Chinese Foreign Real Estate Investment and Local Voting in U.S. Presidential Elections*

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Abstract

Despite the increasing globalization of housing markets, little is known about its political implications. This study investigates whether rising Chinese investments in U.S. homes influenced local voting in recent U.S. presidential elections. Building on pocketbook/sociotropic voting and nativism theories, I develop hypotheses on the electoral consequences of foreign real estate investment through greater home demand and equity, improved local economies, and changing neighborhoods. Using difference-in-differences designs that combine a unique shock to Chinese capital outflows in 2013 with county-level measures of local attractiveness to Chinese investments, I find that greater exposure reduced Democratic vote shares in both the 2016 and 2020 presidential elections. Furthermore, an initially larger white population strengthened this effect while a larger college-educated population weakened it. In contrast, local equity gains, housing competition, or economic strength did not systematically influence the effect. Together, the results appear more consistent with the pro-conservative effects of nativism.

Key Words: Foreign Real Estate Investment, China, Elections, Immigration

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1 Introduction

Few economic issues affect people as personally and universally as housing. It represents most people's single largest asset, and its associated mortgage debt is one of the biggest financial assets in most countries (Schwartz and Seabrooke 2009, 1). Furthermore, it has an arguably broader impact than even employment—while pensioners and children may not seek jobs, everyone needs housing (Dorling 2014). No example better demonstrates the significance of housing as the devastating U.S. Subprime Mortgage Crisis (Zonta, Edelman, and McArthur 2016).

The increased globalization of housing markets through Foreign Real Estate Investment (FREI) is thus deeply contentious. In the 1980s, FREI from East Asia introduced strong political emotions about race, wealth, and inequality in many Anglosphere markets, including the United States (Ley 2011). In recent years, the FREI activities of wealthy investors from BRIC countries[1] have revived many of these emotions (Rogers and Koh 2017). For example, Chinese purchases of residential property in the United States grew from \$4 billion in 2009 to approximately \$32 billion in 2017 (National Association of Realtors 2021; Liao, Malhotra, and Newman 2020). This recent surge of global Chinese real estate investments coincides with an uneven recovery of home prices in the United States after the Global Financial Crises (Zonta, Edelman, and McArthur 2016), anti-immigration sentiments in Canada (Gordon 2016), and tighter regulations on foreign property ownership in Australia, Canada, and Hong Kong (Phillips 2016; Pearson 2016; The Economist 2017).

Despite the increasing political salience of FREI in developed countries, we still know relatively little about the extent and ways through which it shapes politics in investment-receiving countries. To date, our understanding of economic globalization and its political consequences rely mainly

[1] Brazil, Russia, India, and China.

on labor market explanations. Such insights stem primarily from the trade literature (e.g., Autor, Dorn, and Hanson 2013; Rickard 2022) and increasingly from the literature on Foreign Direct Investment (FDI) or immigration (e.g., Owen 2019; Malhotra, Margalit, and Mo 2013). This is a critical oversight because housing can play an equal, if not more significant, role than labor markets in driving political attitudes and participation (Ansell 2014). Furthermore, as a form of cross-border capital flow, FREI holds several unusual characteristics that pose challenges to existing theories. Most strikingly, each transaction can entail both the cross-border movement of money *and* people. This feature implies political consequences that can be more complex than investments that only involve the exchange of assets.

To be sure, a fast-growing literature has improved our understanding of the effects of housing on political attitudes, behavior, and elections.[2] However, most of this research has focused on the effects of housing prices and local resident characteristics (e.g., homeownership) without considering the political ramifications of homebuyer characteristics (e.g., foreign vs. native). Such omissions can lead to biases when estimating housing's political effects, especially given the rise of financial globalization. Additionally, housing prices are often endogenous to domestic policies and local economic factors that can be affected by political outcomes of interest (e.g., elections), raising inference challenges when estimating the effects of housing. Thus, while anecdotal reports on the political effects of FREI exist (e.g., Gordon 2016), rigorous empirical assessments are still lacking.

To fill this gap, I focus on the case of Chinese FREI and investigate the extent to which a recent shock in Chinese acquisitions of U.S. residential real estate influenced subsequent local voting patterns in U.S. presidential elections. Building on existing theories of material self-interests,

[2]See, e.g., Ansell (2014), Hankison (2018), Ansell, Broz, and Flaherty (2018), Larsen et al. (2019), Adler and Ansell (2020), and Hall and Yoder (2022).

sociotropic politics, and nativism, I hypothesize that FREI can affect presidential votes through three main mechanisms. From a *pocketbook* voting perspective (Lewis-Beck and Stegmaier 2000), a sudden and massive influx of foreign investments should raise home prices, which can then increase home equity and homeowners' electoral support for the incumbent presidential party, especially for many U.S. households who fell into negative equity after the housing market crash. Yet, the same influx may also increase housing competition, triggering grievances and reducing non-homeowners' electoral support. From a *sociotropic* voting point of view, a surge in FREI stimulates the local economy (Liao, Malhotra, and Newman 2020), which can then positively shape voters' perception of the national economy and lead citizens to reward the incumbent presidential party (Ansolabehere, Meredith, and Snowberg 2014; Larsen et al. 2019), regardless of their stakes in homeownership. Lastly, non-material concerns such as *nativism* may also shape local voting in presidential elections. Massive foreign acquisitions of residential property in neighborhoods can create significant changes in local ethnic composition that challenge existing group identities and statuses, triggering natives' threat perception, anti-immigration sentiments, and political dissatisfaction. Such sentiments may then raise support for conservative nationalist parties (Mutz 2018) or undermine support for the incumbent party since managing capital account policies and immigrant entry are primarily the domain of the national government.

To test the electoral effects, I leverage a countrywide anti-corruption campaign in China that began in 2013 as an exogenous shock that triggered a surge in Chinese FREI to the United States. This identification strategy follows Liao, Malhotra, and Newman (2020) who use the shock to examine the local economic and attitudinal consequences of Chinese FREI activities in the United States. Because foreign purchases of residential property help support both child education and portfolio diversification (Rosen et al. 2016), U.S. localities that had a greater initial population of affluent

Chinese international students attracted a larger influx of Chinese FREI after the shock, while localities with a smaller population did not (National Association of Realtors 2016; Liao, Malhotra, and Newman 2020). As such, the identification strategy exploits the wide local variation in Chinese FREI exposure stemming from the exogenous anti-corruption campaign in China. To systematically capture this variation, I use original individual-level government data on all international students in the United States between 2000 and 2016 to construct student population measures at highly disaggregated levels (e.g., ZIP code).

I then conduct several Difference-in-Differences (DiD) analyses to investigate whether presidential votes in localities more exposed to Chinese FREI changed systematically after the shock. I find that local Chinese FREI exposure stemming from the anti-corruption campaign decreased county-level two-party Democratic vote shares in both the 2016 and 2020 presidential elections compared to 2012. Furthermore, this finding is robust to a variety of placebo tests, model specifications, and treatment versions. Exploring the potential underlying mechanisms, I find that an initially larger white population strengthened this effect while a larger college-educated population weakened it. In contrast, local measures of equity gains or housing competition did not consistently moderate the effect. Lastly, while Chinese investments can boost the local economy, I find no evidence that exposure affected presidential vote shares through the local economy's strength. Together, the results suggest that the U.S. electoral effects of Chinese FREI exposure may have originated from nativist concerns and operated through a pro-conservative (Republican) party channel.

The findings provide a new theoretical angle to the study of the politics of economic globalization. A vast literature seeks to understand how the local consequences of economic globalization affect mass political behavior and electoral outcomes (Margalit 2011; Jensen, Quinn, and Weymouth 2017; Owen 2019; Autor, Dorn, Hanson, and Majlesi 2020; Broz, Frieden, and Weymouth 2021).

This literature, however, has mainly focused on trade and its labor market effects. The study's focus on FREI joins emerging research (e.g., Liao, Malhotra, and Newman 2020) that expands our current understanding to housing market effects. By theorizing and empirically examining the various cross-cutting effects of FREI, the study also responds to recent calls in the field (Walter 2021) for researchers to explore the extent and conditions under which material or non-material factors co-exist or interact in explaining globalization support and backlash (e.g., Mansfield and Mutz 2009; Goldstein and Peters 2014; Norris and Inglehart 2019; Baccini and Weymouth 2021).

2 Chinese Foreign Real Estate Investment in the U.S.

Chinese FREI activities in the United States started roughly in 2009, accelerated beginning in 2013, and peaked around 2017. In particular, the magnitude of Chinese residential property purchases increased from $4 billion in 2009 to approximately $32 billion in 2017 (National Association of Realtors 2021; Liao, Malhotra, and Newman 2020). This trend equates to growth from only 2% of total U.S. FDI net inflows in 2009 to nearly 10% by 2017 (World Bank 2021). However, the data also show that Chinese FREI was mostly stable between 2010 and 2012, with growth happening mainly between 2013 and 2017, increasing by around $5 billion a year (National Association of Realtors 2021; Liao, Malhotra, and Newman 2020). In 2013, China became the biggest foreign buyer of residential properties in the United States in terms of total dollar volume, even outpacing Canada (National Association of Realtors 2021). In 2015, China alone purchased nearly 30% of all residential property sold to foreigners (National Association of Realtors 2021).[3] Survey data indicate that these investments concentrate heavily in California, where most Chinese international students in the United States are located (Rosen et al. 2016; National Association of Realtors 2016).

[3]See Appendix Figure A.1 for recent trends.

These patterns reflect two broader developments in China. First, the onset of Chinese FREI activities in 2009 corresponds to the start of China's effort to promote the international use of its official currency, i.e., the Renminbi (RMB), in financial markets, international trade, and FDI (Liao and McDowell 2015; Prasad 2016; Liao and McDowell 2016). The relaxation of capital outflow controls aided investors in diversifying their investment portfolio in light of increasing economic risks in China. These risks include the fear of market slowdown, growing housing bubbles in some second-tier cities, and more volatile exchange rates following RMB internationalization (Reuters 2016; Fuller 2016; Meyer 2016). These factors, combined with lower U.S. home prices near the end of the housing crisis, contributed to the start of Chinese investments in the United States.

