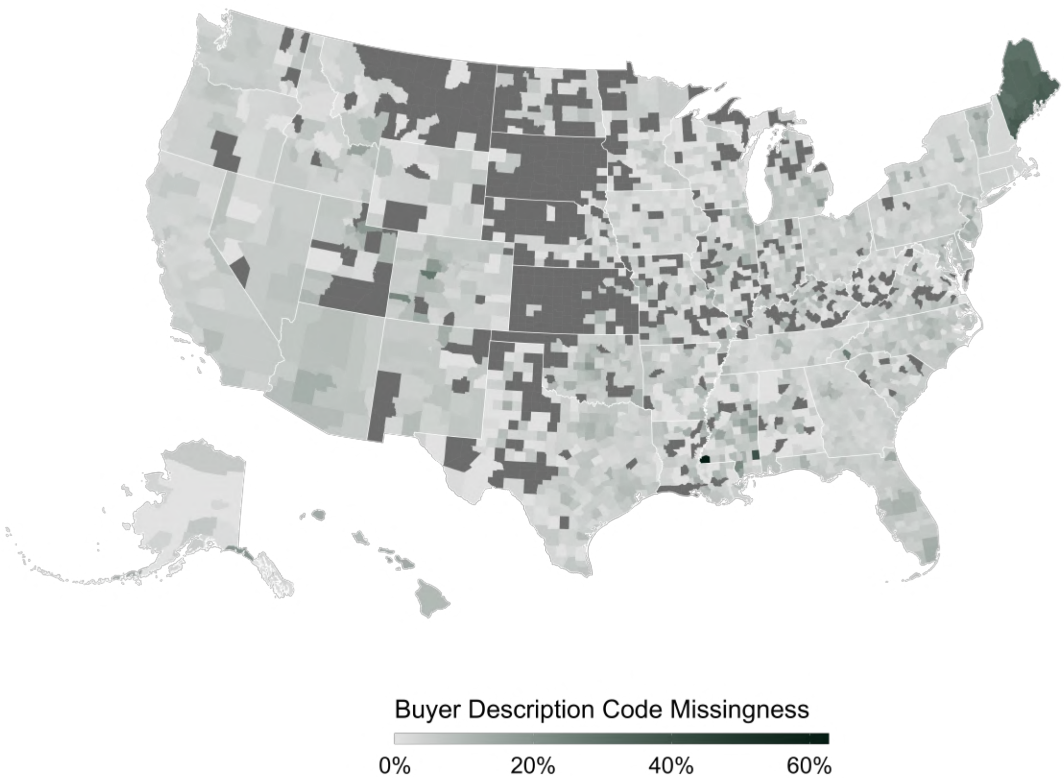
Note: This table examines the correlates of missingness in the *DescriptionCodes* for buyers (Columns 1-3) and sellers (Columns 4-6) in all transactions from 2015-2019. Corporations and trusts are coded using the string-matching procedure described in Appendix Section A.3. Standard errors are clustered at the county level.

willful evasion, but rather a mistake in data entry common to most counties across the country from 2015-2019. Figure A.3 shows this missingness in buyer *DescriptionCode* is present throughout the country during the period. However, because the missingness largely coincides with the imposition of the GTO regulations, it must be properly accounted for in order to accurately the aggregate effect of the orders. Figure A.4 extends H-R Figure 1 (replicated in Figure A.1 above) beyond year 2017 where they ended their analysis. Zooming out in this way indicates that by the end of 2019, Corporate All-Cash Purchases returned to their pre-2016 levels, nearly precisely mapping the missingness in Buyer *DescriptionCodes* for this period. Since the GTOs were in effect for this entire period, we argue this reversal in the trend is because of measurement issues rather than the effects of the GTOs. In all, the listwise-deletion approach adopted by H-R introduces significant measurement error; instead, we propose the string-based approach described in further detail in Appendix Section A.3.

Finally, separate from the missingness issue, H-R arguably miss the majority of trust buyers by using only the "TR" *DescriptionCode* to identify trusts. The ZTrax data documentation lists ten other categories that are also explicitly defined as trusts: Family Irrevocable Trust ("FI"), Family Living Trust ("FL"), Family Revocable Trust ("FR"), Family Trust ("FT"), Irrevocable Living Trust ("IL"), Irrevocable Trust ("IT"), Living Trust ("LV"), Revocable Living Trust ("RL"), Revocable Trust ("RT"), and Survivor's Trust ("SU"). The adjectives affixed to each category simply provide greater detail about the legal nature of the trust. This oversight affects the analysis of substitution effects potentially caused by the GTOs.[33]

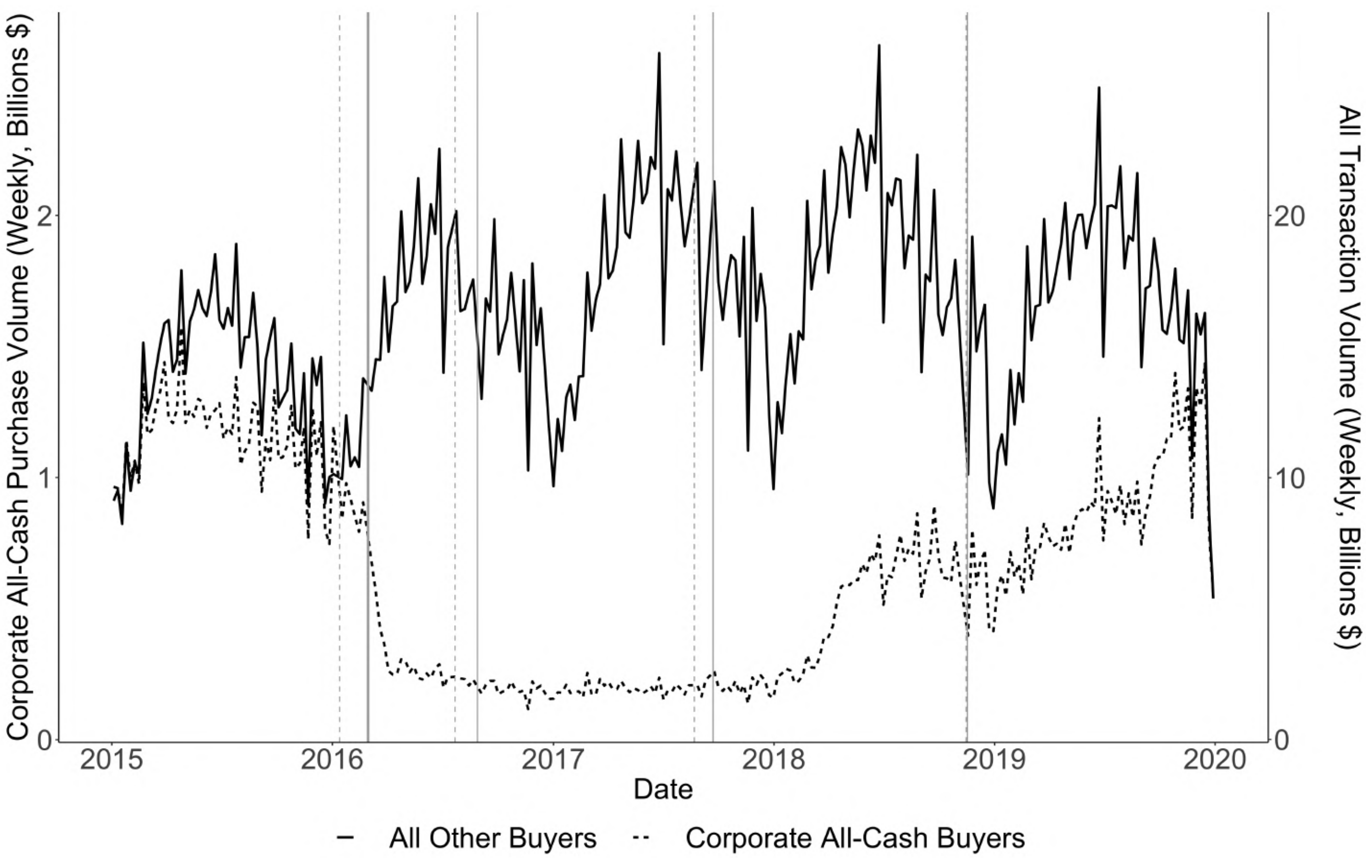
[33]This issue also affected the exclusive use of "CO" for corporations, since there are several other *DescriptionCodes* describing companies, such as Doing Business As ("DB") or Formerly Known As ("FK").