Second, the sudden surge in Chinese investments since 2013 follows the onset of China's anti-corruption campaign and growing political risks. As Liao, Malhotra, and Newman (2020, 486) discuss, thousands of government officials have been "investigated, arrested, or sentenced in criminal corruption cases" since the campaign began in November 2012. As a result, the large-scale crackdown on "party members, high-level officials, and civil servants led to fear of political targeting and asset expropriation, which helped accelerate Chinese capital flight."[4] In 2016, government estimates indicated that approximately $300 billion of capital fled China, with a record $45 billion net capital outflow in September of that year (Bloomberg News 2017). These patterns serve as the basis for using the timing of the anti-corruption campaign in China as an exogenous shock to Chinese FREI flows to the United States.

[4] Ibid. See also Searcey and Bradsher (2015), Prasad (2016), and Cooley and Sharman (2017).

3 The Electoral Implications of FREI

What are the implications for politics when foreigners suddenly emerge as prominent buyers in local housing markets? As a first step towards addressing this broad question, this study focuses on presidential elections, one of the most important outcomes for mass political behavior. In particular, the study asks the extent, ways, and conditions under which a sudden and massive influx of FREI may influence local presidential votes.

The existing literature, however, offers limited guidance in answering these questions. First, our current understanding of economic globalization and its domestic political consequences relies mainly on labor market explanations. For example, the trade literature shows that rising U.S. imports from developing countries, especially China, increase unemployment and reduce wages in local labor markets exposed to competition (e.g., Autor, Dorn, and Hanson 2013). These adverse trade shocks, in turn, can hurt the incumbent in presidential elections (Jensen, Quinn, and Weymouth 2017). Moreover, government compensation through job training and income assistance programs can mitigate such anti-incumbent effects (Margalit 2011). Beyond trade, the FDI literature finds that greenfield investment projects can create jobs and raise wages, which helps the re-election of incumbent parties (Owen 2019). Likewise, the literature on immigration has also emphasized the importance of labor market competition in shaping domestic political attitudes (e.g., Malhotra, Margalit, and Mo 2013).

The role of housing has, however, been neglected in this focus on the labor market. This oversight occurs even though *asset values* can be as important as labor market income in driving political attitudes and participation. For example, researchers find that the local contractionary effects of trade liberalization on home prices can reduce homeowners' support for free trade (Scheve and Slaughter 2001). Furthermore, since housing is a core part of an individual's permanent income and self-insurance, housing prices may have an even larger effect than income on political attitudes (Ansell 2014). More broadly, the oversight occurs despite studies finding no significant difference in preferences toward globalization between individuals who are employed and those who are not in the labor force, such as retirees (e.g., Mansfield and Mutz 2009). Together, these findings suggest that existing studies may be overlooking an essential channel through which globalization shapes political participation and electoral outcomes.

Second, as a form of cross-border capital flow, FREI holds several unusual characteristics that pose challenges to the existing political economy literature. Most strikingly, FREI transactions commonly entail both the cross-border movement of money and people. According to the National Association of Realtors (2016, 18–19), around 72% of Chinese residential acquisitions in the United States imply permanent or temporary migration (e.g., for a primary residence or the use of a student). In contrast, only 18% of Chinese buyers purchased residential properties exclusively for portfolio investment purposes. The linkage between investment and people flows in FREI activities thus implies political consequences that can be more complex and prevalent than cross-border exchanges of productive assets (FDI) or financial assets (portfolio investment). Moreover, while FREI is commonly lumped together with FDI (IMF 2009, 308), the two capital flows involve very different investors and motivations. Individuals and families are the primary investors in residential real estate markets instead of firms (Rosen et al. 2016). This difference suggests distinct investment motivations. For example, while ownership, location, and internalization advantages influence firms' FDI decisions (Dunning 1981), different factors such as child education (Juwai 2015) and portfolio diversification (Sirmans and Worzala 2003) affect families' FREI decisions.[5] Thus, FREI implies

[5]In this regard, FREI is more similar to migrant remittances in its altruistic or self-interested motivations (Singer

spatial patterns and distributive consequences understudied in the literature.

To address this gap, I hypothesize that rising FREI can affect local voting in presidential elections in three main ways. First, a surge in FREI may affect electoral support for the incumbent presidential party through pocketbook considerations. Growing FREI increases demand for housing which, all else equal, should increase home prices. Rising home prices, in turn, increase home equity which enables homeowners to raise their level of investment and consumption (Ansell 2014). For example, larger home equity represents a more substantial loan to draw on when investing in child education or business start-ups. It also means more room for the present consumption of goods or services. Such economic benefits were especially appealing to many U.S. homeowners (e.g., in suburban or rural areas in the Midwest) who have struggled with negative home equities after the late-2000s housing market crash (Zonta, Edelman, and McArthur 2016). Better personal economic conditions should then lead homeowners to reward the incumbent presidential party, according to an extensive literature on pocketbook voting and presidential elections (e.g., Jensen, Quinn, and Weymouth 2017). However, rising home prices can also bring rent increases and challenges to new home purchases. Such economic drawbacks can create grievances among non-homeowners (Hankinson 2018) and undermine electoral support for the incumbent party. Overall, the net effect of FREI in a locality should thus depend on the initial local homeownership rate. For example, the effect should be more favorable to the incumbent party in areas with higher homeownership rates since more local voters would benefit from asset-value increases stemming from FREI. I summarize the first hypothesis below.

2010). Yet, FREI also differs from remittances in its direction of flow. While remittances tend to flow from developed to developing countries, FREI flows in reverse.

Hypothesis 1 (Pocketbook Voting) *A sudden surge in foreign real estate investment should increase local home prices, which can then increase electoral support for the incumbent presidential party in areas with high rates of homeownership given the benefits of equity gains.*

Second, a surge in FREI may increase electoral support for the incumbent presidential party by stimulating the local economy and creating positive sociotropic perceptions. Increases in housing values can boost the local economy through increased consumption (Berger et al. 2018; Mian, Rao, and Sufi 2013). It may also spawn further property development and investments, which can set off a reinforcing loop that accelerates growth in the local economy (Chaney, Sraer, and Thesmar 2012; Miller, Peng, and Sklarz 2011). Indeed, researchers found that increased exposure to Chinese FREI in U.S. ZIP codes led to increases in local employment rates, median household income, business establishments, and new vehicle registrations (Liao, Malhotra, and Newman 2020). Such local benefits were vital for many communities seeking to recover from the late-2000s housing market crash (Zonta, Edelman, and McArthur 2016), and media reports abound on how residents, companies, and governments were knowledgeable about the link between Chinese investments and better local economic conditions (Liao, Malhotra, and Newman 2020, 481).[6]

Local economic conditions can then shape voters' decision-making in national elections, according to an emerging literature in retrospective voting (Ansolabehere, Meredith, and Snowberg 2014). In fact, a recent study shows that new and frequent encounters with local housing markets can make housing more salient to voters and more influential in their evaluations of the incumbent

[6]For example, U.S. realtors suggest to "buy where the Chinese are buying, because they perpetuate the price increase" (The Economist 2016). Meanwhile, university administrators helped Chinese international students' parents purchase houses, airlines introduced direct flights, and local governments approved new development projects to court Chinese buyers (Searcey and Bradsher 2015).

government (Larsen et al. 2019). It finds that increases in local housing values are associated with increased voters' support for the incumbent government party. Furthermore, the effect is independent of voters' homeownership status, suggesting sociotropic motives. Building on this literature, I expect the local economic stimulating effects of FREI to positively shape voters' perception of the national economy and lead them to reward the incumbent party, independent of their personal stakes in the housing market. I summarize this hypothesis below.

Hypothesis 2 (Sociotropic Voting) *A sudden surge in foreign real estate investment can boost the local economy, which can then increase electoral support for the incumbent presidential party through positive sociotropic perceptions.*

Third, a surge in FREI may invoke nativist sentiments and political discontent. A nascent political economy literature documents strong political opposition towards firm-led inward FDI due to concerns about national security or populist sentiments. For example, Tingley et al. (2015) find strong political opposition to attempts by Chinese companies at mergers and acquisitions with U.S. firms in security-sensitive or economically distressed industries. Feng, Kerner, and Sumner (2021) show that cultural nationalism plays a vital role in explaining why Americans view foreign and, especially, Chinese inward investment more skeptically than domestic investment. Li, Kuang, and Zhang (2019) find that misperceptions about China "buying up" Canadian assets led to strong public disapproval of rising Chinese FDI in Canada.

Foreign acquisitions of domestic residential property may be even more contentious given its link with immigration. A surge in FREI not only crowds out prized neighborhoods but also represents an influx of immigrants who can be perceived as threatening. Studies on racial threat have long contended that geographic proximity to large populations of immigrants can trigger natives' perception of political threat and economic competition, which in turn generates political hostility toward immigrants (Tolbert and Grummel 2003). Existing research on social identity has also argued that immigration-driven changes in local ethnic composition can challenge existing group identities and trigger anti-immigration sentiments. For example, Hopkins (2010) shows that sudden influxes of immigrants combined with salient national rhetoric on immigration explain local anti-immigration attitudes. Newman (2013) finds that large influxes of immigrants invoke anti-immigration sentiments in local areas where the immigrant group had mainly been absent.

Since national governments are the primary decision-makers on capital account and immigrant entry policies, natives' dissatisfaction over foreign investment-driven ethnic changes in their neighborhoods may hurt the incumbent party's evaluations. An elevated threat perception may also lead to increased support for conservative parties and right-wing candidates in national elections. To be sure, directly linking voting decisions to Chinese FREI is difficult as surveys conducted in the United States during the study period have not included specific questions about Chinese FREI (Liao, Malhotra, and Newman 2020, 485). However, existing research analyzing presidential vote changes between 2012 and 2016 shows that perceived threats to white Americans' dominant group status, such as the rise of China or immigration, were the main predictors of reduced support for the incumbent Democratic Party and defection towards the Republican candidate Donald Trump (Mutz 2018). Furthermore, media reports have documented how key local actors (e.g., city councils, companies, real estate agents, homeowner associations, and university administrators) were well aware of the growth in home purchases during this period and associated it with Chinese buyers (e.g., Searcey and Bradsher 2015; Shyong 2015; The Economist 2016). Chinese nationals "buying up" U.S. homes may thus be viewed as the epitome of a status threat (Shyong 2015) that influences local voting patterns, especially when immigration is a nationally salient issue. I summarize the expectations below.

Hypothesis 3a (Nativism: Pro-Right Effects) *A sudden surge in foreign real estate investment can increase support for conservative parties and right-wing candidates as nativists react negatively to foreign purchases of residential property in their neighborhoods.*

Hypothesis 3b (Nativism: Anti-incumbent Effects) *A sudden surge in foreign real estate investment can reduce electoral support for the incumbent presidential party as nativists react negatively to foreign purchases of residential property in their neighborhoods.*

4 Research Design, Data, and Measures

I test the hypotheses above by focusing on local exposure to Chinese FREI in the United States. Measuring Chinese FREI exposure over highly disaggregated geographic units and time, however, is a challenging task. The most direct measure in an ideal world would use transaction-level data on all home purchases by Chinese nationals. Yet, such systematic data are missing because of privacy or proprietary concerns (Liao, Malhotra, and Newman 2020, 486).[7]

Given such challenges, I follow Liao, Malhotra, and Newman (2020) and use Chinese international undergraduate students' local presence prior to the anti-corruption campaign as an empirical proxy for Chinese FREI exposure. The motivation is that these undergraduate students "tend to come from more affluent families" and Chinese FREI tracks "affluent international students because it serves the dual purpose of supporting child education and portfolio diversification" (Liao, Malhotra, and Newman 2020, 486). For example, many Chinese investors cite child education as one of the main reasons for buying real estate in the United States (National Association of Realtors 2016).

[7]The author's inquiries with Zillow and CoreLogic confirm this.

Furthermore, surveys show that most affluent Chinese parents want to send their children abroad to receive an education, especially to the United States (Kwong 2015). Since these parents want the best living conditions for their children studying away from home, many have invested in U.S. residential real estate to secure housing.[8] In addition to motivating investments, the students' comparative advantage in local knowledge and language skills can also help family members overcome significant information asymmetries (e.g., search costs) and transaction costs (e.g., communication) when acquiring or managing foreign real estate.

Such linkages suggest that U.S. localities housing more Chinese international undergraduate students in 2012, immediately prior to the Chinese anti-corruption campaign, should attract more Chinese FREI after Chinese capital outflows accelerated. Indeed, Liao, Malhotra, and Newman (2020)'s DiD analyses find evidence consistent with such patterns. Specifically, they find that ZIP codes housing a greater Chinese international undergraduate student population immediately prior to the anti-corruption campaign experienced larger increases in home prices after the campaign. Furthermore, this price effect barely existed before the campaign and was also absent when using placebo student measures. Their findings provide strong evidence supportive of using Chinese international undergraduate students as an empirical proxy for Chinese FREI exposure.[9] As Liao, Malhotra, and Newman (2020, 486) note, although the initial U.S. geographic distribution of Chinese international undergraduate students is non-random and thus local exposure to investments that

[8] Juwai (2015) reports that many Chinese parents who only have one child would prefer buying an apartment or house for their child "rather than letting their child cohabit with strangers in dorms." Increasingly, Chinese parents may also enter the United States with a tourist visa six months at a time to live and take care of their child (The Economist 2016).

[9] In Appendix B, I replicate and extend Liao, Malhotra, and Newman (2020)'s validity tests to 2020 using the Zillow Home Value Index (ZHVI, Zillow 2021) and including additional controls. I find substantively similar results.

follow them is susceptible to selection bias, the change in Chinese FREI tied to these students before and after the anti-corruption campaign is exogenous to local outcomes. Furthermore, a DiD design is able to account for potential selection biases through between-locality differences in their pre-treatment outcomes (e.g., attitudes, presidential vote, etc.). Lastly, the design's focus on Chinese international student populations in 2012 can also help produce cleaner estimates since they are prior to and not affected by, e.g., the anti-corruption campaign or Trump's anti-China rhetoric or policies.[10]

To measure the local Chinese international undergraduate student population, I obtained individual-level data of all international students who have been in the United States between 2000 and 2016 via a Freedom of Information Act (FOIA) request submitted to the U.S. Immigration and Customs Enforcement. This original dataset documents important student characteristics such as visa class (F-1 Academic Student or M-1 Vocational Student), country of citizenship, school name and location, academic level (e.g., undergraduate or graduate), program major, and start/end dates.[11] Overall, the dataset provides data coverage that is both longer (2000–2016) and wider (all student origin countries at all academic levels) than Liao, Malhotra, and Newman (2020, 486) (three years: 2010, 2012, 2014; two origin countries: China and India; two academic levels: undergraduate and graduate).

[10] There is some similarity between a DiD design and the Bartik (shift-share) design (see Goldsmith-Pinkham, Sorkin, and Swift 2020). For example, both might exploit differences across units that create differential exposure to a common shock and lead to differential changes in the outcome. However, their identification strategies are different. The former relies on the parallel trends assumption, while the latter relies mainly on the exclusion restriction assumption (and primarily the exogenous "shares" assumption).

[11] Figure A.2 shows that China has been the single largest origin country of international students in the United States since 2009.

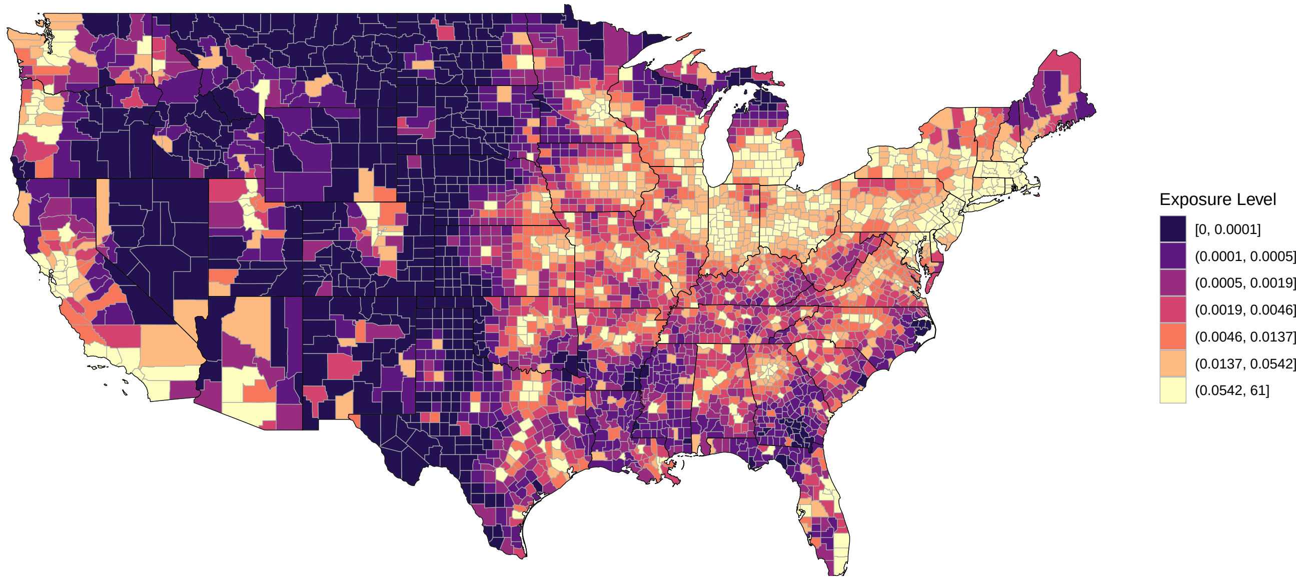

Figure 1: **Chinese FREI Exposure in the Contiguous United States by County.** Exposure is measured using Chinese international undergraduate students per square mile in 2012, immediately prior to China's anti-corruption campaign. Lighter colors represent areas with higher exposure. Bins are based on quantiles.

Using the FOIA data, I computed estimates of the population of Chinese international undergraduate students in each ZIP Code Tabulation Area (ZCTA) and year.[12] I then aggregated ZIP-code estimates up to the county level and divided them by county land area (square miles) to account for the large variation in county sizes. Such normalization is important because, even if the number of Chinese home purchases was the same in counties, a larger land area could reduce the likelihood that local residents were exposed (e.g., observing or learning of Chinese FREI). Furthermore, in larger counties, price effects may be less pronounced while local economic benefits may be more dispersed. Figure 1 illustrates the county-level variation in Chinese FREI exposure per square mile according to the measure.[13] It suggests that counties in states such as California and New York were more exposed to Chinese FREI after the anti-corruption campaign, which is

[12]See Appendix A.2 for details about the construction of the measure.

[13]Appendix Figure A.4 illustrates finer-grained ZIP-code level variation using the greater Los Angeles area as an example.

fairly consistent with survey findings of Chinese FREI destinations during the period (National Association of Realtors 2016).[14]

I also constructed placebo measures of Chinese FREI exposure for Chinese international graduate students and Indian international undergraduate students in a similar fashion. As Liao, Malhotra, and Newman (2020, 482) discuss, these students represent groups most similar to Chinese international undergraduate students but are unlikely to channel Chinese FREI. The first group captures students from the same origin country but "mainly rely on institutional funding and whose families are less capable of investing." The second group captures students at the same level of education and "tend to target similar universities" but are from countries "not tied to China's investors and the anti-corruption campaign."[15] Furthermore, there were no known exogenous capital account policy changes by the Indian government in 2013 (Fernández et al. 2016).

To assess the impact of Chinese FREI exposure on local presidential voting, I merge the above measures with data on county-level vote returns (Leip 2021) before and after the Chinese anti-corruption campaign. Specifically, I calculate each county's two-party Democratic vote share in presidential elections between 2008 and 2020.[16] Analyses comparing the 2012 and 2016 elections would reveal whether voting patterns for the Democratic Party, the incumbent in both elections,

[14]More systematically, Appendix Figure A.5 shows positive state-level correlations between Chinese international undergraduate students and FREI based on NAR survey data.

[15]India is the second-largest origin of international students in the United States (see Appendix Figure A.2) and the third-largest foreign real estate investor after China and Canada in 2015 (see Figure A.1). Data indicate notably high levels of overlap in top universities where Chinese and Indian international undergraduates enroll (Liao, Malhotra, and Newman 2020, 482).