Figure A.3: Missing Buyer Description Codes Across Counties



Note: this Figure plots the percentage of buyers that have a missing *DescriptionCode* in each US county from 2015-2019. The left y-axis maps onto the Corporate All-Cash Buyers, while the right y-axis maps onto All Other Buyers. Light grey columns indicate more or less full coverage of *DescriptionCode*, with the darker shades of green indicating more missingness (dark grey counties have no transactions data in ZTrax over these years).

Figure A.4: Extension of H-R Figure 1 to Longer Time Period of 2015-2019



Note: this Figure extends H-R Figure 1 (replicated in Figure A.1) to the years 2017-2020. The left y-axis maps onto the Corporate All-Cash Buyers, while the right y-axis maps onto All Other Buyers. The vertical gray lines indicate the announcement (dashed) and implementation (solid) of the GTOs from 2016-2018.

A.3 Appendix: A String-based Approach for Coding Corporations and Trusts

Because of the missingness in *DescriptionCode* and the opacity around how the non-missing values were created, we adopt a string-based approach for classifying corporate and trust buyers.[34] We code corporations, trusts, and other types of organizations based on the name field of the legal entity used to purchase the real estate property. This method allows us to more accurately and transparently identify buyers of all types, as well as focus on the activity of corporate entities, rather than financial institutions, affected by the GTOs. For ease of explanation below, we refer the method used by H-R that relies on *DescriptionCode* as the 'H-R Code' and to our string method as 'String Code'.[35]

We begin by creating a dictionary of 'ending noise words' used by several US states to identify corporations in their Uniform Commercial Code.[36] We then supplemented this list with common noise words for other categories of interest based on the frequency they appeared in the categories of specific *DescriptionCodes* identified by Zillow. The list of noise words for the five main buyer categories of interest is shown in Table A.2.[37] We then use exact partial string matching to assign a single 'String Code' to each transaction buyer based on the name of the legal entity included in the transaction record.[38] That is, we only match strings for buyers that Zillow has identified as non-natural persons, and code all buyers where that field is is blank as individual (person) buyers.

Table A.3 illustrates how this new 'String Code' differs from the H-R approach using a sample of 20 buyers from the dataset. The column NonIndividualName gives the buyer name as it appears in the ZTrax data, and the column *DescriptionCode* shows the value that appears in the Zrax data. The column 'H-R Code' shows how H-R built their sample off the data in these columns: if the *DescriptionCode* equalled "CO" or "TR", it entered their analysis sample as either a corporation or a trust. Otherwise, it was excluded. The column 'String Code' shows the

[34] The ZTrax data documentation only partially describes how the "CO" code was assigned, noting it included, but was not limited to "Corporation", "Corp", "LLC", "Inc", and "Co". We presume this meant string-matching was used, but this could not be independently confirmed. Moreover, no list of matched terms was provided for the other 96 values in the *DescriptionCode* dictionary.

[35] H-R write in footnote 19 that searching for 'LLC' in the string of the buyer name returned similar results to using the *DescriptionCode* of "CO" to classify corporation buyers. We took this to mean they only were looking at the validity of the *DescriptionCode* indicator, and did not use other string matching procedures to address the missingness issue.

[36] Colorado (https://www.sos.state.co.us/pubs/UCC/FAQs/noiseWords.html) and Delaware (https://corpfiles.delaware.gov/uccnoisewords.pdf) post their guides online.

[37] Note we break out financial institutions from corporations since banks are not covered by the GTOs.

[38] In the roughly 0.8% of instances where multiple 'String Codes' could be assigned to single buyer, we used the following rough ordering: CO>TR>GV>BK>RG.

Table A.2: **Noise Words Dictionary**

String Code	Description	Noise Words
GV	Government	MORTGAGE CORPORATION; NATIONAL MORTGAGE ASSOCIATION; FHLMC; FNMA; DEPARTMENT; COUNTY; STATE OF; FINANCE AGENCY; FANNIE MAE; COMMISSION; THE SECRETARY OF; DISTRICT COUNCIL; NEIGHBORHOOD HOUSING; VETERANS; HUD; THE UNITED STATES OF AMERICA; VILLAGE OF; AUTHOR; DISTRICT; TOWN OF; CITY; SECRETARY; UNIVERSITY; SCHOOL
CO	Corporation	ASSN; ASSOC; BUSINESS TRUST; CHARTERED; CHTD; CO-OP; COMPANY; COOPERATIVE; CORP; CORPORATION; CU; FCU; MORTGAGE; FUND; ENTERPRISES; INVESTORS; CAPITAL; CONSTRUCTION; MANAGEMENT; APARTMENT; CONDOMINIUM; RESIDENTIAL; FINANCIAL; BUILDERS; VENTURES; ACQUISITION; EQUITY; COMMERCIAL; COMPANIES; PORTFOLIO; GRP; GENERAL PARTNERSHIP; GMBH; GP; INC; INCORPORATED; JOINT STOCK COMPANY; JOINT VENTURE; JSC; JV; LIABILITY COMPANY; LIMITED; LIMITED COMPANY; LIMITED LIABILITY COMPANY; LIMITED LIABILITY LIMITED; LIMITED LIABILITY PARTNERSHIP; LIMITED PARTNERSHIP; LLC; ASSOCIATION; LLLP; LLP; LP; LTD; LTD CO; PARTNERSHIP; PC; PLC; PLLC; RLLP; SSB; HOLDING; L L C; INVESTMENT; PROPERTIES; PROPERTY; SAVING; PARTNERS; LOAN; HOMES; DEVELOPMENT; GROUP; REAL; REALTY; RELOCATION; TITLE; LC; COMP; ASSET; DEVELOPME; FOUNDATION; L P; INSTITUTE; CENTER; INVESTOR; BUSINESS
BK	Bank	CREDIT UNION; FEDERAL CREDIT UNION; BANK; FEDERAL SAVINGS BANK; SAVINGS ASSOCIATION; FSB
TR	Trust	TRUST; TRSTEES; FAMILY TRU; TRSTEE; LIV; TRS; REM; TR TITL HOLDRS; FAMILY TRUS; LIVING; REVOC; IRREVOC; JTRS; REVOCABLE; RRERF; REV; QUALIFIED; ESTATE; PERSONAL; SEPARATE; JOINT; FAMILY; IRA
RG	Religious	CHURCH; CATHOLIC; PARISH; CHABAD; SYNAGOGUE; MISSION; MOSQUE

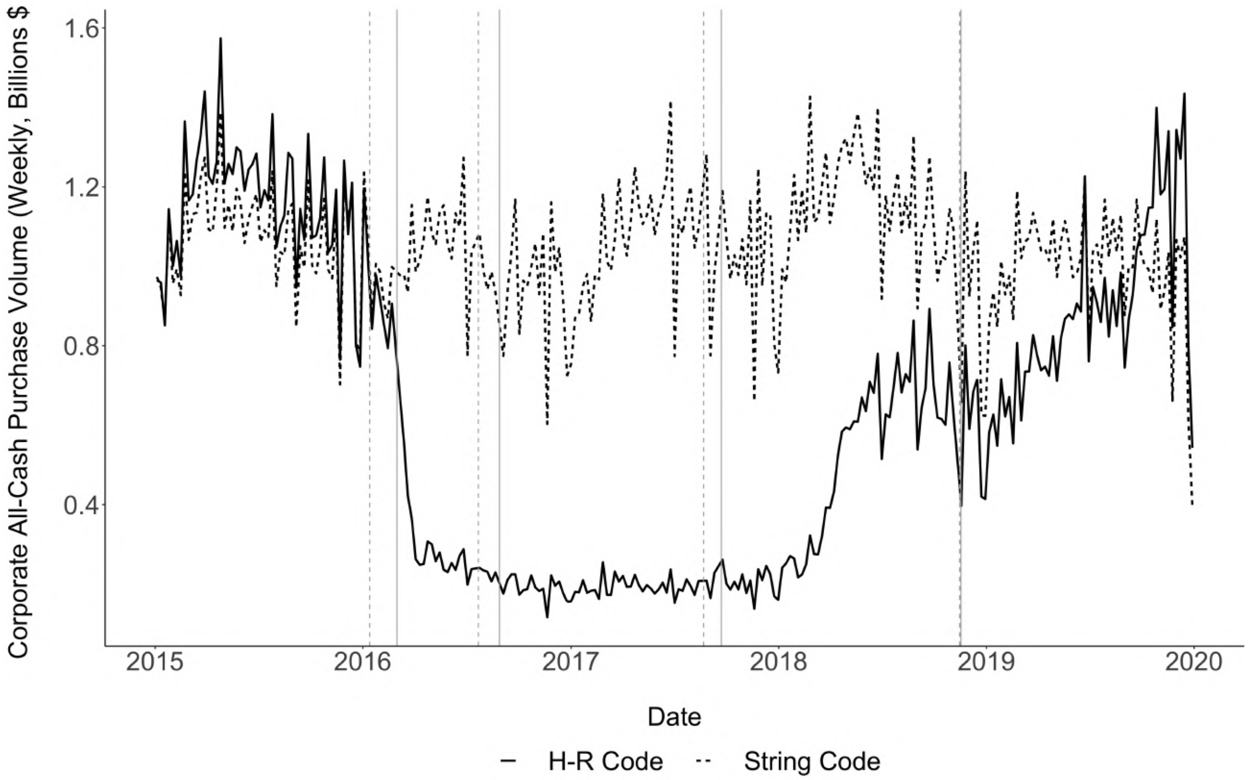
results of applying our new string-based matching for classifying buyers as corporations ("CO") or trusts ("TR") based on the keywords. We see that this procedure correctly picks up both corporate and trust buyers that were originally missing Description Codes and those that might have been classified using related codes but were missed in H-R.

From 2015-2019, the 'String Code' approach calculates that of 32.6 million transactions in the ZTrax datasets, 13.9% had corporate buyers (roughly 4.5 million transactinos). That is nearly double the amount captured by the 'H-R Code' approach, which saw 8% of transactions having corporate buyers, or 2.6 million transactions. Critically, the corporate transactions uncovered by the 'String Code' approach return a much different, smoother over-time pattern of sales volume. Figure A.5 directly compares the volume of corporate all-cash purchases using the two approaches, illustrating again how the 'String Code' approach delivers a more accurate accounting of the full spectrum of buyers during this period.

Table A.3: Difference in Coding Approaches

Entity Name	Ztrax Description Code	H-R Code	String Code
860 COURTLAND AVE LLC	CO	CO	CO
A CHRISTIAN CONSTRUCTION COMPANY #1 LLC	RG	**missing**	CO
BETTER CALL HOMES LLC	**missing**	**missing**	CO
CARRINGTON MTG SVCS LLC	CO	CO	CO
COLDWELL BANKER MORTGAGE	DB	**missing**	CO
GAIBLE FAMILY PROPERTIES, LLC	CO	CO	CO
HOLMGREN WAY INVESTMENTS LLC	**missing**	**missing**	CO
JEANA DAWN MODDEN REVOCABLE LIVING TRUST	RL	**missing**	TR
KALARA LLC	CO	CO	CO
MARRIOTT OWNERSHIP RESORTS INC	**missing**	**missing**	CO
MASI FAMILY REV LIV TRUST	RL	**missing**	TR
MASTER ADJUSTABLE RATE MORTGAGE TRUST	CO	CO	CO
MNH SUB LLC	**missing**	**missing**	CO
ONEWEST BANK FSB	BF	**missing**	BK
REVOCABLE LIVING TRUST	LV	**missing**	TR
SUMMIT FUNDING INC	BF	**missing**	CO
THE BANK OF NEW YORK	FK	**missing**	BK
THE VAUGHN FAMILY TRUST	FT	**missing**	TR
VESUVIUS TRUST	TE	**missing**	TR
ZIEGLER CUSTOM HOMES INC	CO	CO	CO

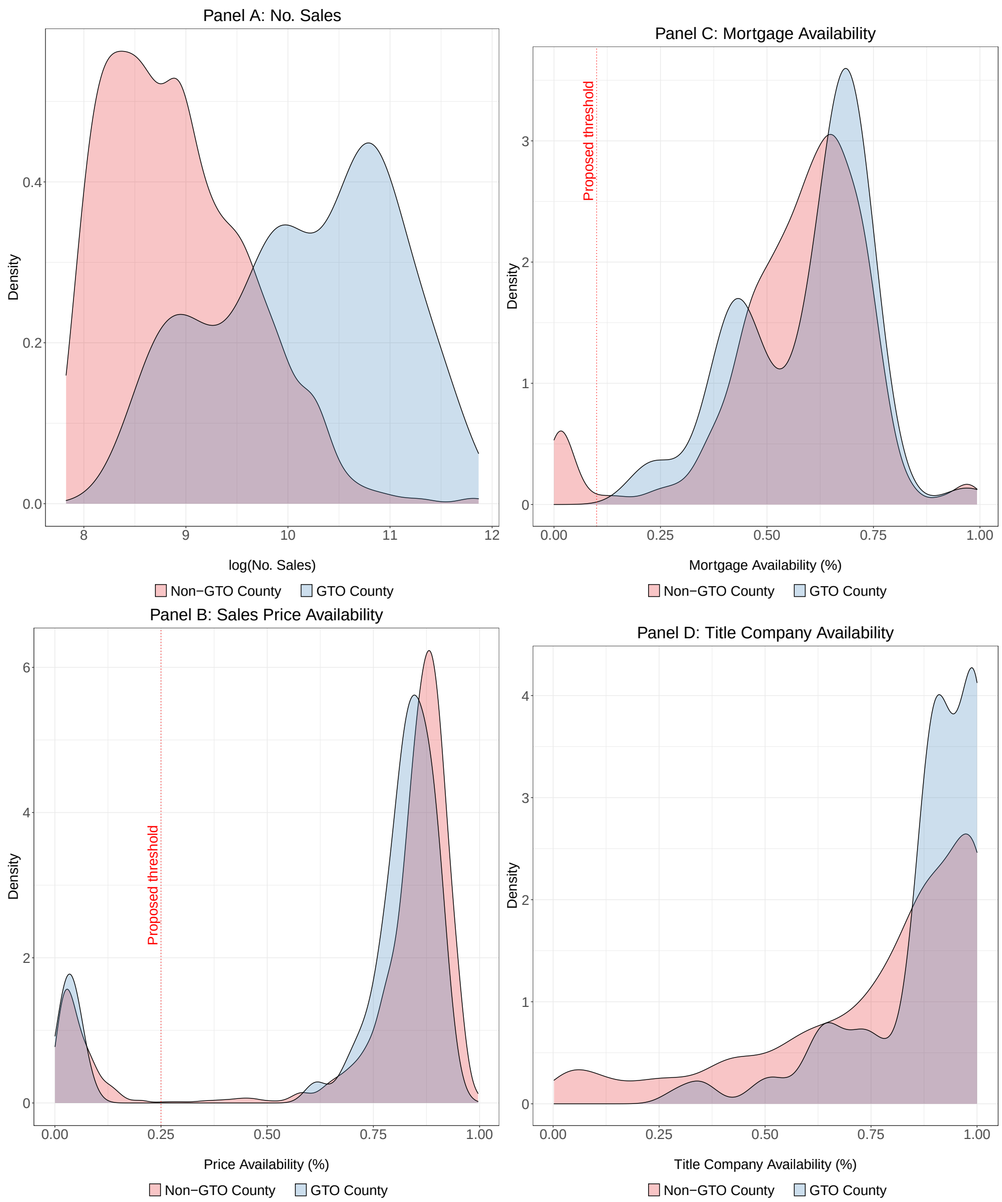
Figure A.5: Comparing the 'H-R Code' and 'String Code' Corporate All-Cash Purchases