[16]The approach follows recent studies that examine the electoral effects of economic globalization, e.g., Baccini and Weymouth (2021).

systematically changed right before and after the start of the anti-corruption campaign in counties more exposed to Chinese FREI. Analyses comparing the 2008 and 2012 elections would serve as a temporal placebo test to rule out any pre-shock trending effects in U.S. counties. Since Republicans were the incumbent party in 2020 as opposed to Democrats in 2012, analyses comparing the 2012 and 2020 elections would help disentangle whether the findings suggest anti-incumbent or pro-conservative effects. This comparison would also provide a more similar setting where candidates from both parties (Obama in 2012 and Trump in 2020) were seeking a second term. Lastly, the analyses focus on the contiguous United States and the District of Columbia (D.C.), where economic, demographic, and voting data at the county level are systematically available during the study period. Overall, the merged dataset yields 3,107 counties in 48 states, and D.C. Appendix Figure A.3 illustrates the county-level variation in Democratic vote share changes before and after China's anti-corruption campaign.[17]

5 Chinese FREI Exposure and Local Presidential Vote

To estimate the effect of Chinese FREI exposure on county-level presidential votes, I fit the following two-period DiD regression model to data from different election years (2012 vs. 2016, 2012 vs. 2020, and 2012 vs. 2008):

$$Y_{it} = \alpha_i + P_t + \tau_e(P_t D_{i,2012}) + \boldsymbol{\gamma}(P_t \boldsymbol{S}_{i,2012}) + \boldsymbol{\delta}\boldsymbol{U}_{it} + \boldsymbol{\kappa} P_t \boldsymbol{U}_{it} + \epsilon_{it}, \tag{1}$$

where the outcome variable Y_{it} measures the two-party Democratic vote share in county i in election year t (2008, 2012, 2016, or 2020). The treatment-condition variable $D_{i,2012}$ is a dichotomous version of the exposure measure discussed in the previous section, with above-median values equal

[17] Appendix C presents descriptive statistics.

to 1 (high) and 0 otherwise (low). For robustness checks, I also analyze a trichotomous version of the exposure measure (low, medium, high) binned by terciles. The discretization of the continuous exposure measure guards against excessive model dependency and violations of the linear interactive effect assumption (Hainmueller, Mummolo, and Xu 2019).[18] The treatment-period variable P_t is a dichotomous variable assigned the value of 1 for post-treatment years (2013 and after) and 0 otherwise. The variable α_i indicates county fixed-effects. The inclusion of county fixed-effects controls for time-*invariant* county characteristics that may correlate with Chinese international undergraduate students' existence.[19]

The variables $\boldsymbol{S}_{i,2012}$ represent measures of other student groups most similar to Chinese international undergraduate students but are unlikely to channel Chinese FREI as discussed in the previous section (Chinese graduate students and Indian undergraduate students). Since measures of these student groups tend to correlate quite positively with both Chinese international undergraduate students and Democratic vote shares, controlling for them prevents misattributing their positive effects to that of Chinese FREI exposure. Meanwhile, estimates associated with the interaction terms of these student groups ($\boldsymbol{\gamma}$) can also serve as placebo tests since these student groups are unlikely to channel Chinese FREI after the shock, and the effect of Chinese FREI exposure should

[18] Continuous measure results are similar (see Appendix D.1), albeit with less precise estimates in one model. Note that the non-linear effects I find using a trichotomous measure suggest the important linear interaction effect assumption fails. Thus, results based on a continuous measure can be more biased and inconsistent than the binned measures (Hainmueller, Mummolo, and Xu 2019, 172).

[19] For example, college towns may be more liberal but also rebound faster from economic crises, given higher levels of human capital. Both characteristics can correlate with Chinese student presence and affect local votes. Note that student population measures ($D_{i,2012}$ and $\boldsymbol{S}_{i,2012}$) are time-invariant and subsumed by county fixed-effects in the equation.

already be accounted for by $P_t D_{i,2012}$.[20] The variables $\boldsymbol{S}_{i,2012}$ are discretized in similar ways as $D_{i,2012}$.

The variables $\boldsymbol{U}_{it}$ are time-*varying* covariates that account for potential imbalances between treated and control counties. These include an array of county-level demographic or economic characteristics that may influence both local housing markets and the presidential vote.[21] The interaction term $P_t \boldsymbol{U}_{it}$ accounts for potential trending effects stemming from these control covariates. Standard errors are clustered by counties to allow for within-unit correlations of errors. τ_e is the coefficient of interest and represents the DiD estimate of the electoral effect of Chinese FREI exposure.

I fit three versions of equation (1) to the data. The baseline model only includes the interaction term $P_t D_{i,2012}$, the treatment-period dummy variable P_t, and county fixed-effects. An extended model adds interaction terms for other similar international student groups. The full model further includes time-varying controls and their trending effects.

The results reveal three main findings. First, without controlling for pre-existing county-level

[20] This approach is akin to studies (e.g., Autor 2003) that estimate period-decomposed treatment effects within the same model (i.e., treatment status interacted with different leads and lags) and use pre-treatment period estimates as placebo tests. Here, I estimate student-group-decomposed treatment effects within the same model and use student groups that were unlikely to channel Chinese FREI as placebo tests.

[21] I control for population shares by age (5–17, 18–24, 25–34, 35–44, 45–54, 55–64, and 65–), female population share, population shares by race (White, Black, Hispanic, and Asian), Chinese population share, foreign-born population share, the share of the population with college or above education, the share of the population enrolled in college or above, population density (log), effective real estate tax rate (%), trade exposure (Imports Per Worker), employment rate, median household income, and the share of vacant houses. I construct county-level measures of trade exposure following Autor, Dorn, and Hanson (2013) and using data from U.N. Comtrade and County Business Patterns (Census Bureau). The Census Bureau provides data for all the remaining covariates.

characteristics and their correlations with both the treatment condition variable and the outcome, baseline results seem to suggest a small but positive treatment effect (see column (1) of Appendix Table D.1). Such results, however, can be misleading. For example, the data also show that the treatment condition variable 2012 Chinese international undergraduate students per square mile is positively and strongly correlated with per-square-mile 2012 populations of Chinese graduate students (0.90) and Indian undergraduate students (0.83). Yet these latter student groups are unlikely to channel Chinese investment inflows but tend to correlate positively with Democratic vote shares. The baseline results would thus suffer from substantial bias without controlling for such pre-existing county-level characteristics.

Second, once county-level confounders are accounted for in the extended or full models, the results show that counties more exposed to Chinese FREI *decreased* more in presidential votes for the incumbent Democratic Party between 2012 and 2016. As shown in Figure 2, results based on a binary treatment indicate that high-exposure counties saw a 1 percentage point larger decrease in Democratic vote share compared to low-exposure counties (95% C.I. = -1.36 to -0.65), even after accounting for the various time-invariant or time-varying county characteristics discussed above, such as demographics and local trade exposure (see Appendix Table D.1 column (3) for details). Meanwhile, results based on a trichotomous treatment (Appendix Table D.1 column (4)) show that this negative effect grows consistently as exposure levels increase. Compared to low-exposure counties, medium-exposure counties saw a 0.6 percentage point larger decrease (95% C.I. = -0.99 to -0.14), while high-exposure counties experienced a 1.6 percentage point larger decrease (95% C.I. = -2.16 to -1.02).

How large are these effects in substantive terms? For a county in 2012 with a median two-party Democratic vote share of 0.38 (e.g., Pipestone County, MN), a 1 percentage point decrease

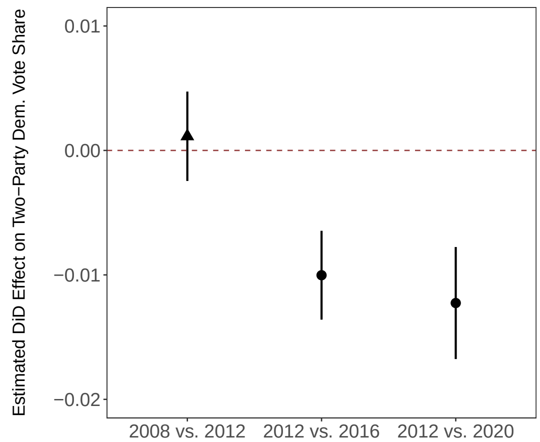

Figure 2: **The Effect of Chinese FREI Exposure on Two-Party Democratic Vote Shares.** The figure summarizes DiD point estimates and 95% confidence intervals based on a dichotomous measure of exposure (low vs. high) and different model periods. It shows that high exposure reduced counties' two-party Democratic vote shares between 2012 and 2016. Furthermore, this effect increased when extending the election year to 2020. In contrast, the point estimate was close to zero and imprecisely estimated in the pre-treatment period between 2008 and 2012 (before the anti-corruption campaign and mass influx of Chinese FREI).

represents a 3% decrease in vote shares. Given that one standard deviation in Democratic vote share in the sample in 2012 is 15 percentage points, it also represents an effect size of around 0.07 standard deviations of the outcome. In comparison, the effect of increasing a county's population density—a strong predictor of the outcome in the 2016 presidential election (Badger, Bui, and Pearce 2016)—from the 25$^{\text{th}}$ percentile to the 75$^{\text{th}}$ percentile increases Democratic vote shares by around 11 percentage points. Thus, the effect of exposure to Chinese FREI is around 9% of the effect size of population density. Lastly, consider the counterfactual thought experiment where high-exposure counties were low-exposure (not treated) and did not experience a one percentage point decrease in Democratic two-party vote share in 2016. Under this scenario, election outcomes data suggest that the Democratic Party may have won 14 additional counties in 9 battleground states, including Florida (Duval, Pinellas, and Seminole), Iowa (Dubuque, Jefferson, and Winneshiek), Michigan (Saginaw), New Hampshire (Hillsborough), North Carolina (Nash), Ohio (Montgomery), Pennsylvania (Erie), Virginia (Chesapeake), and Wisconsin (Kenosha and Sauk). Since some of these states had vote margins around or less than one percent (e.g., Florida, Michigan, and Wisconsin) and several of these counties had large numbers of voters, such results could have potentially brought changes in the overall 2016 presidential election outcome.

Third, this negative effect for the Democratic Party worsened in the 2020 presidential election when Republicans were the incumbent party. Here, the binary treatment in Figure 2 shows a 1.2 percentage point larger decrease between 2012 and 2020 for high-exposure counties compared to low-exposure counties (95% C.I. = -1.68 to -0.76). This result suggests a 20% increase in the negative effect of Chinese FREI exposure compared to the period 2012–2016 (Appendix Table D.1 column (5)). Again, under a trichotomous treatment, the negative effect grew consistently as exposure levels increased (Appendix Table D.1 column (6)), with a 0.8 percentage point larger decrease for medium-exposure counties (95% C.I. = -1.30 to -0.26) and a 2 percentage point larger decrease for high-exposure counties (95% C.I. = -2.71 to -1.28).