Note: this Figure plots the volume of Corporate All-Cash purchases over time using the 'String Code' approach (solid line) and the 'H-R Code' approach (dashed line). The left y-axis maps onto the Corporate All-Cash Buyers, while the right y-axis maps onto All Other Buyers. The vertical gray lines indicate the announcement (dashed) and implementation (solid) of the GTOs from 2016-2018.

A.4 Appendix: Assessing missingness

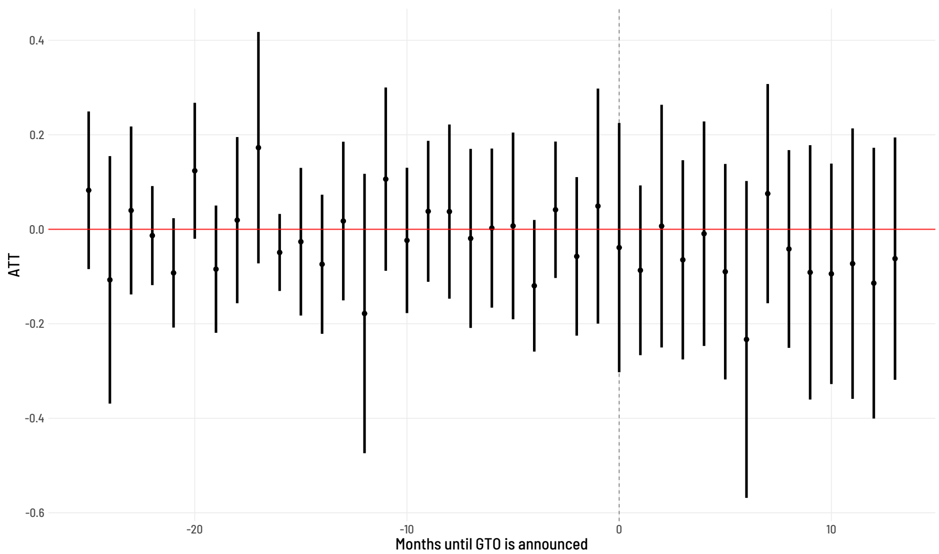
Figure A.6: Data Missingness at County-Year Level: GTO versus Non-GTO Covered Counties



Note: Figure shows the distribution of outcomes at the county-year level in order to assess data coverage in GTO-covered counties (blue) and non-GTO-covered counties (red). A county is covered by a GTO if the policy applied at any point from 2015-2019. Panel A plots a density curve of the logged number of sales, while Panels B-D plot the density of a missing indicator for whether data on mortgages, sales price, or title company usage was available at the county-year level. The red dotted lines indicate the thresholds we apply in the primary analysis to pare down the full data into a set of counties with comparably full data coverage on key outcomes.

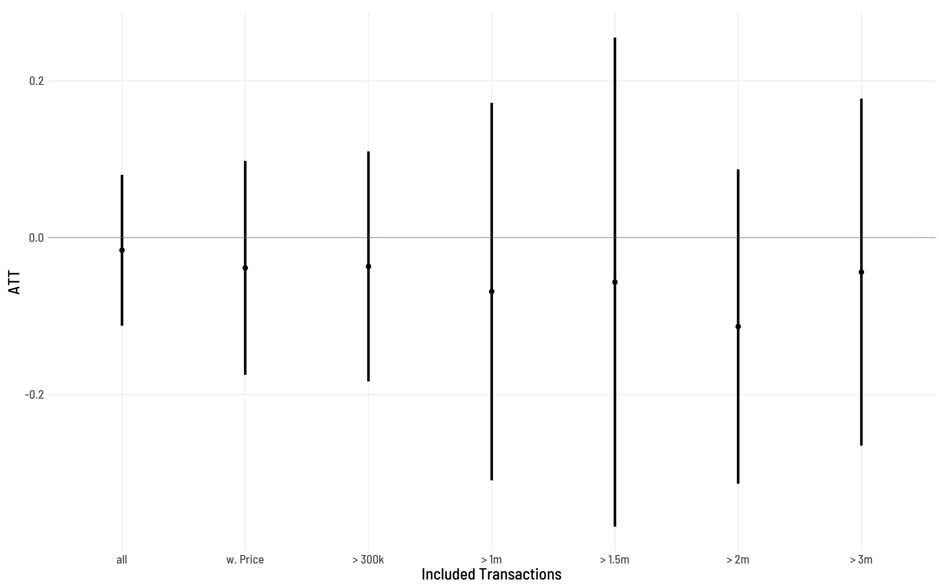
B Appendix: Robustness checks

Figure B.7: Event-study estimates – maximum pre-treatment exposure



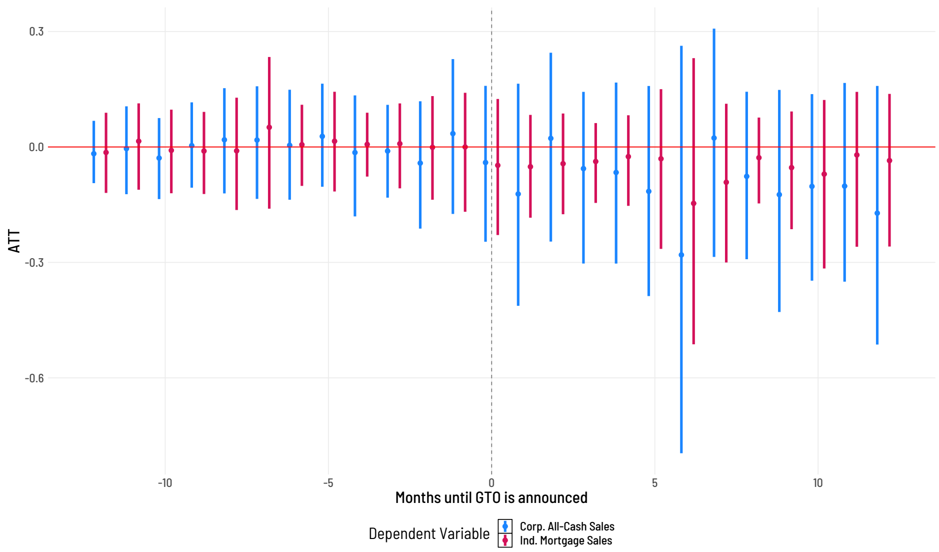
Note: Figure shows the event time estimates for the IHS transformed number of corporate all-cash purchases for the maximum pre-treatment exposure with all three GTO announcements used for estimation.

Figure B.8: County-level aggregation versus county-level aggregation keeping only high-value transactions at different thresholds.



Note: Figure shows the average group ATT and its 95% confidence interval for the non-bracket aggregation when we vary the sample by increasing a minimum sales price above which transactions are included in the aggregation, here using the GTO thresholds. Again, there is no clear evidence of a negative effect of the GTOs.

Figure B.9: Event-study estimates – Augmented Synthetic Control Method



Note: Figure shows the event time estimates for the IHS transformed number of corporate all-cash purchases and individual mortgage purchases estimated using the augmented synthetic control method for staggered treatments (Ben-Michael, Feller, and Rothstein, 2021).

Table B.4: Main Results Corporate & Individual Purchases No Price Bracket

	Corporate All-Cash		Individual Mortgages	
	No. Sales	Price Vol.	No. Sales	Price Vol.
	(1)	(2)	(3)	(4)
Average	−0.066	−0.008	0.013	0.054
	[−0.210, 0.079]	[−0.171, 0.155]	[−0.081, 0.106]	[−0.046, 0.155]
March 2016	−0.151	−0.169	−0.302	−0.196
	[−0.222, −0.080]	[−0.425, 0.087]	[−0.885, 0.282]	[−0.392, −0.001]
August 2016	−0.092	−0.014	0.112	0.147
	[−0.324, 0.140]	[−0.271, 0.243]	[−0.022, 0.247]	[0.005, 0.290]
November 2018	0.027	0.070	−0.081	−0.050
	[−0.153, 0.207]	[−0.179, 0.319]	[−0.224, 0.062]	[−0.217, 0.117]
No. Obs.	263	263	263	263

Note:
Models estimated using the did package in R. Unit of analysis is the county-month, including all transactions. Counties are coded as treated after the first GTO for any price threshold is announced. Standard errors are clustered at the county level. Control group: not-yet-treated. Sample includes 18 GTO counties. Included pre-treatment covariates are: County level GDP in 2015 (ln) and median sales price 2015 (ln). Group ATTs calculated based on 12 pre- and post-treatment months.

Table B.5: Main Results Corporate & Individual Sales – Quarterly Aggregation No Price Brackets

	No. Sales	Price Vol.	No. Sales	Price Vol.
	Corporate All-Cash		Individual Mortgages	
	(1)	(2)	(3)	(4)
Average	−0.062	−0.017	−0.030	−0.012
	[−0.151, 0.027]	[−0.123, 0.088]	[−0.089, 0.030]	[−0.078, 0.054]
March 2016	−0.126	−0.110	−0.193	−0.100
	[−0.347, 0.095]	[−0.360, 0.139]	[−0.435, 0.049]	[−0.195, −0.005]
August 2016	−0.042	0.000	0.071	0.089
	[−0.214, 0.129]	[−0.141, 0.142]	[−0.013, 0.155]	[−0.016, 0.195]
November 2018	−0.079	−0.018	−0.186	−0.200
	[−0.337, 0.180]	[−0.263, 0.227]	[−0.278, −0.094]	[−0.295, −0.105]
No. Obs.	263	263	263	263

Note:
Models estimated using the did package in R. Unit of analysis is the county-quarter, including transactions with sales price. Standard errors are clustered at the county level. Control group: not-yet-treated. Sample includes 18 GTO counties. Included pre-treatment covariates are: County level GDP in 2015 (ln) and median sales price 2015 (ln). Group ATTs calculated based on 4 pre- and post-treatment quarters

Table B.6: Corporate & Individual Purchases Secret Announcement

	Corporate All-Cash		Individual Mortgages	
	No. Sales	Price Vol.	No. Sales	Price Vol.
	(1)	(2)	(3)	(4)
Average	−0.069	−0.052	0.082	0.107
	[−0.226, 0.088]	[−0.259, 0.155]	[−0.014, 0.178]	[0.002, 0.212]
March 2016	−0.151	−0.169	−0.302	−0.196
	[−0.222, −0.080]	[−0.442, 0.103]	[−0.885, 0.282]	[−0.385, −0.008]
August 2016	−0.092	−0.014	0.112	0.147
	[−0.324, 0.140]	[−0.275, 0.247]	[−0.022, 0.247]	[0.002, 0.293]
April 2018	0.015	−0.088	0.169	0.139
	[−0.331, 0.361]	[−0.598, 0.422]	[0.006, 0.332]	[−0.071, 0.349]
No. Obs.	263	263	263	263

Note:
Models estimated using the did package in R. Unit of analysis is the county-month, including all transactions. Counties are coded as treated after the first GTO for any price threshold is announced. Standard errors are clustered at the county level. Control group: not-yet-treated. Sample includes 18 GTO counties. Included pre-treatment covariates are: County level GDP in 2015 (ln) and median sales price 2015 (ln). Group ATTs calculated based on 12 pre- and post-treatment months.

Table B.7: Corporate & Individual Purchases Never-Treated Comp.

	Corporate All-Cash		Individual Mortgages	
	No. Sales	Price Vol.	No. Sales	Price Vol.
	(1)	(2)	(3)	(4)
Average	−0.084	−0.020	0.013	0.054
	[−0.227, 0.059]	[−0.185, 0.145]	[−0.095, 0.120]	[−0.054, 0.163]
March 2016	−0.181	−0.143	−0.302	−0.196
	[−0.267, −0.094]	[−0.454, 0.169]	[−0.466, −0.137]	[−0.377, −0.016]
August 2016	−0.117	−0.038	0.112	0.147
	[−0.350, 0.116]	[−0.297, 0.220]	[−0.021, 0.246]	[0.001, 0.294]
November 2018	0.027	0.070	−0.081	−0.050
	[−0.160, 0.214]	[−0.186, 0.325]	[−0.222, 0.060]	[−0.218, 0.119]
No. Obs.	263	263	263	263

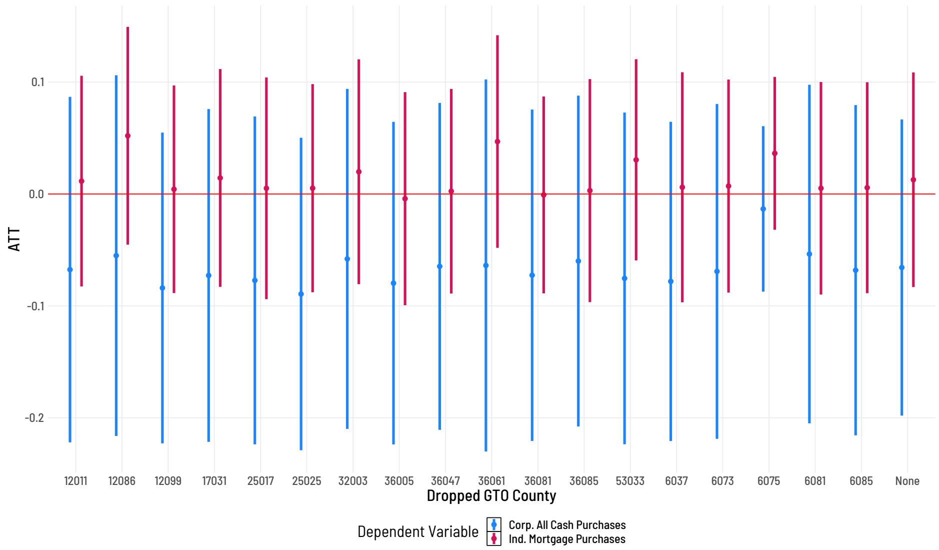
Note:
Models estimated using the did package in R. Unit of analysis is the county-month, including all transactions. Counties are coded as treated after the first GTO for any price threshold is announced. Standard errors are clustered at the county level. Control group: never-treated. Sample includes 18 GTO counties. Included pre-treatment covariates are: County level GDP in 2015 (ln) and median sales price 2015 (ln). Group ATTs calculated based on 12 pre- and post-treatment months.