To assess the finding's robustness, I implement several placebo tests. I first conduct a temporal placebo test to examine whether greater exposure to Chinese FREI since 2013 predicts changes in Democratic vote shares between 2008 and 2012—right *before* the start of China's anti-corruption campaign and the massive influx of Chinese investments. As shown in Figure 2, the estimate is small and statistically indiscernible from zero (0.001, 95% C.I. = -0.002 to 0.005, see Appendix Table D.2 column (2) for details). This result suggests that the effect originated from the anti-corruption campaign rather than pre-treatment trends in locations housing affluent Chinese international students.

I also conduct placebo tests using international student groups that should not be related to Chinese FREI's spatial distribution. As discussed in Section 4, if my estimates were capturing effects arising from unobserved local characteristics, then the population of Indian international undergraduate students should generate comparable effects as Chinese and Indian international undergraduate students usually enroll in similar universities. Furthermore, if my findings were driven by local reactions to the presence of Chinese nationals in general, then Chinese international graduate students should also generate comparable effects. As shown in column (3) of Appendix Table D.1, the point estimates associated with the interaction term of these student populations are small and both oppositely signed, which suggest modestly *pro*-Democratic party effects that are more commonly expected near universities. These results demonstrate that the electoral effect found above is uniquely tied to Chinese international undergraduate students more capable of channeling Chinese FREI and not simply caused by unobserved characteristics of counties that house larger populations of international students from China or India.

Together, the findings above show that U.S. counties with greater Chinese FREI exposure stemming from China's anti-corruption campaign have increasingly shifted away from Democrats and closer toward Republicans in the 2016 and 2020 presidential elections.

6 Exploring the Mechanisms

Why does greater Chinese FREI exposure appear to influence local U.S. presidential voting patterns? To gain a deeper understanding of the potential mechanisms underlying the net negative effect for Democrats, I conduct further tests below.

Nativism. Existing studies have shown that increased economic globalization (e.g., trade and immigration) can trigger stronger nativist sentiments in majority White population areas in the United States (e.g., Mutz 2018; Autor, Dorn, Hanson, and Majlesi 2020). Following the literature, I examine whether a county's initial White population share in 2012 (pre-shock) moderates the effect of Chinese FREI exposure. That is, if Chinese FREI indeed worked through nativism, it should activate stronger threat perceptions in counties with a larger initial share of their White population. If Hypothesis 3a (Pro-Right Effect) is correct, a larger initial white population should then consistently magnify the negative effect of Chinese FREI exposure for the left-leaning Democratic Party in both the 2016 and 2020 elections. In contrast, if Hypothesis 3b (Anti-incumbent Effect) is correct, stronger threat perceptions and dissatisfaction should magnify the negative effect for the Democratic Party in 2016 when it was the incumbent but attenuate the effect in 2020 when it was the opposition party.[22]

Alternatively, I also analyze whether a county's initial share of its population with at least a college degree moderates the effect. Studies have shown that education is arguably the strongest predictor of attitudes toward globalization, such as immigration, with higher-educated natives being more supportive (Hainmueller and Hopkins 2014). Therefore, even if Chinese FREI exposure triggers threat perceptions, it should trigger lower levels of such perceptions in areas with a larger initial share of their college-educated population. This implies moderation effects in the opposite direction from those based on the initial White population. Here, if Hypothesis 3a (Pro-Right effect) is correct, a larger initial college-educated population and lower resulting threat perceptions should consistently attenuate the negative effect of Chinese FREI exposure for the Democratic Party in both elections. In contrast, if Hypothesis 3b (Anti-incumbent Effect) is correct, a larger initial college-educated population should attenuate the negative effect of Chinese FREI exposure for the incumbent Democratic Party in 2016. In 2020, a weaker dissatisfaction about Chinese FREI in areas with a larger initial college-educated population, all else equal, should benefit the incumbent

[22]The outcome variable is two-party Democratic vote shares. Thus, a larger anti-incumbent effect for the Republican Party in 2020 is equivalent to a smaller negative effect for the Democratic Party.

Mechanism	*Key Variables*	*Type*	*Expected Influence on Negative Effects for Dem.*	
			2016 (Dem. Incumbent)	2020 (Rep. Incumbent)
Pocketbook Voting	Init. Homeownership Rate	Moderator	Attenuate	Amplify
	Init. Vacancy Rate	Moderator	Attenuate	Amplify
Sociotropic Voting	GDP Growth	Mediator	Attenuate	Amplify
	Employment Rate	Mediator	Attenuate	Amplify
Nativism: Pro-Right	Init. White Pop.	Moderator	Amplify	Amplify
	Init. College-Edu. Pop.	Moderator	Attenuate	Attenuate
Nativism: Anti-incumbent	Init. White Pop.	Moderator	Amplify	Attenuate
	Init. College-Edu. Pop.	Moderator	Attenuate	Amplify

Table 1: **Summary of Mechanism Tests: Key Measures and Expectations**

Republican Party and magnify the negative effect on Democrats' two-party vote share. Table 1 summarizes the mechanism tests discussed here and later in the section, comparing key measures and their expected influence on Chinese FREI exposure's negative effect on Democratic vote shares.

To test these implications, I discretize the moderators into three levels based on terciles (small, medium, and large). Again, this approach protects against model dependencies that may stem from excessive extrapolation and the potential lack of common support in interaction models. I then investigate heterogeneity in the effect of Chinese FREI exposure *conditional* on each moderator by fitting the following triple-DiD model to the data:

$$Y_{it} \;=\; \alpha_i + P_t + \tau_e^{'}(P_t D_{i,2012}) + \lambda(P_t M_i) + \boldsymbol{\tau_e^*}(P_t D_{i,2012} M_i) + \boldsymbol{\gamma}(P_t \boldsymbol{S}_{i,2012}) + \boldsymbol{\delta U}_{it} + \boldsymbol{\kappa} P_t \boldsymbol{U}_{it} + \epsilon_{it}, \quad (2)$$

where the model is a simple expansion of equation (1) with heterogeneous treatment effects for Chinese FREI exposure ($P_t D_{i,2012}$). In particular, the coefficient $\tau_e^{'}$ represents the electoral effect of Chinese FREI exposure when the moderator M_i (White or college-educated population in 2012) is assigned its baseline level (small). The coefficients $\boldsymbol{\tau_e^*}$ associated with the triple interaction terms indicate additional effects of Chinese FREI exposure as the moderator moves to other levels (e.g., large). The sum of the coefficients $\tau_e^{'} + \boldsymbol{\tau_e^*}$ thus represents the effects of Chinese FREI exposure for each non-baseline level of the moderator. All remaining variables are the same as those in equation

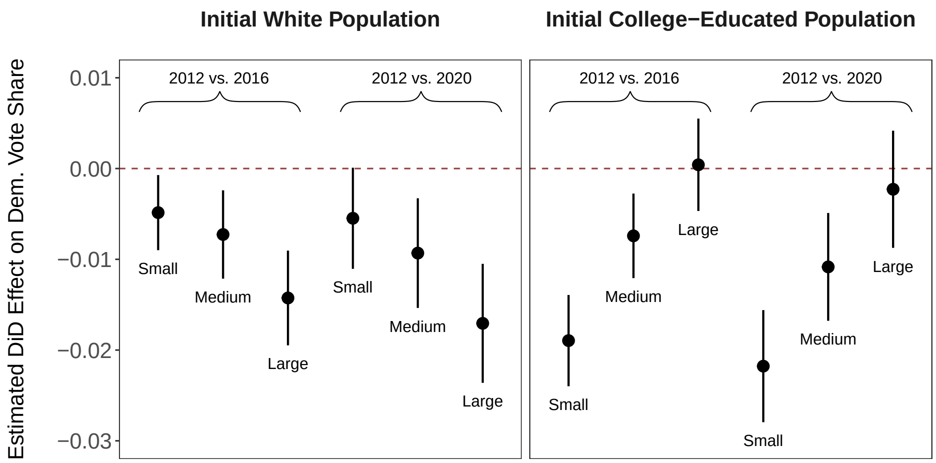

Figure 3: **Effect Heterogeneity: Nativism Mechanisms.** The figure summarizes DiD point-estimates and 95% confidence intervals across models. In support of predictions of a pro-conservative effect, the figure shows that increases in the share of a county's initial white population consistently magnified the negative effect of Chinese FREI exposure on Democratic vote shares (left panel) in both 2016 and 2020. Meanwhile, increases in the share of the initial population with at least college degrees attenuate the negative effect (right panel).

(1), and standard errors are again clustered by counties.[23]

I find results more consistent with Hypothesis 3a (Pro-Right Effect) than Hypothesis 3b (Anti-incumbent Effect). First, as shown in the left panel in Figure 3, a county's initial White population consistently magnifies the negative effect of Chinese FREI exposure on Democratic vote shares in *both* periods. In 2016, Chinese FREI exposure had a negative but smaller baseline effect on presidential votes for the incumbent Democratic Party when a county's initial white population was small (−0.005, 95% C.I. = −0.009 to −0.001). This negative effect exacerbates as the initial white population increases to medium (−0.007, 95% C.I. = −0.012 to −0.002) and large (−0.014, 95% C.I. = −0.019 to −0.009). The estimates suggest that the negative effect of Chinese FREI exposure

[23]Instead of analyzing moderation effects separately, I also fit one joint model that includes all the moderators in this section and their interaction terms simultaneously. In particular, I substitute M_i in equation (2) with $\boldsymbol{M_i}$, a vector of moderators (initial white pop., initial college-educated pop., initial homeownership rate, and initial vacancy rate). As shown in Appendix Figure D.2, substantive findings are similar to those presented in this section.

for the Democratic Party is nearly four times larger in areas with a large initial white population compared to those with a small population, with a precisely estimated difference (−0.009, 95% C.I. = −0.015 to −0.004).[24] This negative effect for the Democratic Party further increased in the 2020 election when Trump (Republican) was the incumbent president. Here, the negative effect in large initial white population counties increased to −0.017 (95% C.I. = −0.024 to −0.011). This larger negative effect is consistent with the stronger anti-China and anti-immigration position taken by the Republican party during the Trump presidency.