Table B.8: Main Results Corporate & Individual Purchases Bracket Models

	Corporate All-Cash		Individual Mortgages	
	500k	GTO Threshold	500k	GTO Threshold
	(1)	(2)	(3)	(4)
Average	0.052	0.025	0.190	0.196
	[−0.061, 0.166]	[−0.111, 0.162]	[0.004, 0.376]	[−0.023, 0.415]
March 2016	−0.226	−0.523	0.110	0.012
	[−0.702, 0.250]	[−0.924, −0.122]	[−0.169, 0.390]	[−0.227, 0.250]
August 2016	0.105	−0.001	0.086	0.094
	[−0.035, 0.245]	[−0.199, 0.197]	[−0.038, 0.210]	[−0.049, 0.236]
November 2018	0.045	0.108	0.311	0.287
	[−0.172, 0.262]	[−0.137, 0.352]	[−0.128, 0.750]	[−0.107, 0.681]
No. Obs.	2893	1841	2893	1841

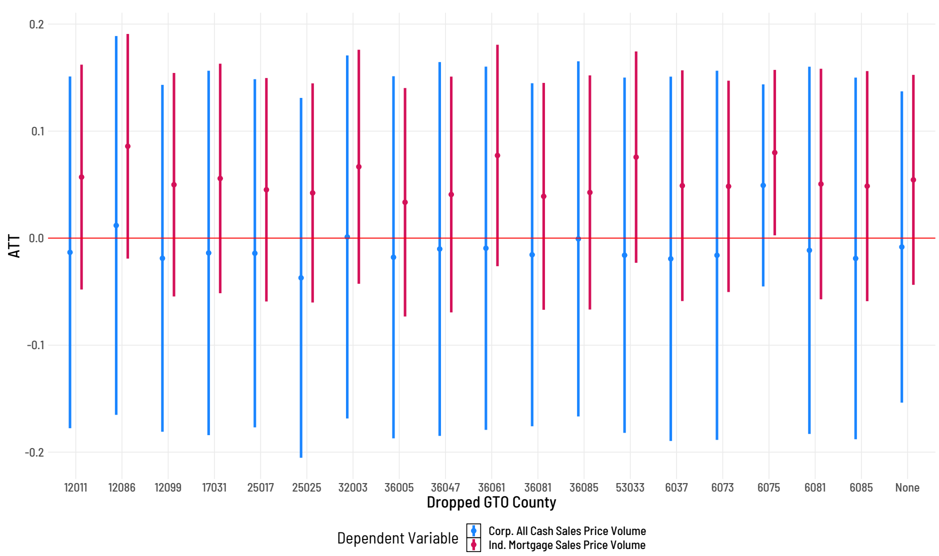
Note:
Models estimated using the did package in R. Unit of analysis is the county-price bracket-month, including all transactions. Standard errors are clustered at the county-gto threshold level. Control group: not-yet-treated. Sample includes 18 GTO counties. Included pre-treatment covariates are: County level GDP in 2015 (ln) and median sales price 2015 (ln). Group ATTs calculated based on 12 pre- and post-treatment months.

Figure B.10: No Corporate Cash Purchases - Average Group ATT – Dropping Individual GTO Counties



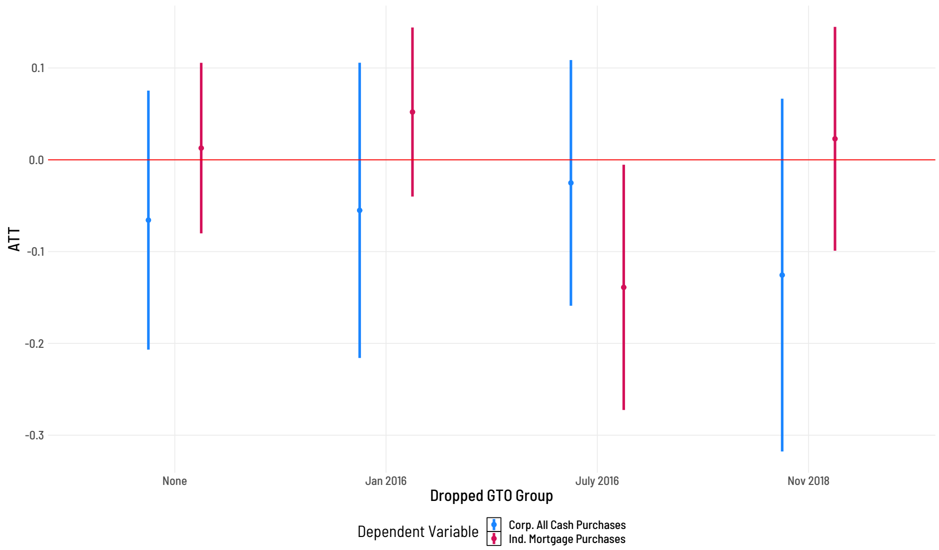
Note: Figure shows the overall average group ATT for corporate all cash purchases (blue) and individual mortgage purchases (red) as the dependent variables when dropping individual GTO counties. The overall ATTs are quite stable across the different samples.

Figure B.11: Price Volume Corporate Cash Purchases - Average Group ATT – Dropping Individual GTO Counties



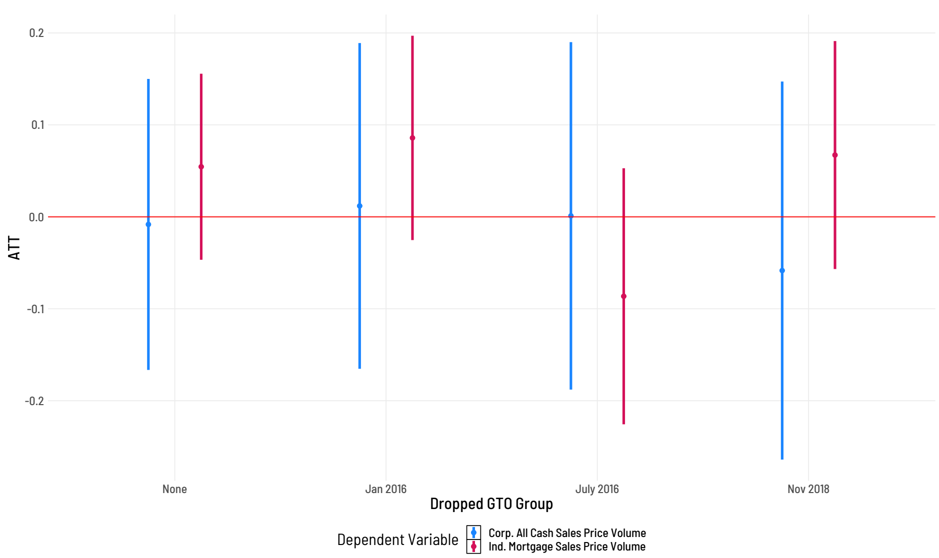
Note: Figure shows the overall average group ATT for corporate all cash purchases price volume (blue) and individual mortgage purchases price volume (red) as the dependent variables when dropping individual GTO counties. The overall ATTs are quite stable across the different samples.

Figure B.12: No Corporate Cash purchases - Average Group ATT – Dropping Treatment Groups



Note: Figure shows the overall average group ATT for corporate all cash purchases (blue) and individual mortgage purchases (red) as the dependent variables when dropping specific treatment groups (GTO announcements).

Figure B.13: Price Volume Corporate Cash purchases - Average Group ATT – Dropping Treatment Groups



Note: Figure shows the overall average group ATT for corporate all cash price volume (blue) and individual mortgage price volume (red) as the dependent variables when dropping specific treatment groups (GTO announcements).

Table B.9: Suspicious Patterns – No. Purchases

	All-Cash Corporate Purchases		
	Formation Agents	Newly Incorp.	Secretive State
	(1)	(2)	(3)
Average	0.185	−0.054	−0.018
	[−0.201, 0.572]	[−0.285, 0.178]	[−0.294, 0.258]
March 2016	−0.005	−0.322	−0.497
	[−0.311, 0.301]	[−0.582, −0.063]	[−0.731, −0.262]
August 2016	0.242	−0.007	0.149
	[−0.378, 0.863]	[−0.337, 0.324]	[−0.217, 0.515]
November 2018	0.136	−0.049	−0.193
	[−0.122, 0.395]	[−0.481, 0.382]	[−0.443, 0.057]
No. Obs.	263	263	263

Note:
Models estimated using the did package in R. Unit of analysis is the county-month, including all transactions. Counties are coded as treated after the first GTO for any price threshold is announced. Standard errors are clustered at the county level. Control group: not-yet-treated. Sample includes 18 GTO counties. Included pre-treatment covariates are: County level GDP in 2015 (ln) and median sales price 2015 (ln). Group ATTs calculated based on 12 pre- and post-treatment months.

B.1 Appendix: Declines in transactions more likely to be illicit

Table B.10: Suspicious Patterns – Total Price Volume

	All-Cash Corporate Price Volume		
	Formation Agents	Newly Incorp.	Secretive State
	(1)	(2)	(3)
Average	-0.029	0.131	0.494
	$[-2.439, 2.382]$	$[-0.330, 0.592]$	$[-1.320, 2.308]$
March 2016	0.491	0.237	-0.060
	$[-0.806, 1.789]$	$[-0.259, 0.734]$	$[-0.540, 0.420]$
August 2016	0.150	0.357	0.326
	$[-3.592, 3.893]$	$[-0.222, 0.936]$	$[-2.553, 3.206]$
November 2018	-0.630	-0.410	1.085
	$[-2.893, 1.632]$	$[-1.734, 0.915]$	$[-1.853, 4.023]$
No. Obs.	263	263	263

Note:
Models estimated using the did package in R. Unit of analysis is the county-month, including all transactions. Counties are coded as treated after the first GTO for any price threshold is announced. Standard errors are clustered at the county level. Control group: not-yet-treated. Sample includes 18 GTO counties. Included pre-treatment covariates are: County level GDP in 2015 (ln) and median sales price 2015 (ln). Group ATTs calculated based on 12 pre- and post-treatment months.

Table B.11: Substitution Patterns – No. Purchases

	Trust Purchases	Corp. Purchases	Corp. Purchases
	All-Cash	Mortgage Bad Bank	Mortgage Foreign Bank
	(1)	(2)	(4)
Average	−0.127	0.006	−0.483
	[−0.384, 0.130]	[−0.245, 0.258]	[−0.773, −0.193]
March 2016	−0.450	−0.146	−1.088
	[−0.753, −0.147]	[−0.573, 0.281]	[−1.496, −0.680]
August 2016	−0.098	0.066	−0.308
	[−0.459, 0.262]	[−0.306, 0.437]	[−0.705, 0.090]
November 2018	−0.060	−0.063	−0.627
	[−0.539, 0.419]	[−0.456, 0.330]	[−1.397, 0.143]
No. Obs.	263	263	263

Note:
Models estimated using the did package in R. Unit of analysis is the county-month, including all transactions. Counties are coded as treated after the first GTO for any price threshold is announced. Standard errors are clustered at the county level. Control group: not-yet-treated. Sample includes 18 GTO counties. Included pre-treatment covariates are: County level GDP in 2015 (ln) and median sales price 2015 (ln). Group ATTs calculated based on 12 pre- and post-treatment months.

B.2 Appendix: Substitution into other forms of purchases

Table B.12: Substitution Patterns – Total Price Volume

	Trust Purchases	Corp. Purchases	Corp. Purchases
	All-Cash	Mortgage Bad Bank	Mortgage Foreign Bank
	(1)	(2)	(4)
Average	0.016	0.202	−4.461
	[−0.969, 1.001]	[−0.305, 0.709]	[−8.002, −0.920]
March 2016	−0.205	−0.377	−4.914
	[−0.630, 0.220]	[−1.153, 0.400]	[−8.801, −1.026]
August 2016	−0.170	0.293	−2.995
	[−1.759, 1.419]	[−0.683, 1.269]	[−8.106, 2.115]
November 2018	0.514	0.233	−7.503
	[−2.362, 3.389]	[−0.371, 0.837]	[−17.147, 2.141]
No. Obs.	263	263	263

Note:
Models estimated using the did package in R. Unit of analysis is the county-month, including all transactions. Counties are coded as treated after the first GTO for any price threshold is announced. Standard errors are clustered at the county level. Control group: not-yet-treated. Sample includes 18 GTO counties. Included pre-treatment covariates are: County level GDP in 2015 (ln) and median sales price 2015 (ln). Group ATTs calculated based on 12 pre- and post-treatment months.

B.3 Appendix: identifying banks more likely to facilitate illicit transactions

We consider three different, potentially-overlapping categories of financial institutions that more likely to have compliance deficiencies and thus be a target for individuals attempting to purchase property without a great deal of scrutiny:

1. **Banks with a history:** smaller financial institutions that have a history of compliance failings, as proxied by those that have been subject to enforcement actions by US financial regulators.

2. **Foreign banks**: small, foreign-owned bank branches.

3. **Small players:** lenders that make up a very small percentage of the total mortgage market.

In this section, we describe how we identify each of these three groups, how we identify 'small' banks, and how we match this information to the Zillow data on mortgage providers.

B.3.1 Identifying banks with a history of enforcement actions

To identify banks that have had systematic compliance failings, we turn to data on enforcement actions by federal agencies spanning the 2001-2020 period.[39] We use enforcement action lists produced by the following regulators:

- Federal Deposit Insurance Corporation (FDIC)
- Federal Reserve
- The Financial Crimes Enforcement Network (FINCEN)
- The National Credit Union Association (NCUA)
- The Office of the Comptroller of Currency (OCC)
- The Office of Thrift Supervision (OTS)

Most of these regulator publish lists of prior enforcement actions which are machine-readable and can be linked through unique institution-specific identifiers. We focus on enforcement actions that are levied directly against financial institutions (as opposed to individuals working in

[39] Specifically, we focus on enforcement actions taken following the introduction of the Patriot Act in 2001, after which the US government drastically increased its scrutiny of and enforcement over financial institutions.

Table B.13: Sources of enforcement action data

Regulator	Sector	Enforcement Actions Used	Number of entities sanctioned
FDIC	Depository Institutions, including state chartered banks not part of the Fed System	Civil Money Penalties; Cease and Desist Orders; Termination of Insurance; Prohibition/Removal Orders	2,585
Federal Reserve	State and National Banks that are members of the Federal Reserve System	Civil Money Penalties; Cease and Desist Orders; Order to Terminate Activities; Consent Orders; Written Agreements	1,415
FINCEN	All financial institutions	All available	55
NCUA	Credit Unions	All available	341
OCC	National banks and federal savings associations	Civil Money Penalties; Cease and Desist Orders; Prohibition Orders	697
OTS*	Federal savings associations	Civil Money Penalties; Cease and Desist Orders; Removal/Prohibition Orders; Supervisory Agreements	435

Notes: *The Office for Thrift Supervision (OTS) was the relevant regulator for Federal savings associations until July 2011, after which the responsibility moved to the Office of the Comptroller of Currency (OCC).

financial institutions). Most regulators do not specify whether an enforcement action is specifically for money-laundering related compliance failings[40] When possible we omit enforcement actions which are clearly linked to capital requirements. The source of our data, which sectors each regulator covers and the enforcement actions we drew on are presented in Table B.13.

B.3.2 Identifying foreign-owned banks

To identify foreign-owned bank, we use the Federal Reserve's list of U.S. Banking Offices of Foreign Entities, keeping all offices active between 2010-2020 (a total of 570).

B.3.3 Creating a master list of banks and merging with ZTrax data

We assemble our master list of financial institutions from three sources: (i) the FDIC's database of 27,624 FDIC-insured institutions and their 86,148 branches, (ii) the Federal Reserve list of 570 foreign-owned banks described above and (iii) a list of 5,205 credit unions identified from a database maintained by the National Information Center.

[40]This is often specified in the text of the enforcement action, but not in the published metadata.