Second, the right panel in Figure 3 shows that a county's initial college-educated population consistently attenuates the negative effect of Chinese FREI exposure on Democratic vote shares in both 2016 and 2020. For example, in the 2016 election when the Democratic Party was the incumbent, the negative effect in counties with a small college-educated population was strong and precisely estimated at −0.019 (95% C.I. = −0.024 to -0.014). However, in counties with a large college-educated population, this effect was attenuated to virtually zero (0.0004, 95% C.I. = −0.005 to −0.006). While the overall negative effect of Chinese FREI exposure for Democrats further increased in the 2020 election after Trump's first term, a large initial college-educated population was still able to attenuate the effect to nearly zero (−0.002, 95% C.I. = −0.009 to 0.004).

Altogether, these findings provide supportive evidence of a nativism explanation. Furthermore, the results suggest that Chinese FREI exposure is more likely to have affected electoral outcomes by increasing support for right-leaning conservative parties instead of hurting the incumbent party.

Pocketbook Voting. If the effect of Chinese FREI exposure had worked through material self-interests and pocketbook voting, as summarized in Hypothesis 1, we should expect a higher initial (pre-shock) homeownership rate to consistently attenuate the negative effect of Chinese FREI ex-

[24]See column (1) of Appendix Table D.4 for further details.

posure on presidential votes for the incumbent Democratic Party in 2016. This is because, in counties with higher initial homeownership rates, a larger share of voters would benefit from asset appreciation stemming from Chinese FREI. Meanwhile, Chinese investments and rising home prices should also generate less resentment and political discontent in those counties, given fewer non-homeowners.[25] Following a similar logic, initial homeownership rates should also benefit the incumbent Republican Party in 2020, which means magnifying the negative effect on two-party Democratic vote shares.

As an alternative, I also analyze whether a county's initial housing vacancy rate moderates the effect. Influxes of Chinese FREI in counties with lower vacancy rates, i.e., "hot" housing markets, may trigger stronger housing competition, leading to more grievances from non-homeowners towards the government, especially regarding capital inflow policies (Gordon 2016). Conversely, higher initial vacancy rates indicate cooler local housing markets with a greater capacity to absorb influxes of Chinese FREI and fewer non-homeowner grievances. Thus, if local voting patterns were driven by pocketbook considerations, higher initial vacancy rates should consistently attenuate the negative effect of Chinese FREI exposure for the incumbent Democrats in 2016. In 2020, higher initial vacancy rates should help the incumbent Republican Party mitigate grievances, which exacerbates the negative effect of Chinese FREI on two-party Democratic vote shares.

To test this set of implications, I use data on county-level homeownership rates and vacancy

[25] It is possible that housing supply could be more limited in counties with a low initial homeownership rate, leading to larger asset price increases for homeowners after a FREI shock and stronger homeowner support for the incumbent party. However, the share of voters who would benefit from this larger asset price increase would also be low by definition. Hence, potential positive FREI effects for the incumbent party when the initial homeownership rate is low should be limited. Empirically, the joint model analysis in Appendix Figure D.2 shows that initial homeownership rate results are similar even when explicitly controlling for counties' initial housing supply.

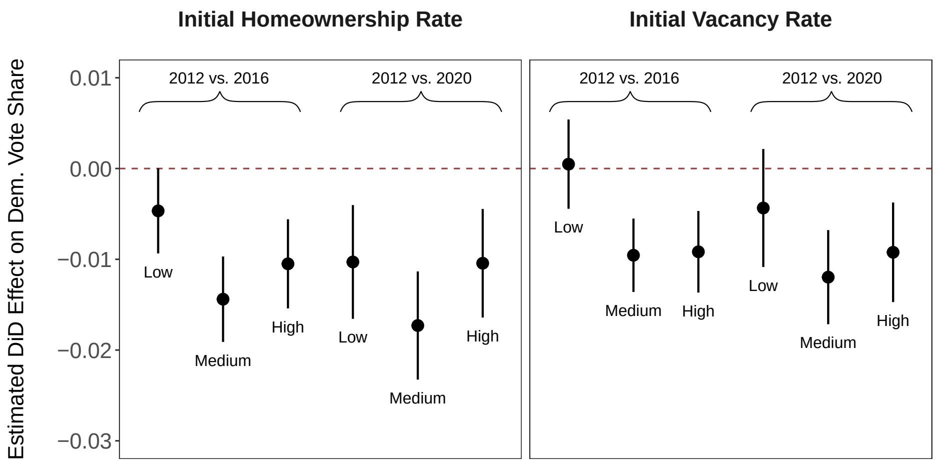

Figure 4: **Effect Heterogeneity: Pocketbook Voting Mechanism.** The figure summarizes DiD point-estimates and 95% confidence intervals across models. Contrary to predictions of pocketbook voting, the left panel shows that initial homeownership rates did not consistently help reduce the negative effect of Chinese FREI exposure on the incumbent Democratic party in 2016. Nor did it consistently help the incumbent Republican party in 2020 by increasing the negative effect on two-party Democratic vote shares. Similarly, the right panel shows that initial vacancy rates, a proxy for lower levels of housing competition, did not consistently help incumbent presidential parties in either election.

rates in 2012 from the Census Bureau. Again, I divide this variable into three categories based on terciles (low, medium, and high). I then conduct a similar triple-DiD analysis based on equation (2). The main difference here is that I substitute in initial homeownership and vacancy rates as moderators in the model.

I do not find evidence that pocketbook concerns were driving the findings. As shown in the left panel of Figure 4, a county's initial homeownership rate does not consistently attenuate the negative effect of Chinese FREI exposure for the incumbent Democratic Party in the 2016 election. Nor does it consistently help the incumbent Republican Party in 2020 and exacerbate the negative effect for Democratic vote shares.[26] Similarly, the right panel of Figure 4 shows that initial vacancy rates do not consistently attenuate or exacerbate the negative electoral effect for the Democratic

[26]These findings are also inconsistent with the potential claim that preferences against redistribution stemming from home equity gains (Ansell 2014) drove support for the right-leaning Republican Party.

Party.[27]

Overall, these findings do not support a pocketbook voting mechanism. In my study period and sample, there was no consistent evidence that home equity gains drove support for incumbent parties. Furthermore, economic anxieties about affordable housing did not consistently hurt incumbent parties either.

Sociotropic Voting. As summarized in Hypothesis 2, Chinese investments can boost the local economy, which may then increase support for the incumbent party in national elections through positive sociotropic perceptions. In other words, the strength of the local economy right before elections may mediate the effect of Chinese FREI exposure on presidential votes. Depending on the magnitude of this positive mediation effect, it may attenuate or even offset effects driven by other mechanisms (e.g., nativism). Thus, if the effect of Chinese FREI exposure worked through sociotropic perceptions and voting, we should expect stronger local economies to attenuate the negative effect for the Democratic Party in 2016 when it was the incumbent but amplify the effect in 2020 when it was the opposition party.

To test this observable implication, I measure the strength of the local economy in 2016 and 2020 using data on county-level GDP growth rates from the Bureau of Economic Analysis and employment rates from the Census Bureau. Since the variables of interest here are post-treatment, I then conduct a causal mediation analysis based on sequential g-estimation, which helps avoid biases due to simply conditioning on post-treatment variables (Acharya, Blackwell, and Sen 2016). The method helps assess support for the sociotropic mechanism by decomposing the total effect of Chinese FREI exposure into an *indirect effect* through GDP growth or employment and a *controlled direct effect* that circumvents measures of local economic strength (see Appendix E for implemen-

[27] See Appendix Table D.5 for details.

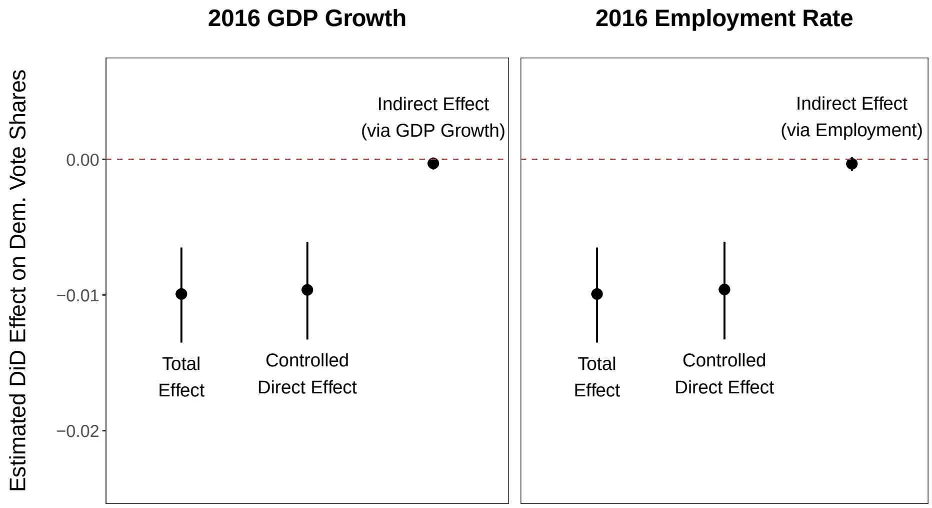

Figure 5: **Effect Decomposition: Post-shock Election-year Local Economic Condition as a Mediator.** The figure displays DiD point-estimates and 95% confidence intervals using 1,000 block-bootstraps at the county level. Focusing on the period between 2012 and 2016, the figure shows the decomposition of the negative total effect of Chinese FREI exposure on Democratic vote shares when local GDP growth and employment rates in 2016 are set at their mean. The estimated effects of Chinese FREI exposure through both mediators are small and indiscernible from zero.

tation details).

Overall, I do not find evidence that Chinese FREI exposure affected county-level presidential votes through the strength of the local economy. Focusing on the 2016 election, the left panel in Figure 5 shows that the estimated indirect effect of Chinese FREI exposure through 2016 local GDP growth is close to zero and imprecisely estimated (−0.0003, block-bootstrapped 95% C.I. = −0.0008 to 0.0001). Similarly, the estimated indirect effect through the 2016 local employment rate is also negligible and imprecisely estimated (−0.0003, block-bootstrapped 95% C.I. = −0.0009 to 0.0002). Meanwhile, results for the 2020 election are substantively similar (see Appendix Table E.1 for details). These results suggest that the negative total effect of Chinese FREI exposure on Democratic vote shares is driven by an alternative mechanism that does not operate through the strength of the local economy. Such findings provide indirect support for an explanation based on nativist reactions and cultural backlash.

Taken altogether, while the electoral effect of Chinese FREI exposure may have worked through multiple mechanisms and directions, I find evidence more consistent with expectations derived from nativist concerns and a pro-right wing effect. In both the 2016 and 2020 presidential elections, a larger white population or a smaller college-educated population, proxies for stronger nativists' reactions, exacerbated the negative effect of Chinese FREI exposure on two-party Democratic vote shares. In contrast, equity gains, economic anxieties about housing, or stronger local economies stemming from Chinese FREI exposure did not systematically affect incumbent parties during the study period.

7 Conclusion

Housing is one of the most significant economic issues in people's lives. Yet, despite the increasing globalization of housing through FREI, political economists still know relatively little about its political consequences. This study is a first step towards filling the gap in the literature. Focusing on the rise of Chinese FREI in the United States, I developed and tested hypotheses on how sudden and massive influxes of such investments can lead to changes in local voting in U.S. presidential elections. Using a unique exogenous shock and fine-grained measures of Chinese FREI exposure, I found that exposure to Chinese FREI reduced county-level presidential vote for the Democratic Party in both the 2016 and 2020 elections compared to 2012. Exploring empirical support for the mechanisms underlying this electoral effect, I found evidence more consistent with nativism and pro-conservative/right-wing effects but not pocketbook or sociotropic voting.

These findings are distinct in three main ways. First, by examining the electoral consequences of Chinese FREI exposure, this study extends Liao, Malhotra, and Newman (2020)'s seminal work on Chinese FREI's attitudinal effects. However, a key difference in findings is that while they show that

Chinese FREI exposure can reduce negative attitudes towards foreigners by strengthening the local economy, the results in this study imply that Chinese FREI triggered negative nativist reactions that drove local voting patterns in recent presidential elections. What explains this difference? One potential explanation lies in the difference in study periods. Liao, Malhotra, and Newman (2020) focus on the housing-market recovery period between 2010 and 2014, and thus the influx of Chinese FREI was critical as an economic stimulus. Indeed, due to this reason, they argue that their findings should provide an upper-bound estimate of Chinese FREI's positive attitudinal effect. In contrast, by 2016, the U.S. housing market had fully recovered (Federal Reserve Economic Data 2021), likely reducing the positive attitudinal effect of Chinese FREI exposure. This change, combined with an outspoken anti-China and anti-immigration stance from the Republican Party candidate, Trump, in both the 2016 and 2020 presidential elections, may have contributed to this study finding results more consistent with heightened nativist reactions. Apart from the United States, a number of developed countries (e.g., Australia, Canada, and the United Kingdom) have also seen sudden and massive influxes in Chinese FREI (Phillips 2016; Liao, Malhotra, and Newman 2020) and mixed feelings among natives (Gordon 2016). More studies are needed to investigate the scope conditions under which the electoral effects found in this study hold.

Second, most studies in the trade literature have emphasized the importance of material self-interests (e.g., economic anxieties about the local labor market) in explaining the electoral effects of cross-border economic integration. Focusing on the globalization of housing markets, I do not find evidence that self-interests played a similar role in driving recent county-level voting patterns in the United States. This is despite the distributive implications of rising foreign home purchases being arguably easier to grasp for the average voter than free trade (cf. Rho and Tomz 2017). Instead, my findings support a growing view that cultural backlash can also play a critical role in explaining the political consequences of economic globalization (Norris and Inglehart 2019; Baccini and Weymouth 2021). However, empirically corroborating causal mechanisms is not an easy task (Green, Ha, and Bullock 2010). This is especially the case when studying FREI, given its various cross-cutting economic and cultural effects. While this study provides some evidence of the underlying mechanisms at the county level, the effects could be cross-cutting even at the individual level. For example, the voting effects of Chinese FREI exposure could be uncertain for white homeowners experiencing changing neighborhoods and dominant group status but also gaining home equity. Thus, more research is needed to unpack the microfoundations through which housing market globalization affects individual preferences and voting behavior (e.g., turnout, party support).

Lastly, an emerging trade literature finds that local exposure to Chinese imports helped conservative parties and candidates in recent U.S. presidential elections (e.g., Cerrato, Ferrara, and Ruggieri 2018; Autor, Dorn, Hanson, and Majlesi 2020). This study shows that a "China shock" in the local housing market, as opposed to the local labor market, can generate similar effects, even after accounting for local trade exposure. This finding suggests that the electoral effects of increased economic integration with China may have worked independently through both labor and housing markets. More research on how cross-border flows of capital and people—two fundamental pillars of economic globalization—work together in shaping politics should provide new perspectives on the return of protectionism and populism in Western democracies (Zeitz and Leblang 2021; Broz, Frieden, and Weymouth 2021).

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Supplementary Information for

Chinese Foreign Real Estate Investment and Local Voting in U.S. Presidential Elections

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Appendix A Data and Measures

A.1 Patterns

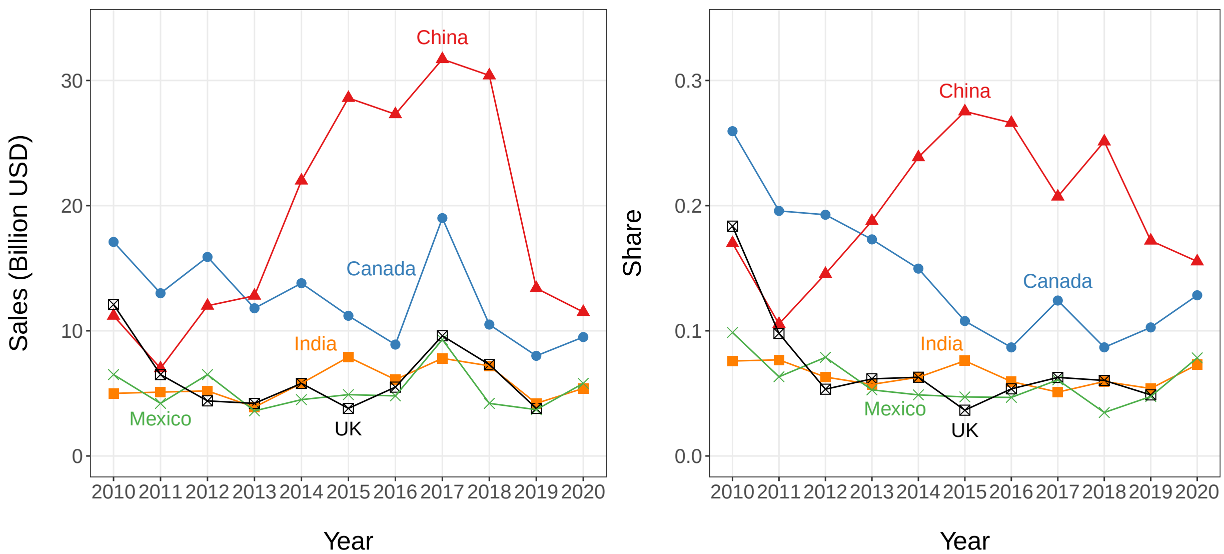

Figure A.1: **Top Five Foreign Buyers of U.S. Residential Real Estate, 2010–2020**. This figure shows the significant growth of Chinese FREI by total dollar volume (left) and share of foreign acquisitions (right). The estimates for year t draw on NAR (2021) data for the 12 months between April $t-1$ and March t.

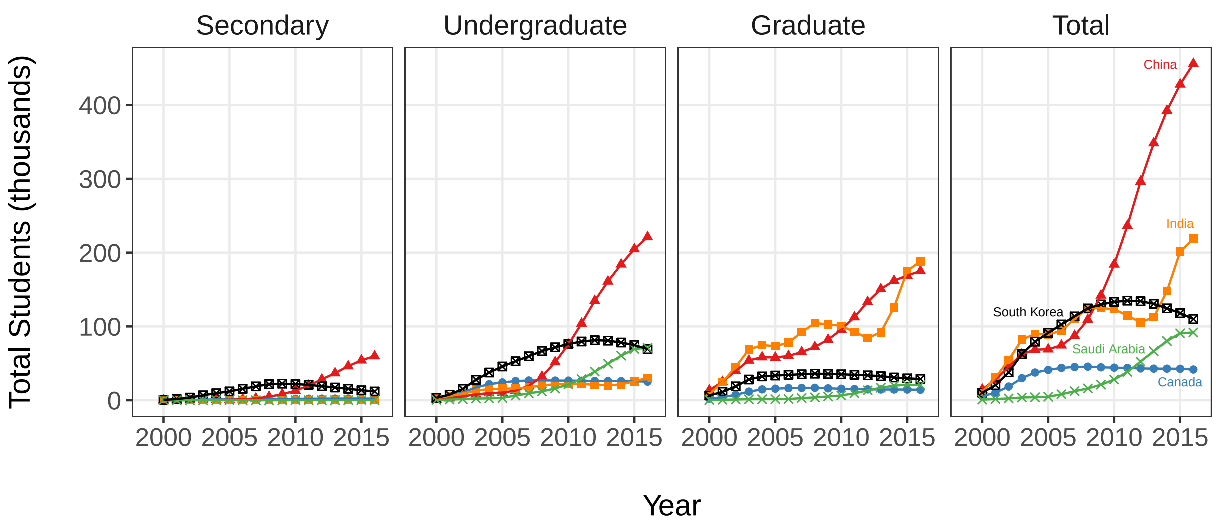

Figure A.2: **Top Five Origin-Countries of International Students in the United States between 2000 and 2016**. Student numbers include all F-1 and M-1 visa holders during the period. Data source: author's FOIA request.