We then merge in enforcement action data from each of the six regulators. For most of these we do it based on the unique regulator ID that is assigned to each institution. For the Federal Reserve and NCUA enforcement action list - which do not include specific identifiers - we match using the fuzzy matching command matchit in Stata.[41]

As we desire to focus only on smaller banks, we identify these by filtering out larger commercial banks. We do this by merging our data with the Federal Reserve's Large Commercial Banks release, a quarterly list of banks that have consolidated assets of $300 or more. As of September 30, 2020, the list comprises over 2,000 banks - both those that have a national charter and those licensed at the state level.

Following this, we merge our ZTrax data to our master list using a fuzzy string matching algorithm. To facilitate the merge, we used the same process to clean and standardize the names of banks and all mortgage lenders in both the master list and the ZTrax data. We then merged the two datasets in Postgres using a conservative string matching threshold, manually checking the results.

C Synthetic Control Method with Staggered Adoption

In this section, we implement a generalization of the popular synthetic control method (SCM) that can evaluate the staggered adoption of the GTOs (Ben-Michael, Feller, and Rothstein, 2021). This generalization first bounds the error on the weighted average effect of control units produced under the single-unit SCM approach (Abadie, Diamond, and Hainmueller, 2010, 2015), and identifies two types of imbalance (unit-specific and global) that result from that fit. The generalized SCM then implements a partial-pooling approach to minimize the average of these two imbalances. When intercept shifts between treated units and their synthetic control are incorporated, this generalized approach approximates the weighted difference-in-differences (DiD) estimator as developed by Callaway and Sant'Anna (2021b).

The input data structure for the generalized SCM is essentially identical to that used above with the doubly robust DiD estimation method of Callaway and Sant'Anna (2021b). We use the *multisynth* command from the *augsynth* package in R, using data at the county-month level. All models combine the synthetic control estimates with a unit fixed effects model, which only estimates the partial-pooling model after outcomes are de-meaned. We also allow the R package to algorithmically choose the heuristic value *nu* based on how well separate versus pooled synthetic

[41]Keeping only matches with above an 85% match score. Credit unions are further matched based on their state of operation.

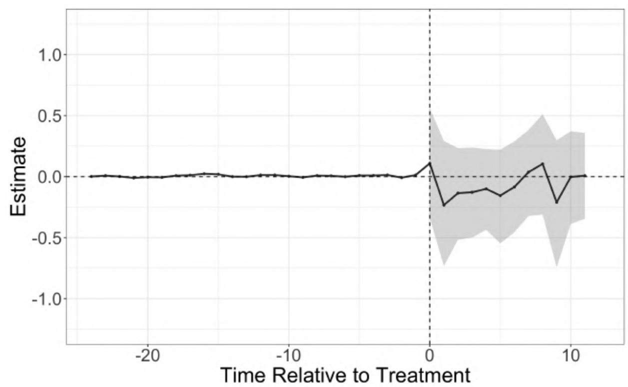
controls balance the overall average. We use 24 months of pre-treatment outcome data to fit the models, and estimate post-treatment outcomes for 12 months after each unit (county) is treated with a GTO. The treatment date used is when the GTO was first announced in each county, the sample used is described in Column 2, Table 1.

Figure **??** plots the results from the SCM with staggered adoption using the two main outcomes variables related to transactions above a threshold of $1 million: all-cash corporate purchases (Panels A and B) and individual mortgage purchases (Panels C and D). Though several of the GTOs instituted lower thresholds for legal entities to declare beneficial ownerships, as in the main text, a $1 million threshold allows us to better compare units that were treated at different times over the evolution of the GTO policy.

First, the generalized SCM returns strong global and individual balance between the weighted control units and the treated units in the pre-treatment period. Average treatment effects for all-cash corporate purchases (Panels A and B) are close to zero and noisily estimated. More telling are the plots showing no visible trends, either positive or negative, after the GTO announcements. The plot lines go both above and below zero in the 12 months following treatment, suggesting the absence of a long-term effect of the GTOs on corporate buying activity. Panels C and D shows the results of a placebo test, where the outcome is individual mortgage purchases. Here we see similarly sized, statistically insignificant effects of the GTOs, suggesting a downward trend in sales activity unconnected to the new transparency requirements. Overall, the results of the SCM method align with the difference-in-differences approach analyzed in the main text.

Figure C.14: SCM with Staggered Adoption, Transactions above $1 million

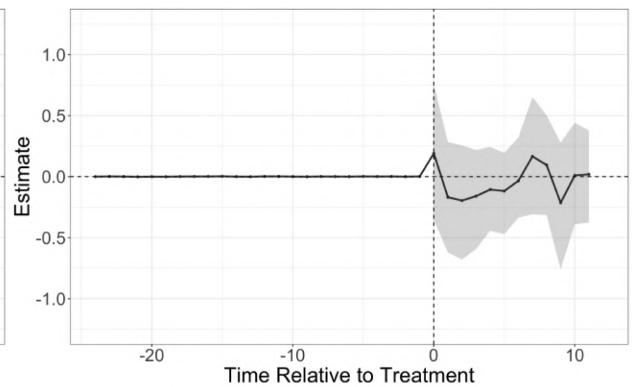
(a) Corporate All-Cash, No Time Cohorts



ATE: -0.066 (0.145)

Global Imbalance: 0.004, Individual Imbalance: 0.089

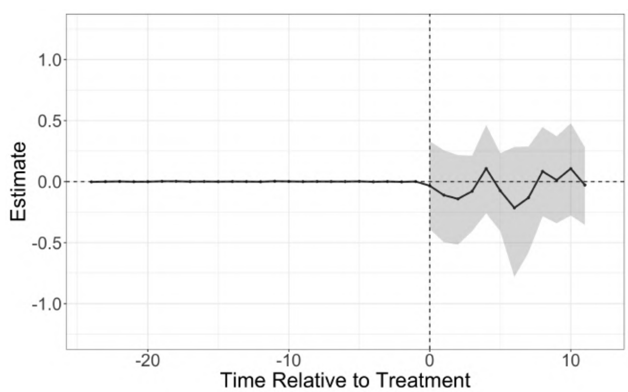
(b) Corporate All-Cash, Time Cohort Avg.



ATE: -0.043 (0.13)

Global Imbalance: 0, Individual Imbalance: 0.016

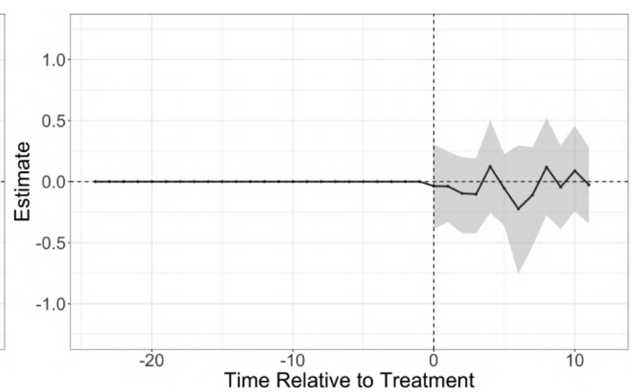
(c) Individual Mortgages, No Time Cohorts



ATE: -0.042 (0.132)

Global Imbalance: 0, Individual Imbalance: 0.048

(d) Individual Mortgages, Time Cohort Avg.



ATE: -0.033 (0.133)

Global Imbalance: 0, Individual Imbalance: 0.027

Notes: this Figure plots the results from the SCM with staggered adoption using two outcomes variables at the county-month level: the number of corporate all-cash purchases over $1 million, IHS transformed (Panels A and B) and the number of individual mortgage purchases over $1 million, IHS transformed (Panels C and D). The left panels show the results without averaging treatment units into 'time cohorts', while the right panels collapse them into these groups. The average treatment effect across all post-treatment time units is given underneath each plot, with the jackknife standard error shown in parentheses.