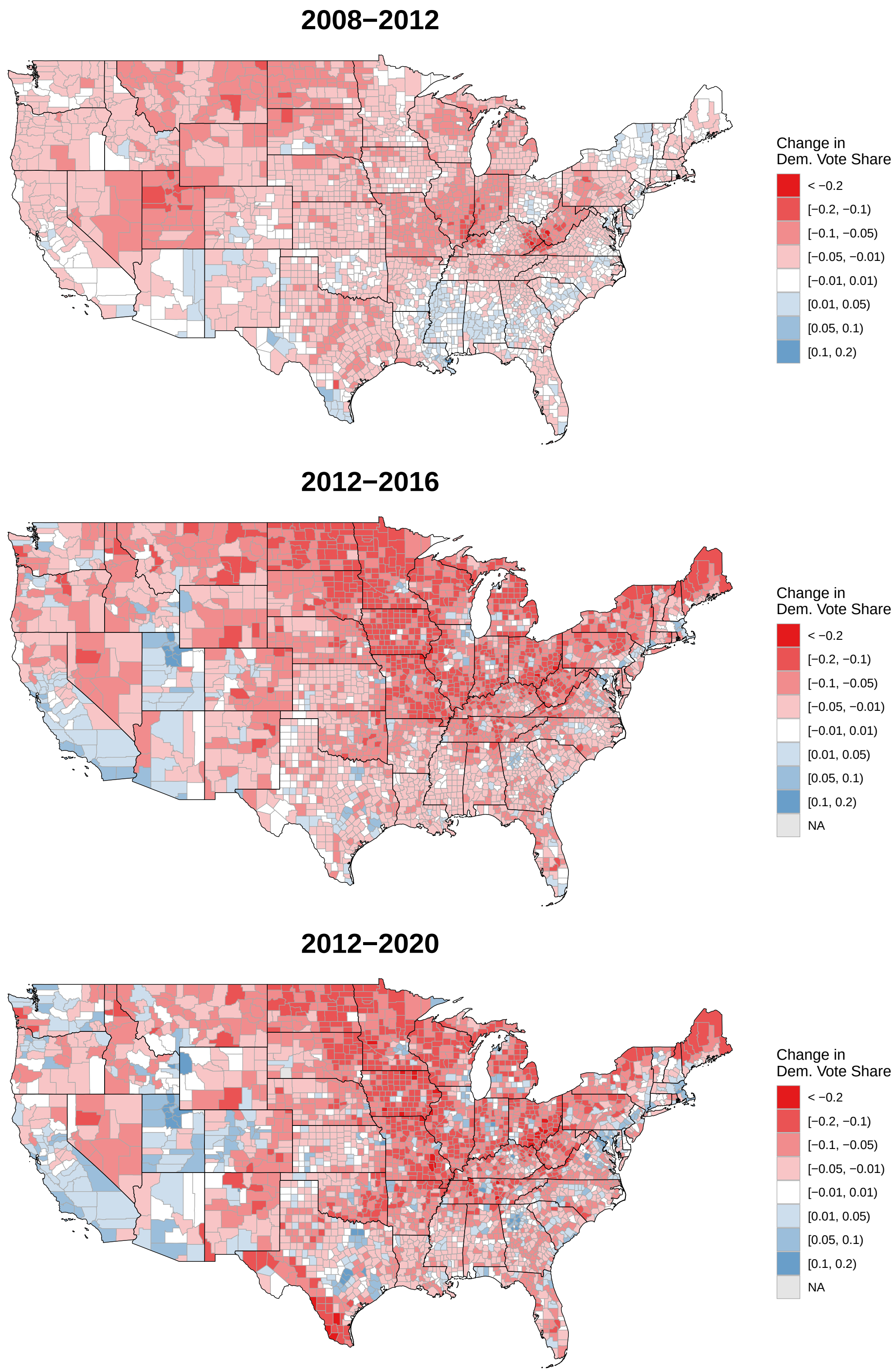

Figure A.3: **Changes in the Democratic Party Vote Shares in Presidential Elections.** The upper panel shows changes in the pre-treatment period (2008–2012) while the middle and lower panels illustrate changes before and after the 2013 shock (2012–2016 and 2012–2020). Blue (red) represents counties where the two-party vote share of Democrats (Republicans) increased. White indicates counties where vote shares remained roughly the same. Grey indicates missing data (for two counties that changed FIPS codes during the period). Data source: Leip (2021).

A.2 Measuring Local International Student Populations

Using the FOIA data, I constructed measures of total international students S from origin-country o at academic level k living in ZIP-code i in year t.

To construct the measures, I assume that students prefer to live near their institution located in ZIP code j. This implies that student numbers decrease dramatically as the distance between ZIP code i and j increases. I assume that the decrease follows an inverse distance-squared relationship. Additionally, I assume that all students live within fifty miles of their institution. I argue that this assumption is reasonable because it covers students roughly within a one-hour commute of the institution. Based on these assumptions, I aggregate the number of international students from the same origin in nearby institutions with inverse-distances as weights. That is, I formally define my measure as,

$$S_{it}^{ok} = \frac{\sum_{j=1}^{N} w_{ij} s_j^{ok}}{\sum_{j=1}^{N} w_{ij}} \tag{3}$$

$$w_{ij} = \begin{cases} 1 & \text{if } d(z_i, z_j) = 0 \\ \frac{1}{d(z_i,z_j)^2} & \text{if } d(z_i, z_j) \neq 0 \text{ and } d(z_i, z_j) \leq 50 \\ 0 & \text{if } d(z_i, z_j) > 50 \end{cases} \tag{4}$$

where s_j^{ok} denotes the number of international students from country o (e.g., China or India) at level k (e.g., undergraduate or graduate) enrolled in institutions situated in ZIP code j, N represents the number of ZIP codes within 50 miles of ZIP code i, $d(z_i, z_j)$ indicates the distance between ZIP code i and j, and w_{ij} denotes weights.

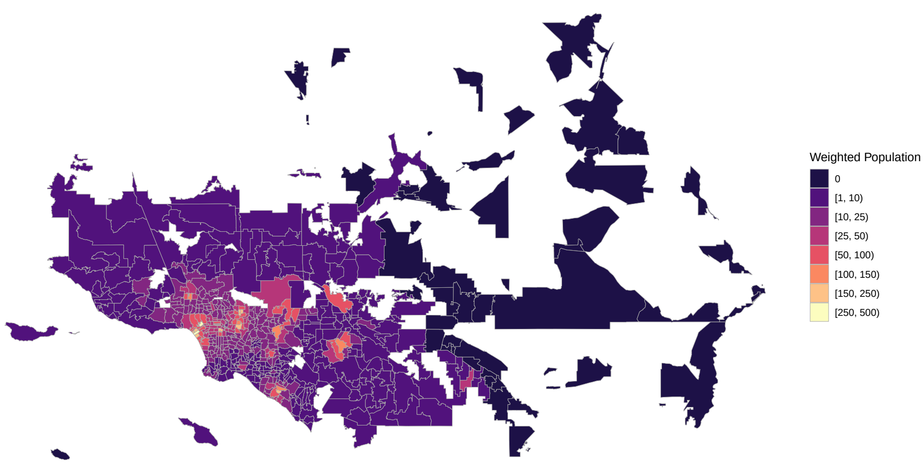

Figure A.4: **Chinese FREI Exposure in the Greater Los Angeles Area by ZIP Codes.** The level of exposure is measured using the 2012-population of Chinese international undergraduate students. Lighter colors represent areas with a larger population and higher exposure.

Figure A.4 uses Chinese international undergraduate student estimates to illustrate ZIP-code level variation in Chinese FREI exposure in the greater Los Angeles area. As designed, the figure shows higher exposure in ZIP codes near large universities where Chinese international undergraduate students concentrate. For example, ZIP codes near the University of California–Los Angeles, California Institute of Technology, the University of Southern California, and the University of California–Irvine score the highest on the measure, hosting more than two hundred of these students. While it is difficult to verify the exact ZIP code where Chinese international undergraduate students live and where their parents invest, these locations are fairly consistent with news reports on many Chinese home acquisitions in areas such as Irvine and Arcadia during the study period (Reckard and Khouri 2014).

As discussed in the main text, more fine-grained measures of Chinese FREI destinations are not systematically available. To gauge FREI patterns in the United States, existing research has relied on surveys. For example, NAR conducts annual surveys (*Profile of International Activity in U.S. Residential Real Estate*) regarding the transactions between its real estate agents and their international clients. The reports provide top destination states for Chinese FREI and their shares in a given year. While common issues with low response rates apply, these surveys provide a broad picture of major FREI origin-countries and destination-states in the United States. Using the data, Figure A.5 shows that states with a greater share of Chinese international undergraduate students before the anti-corruption campaign tend to attract a more significant share of Chinese FREI after the campaign. Furthermore, this relationship is most pronounced immediately after the shock and gradually stabilizes over time. The positive relationship here, albeit at a more aggregated level, provides additional descriptive evidence that affluent international students channel FREI from the same origin.

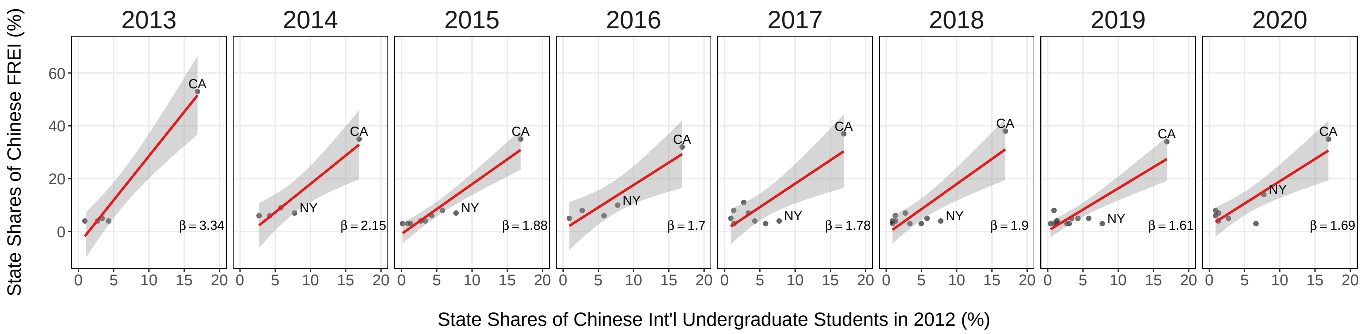

Figure A.5: **State-Level Correlations Between Chinese International Undergraduate Students and FREI**. The red lines represent the estimated slopes from bivariate linear regression models, while the grey areas indicate 95% confidence intervals. Measures of 2012-state shares of students rely on the author's FOIA data.

Appendix B Validation of the Research Design

As a validation of the research design, I follow Liao, Malhotra, and Newman (2020) and investigate the effect of Chinese FREI exposure on U.S. home prices at the ZIP-code level. Here, I replicate and extend their analysis to 2020 and include more control covariates. As Liao, Malhotra, and Newman (2020, 486) discuss, "If the research design is valid, ZIP codes with a larger population of affluent Chinese international undergraduate students right before the anti-corruption campaign...should receive more Chinese FREI after the shock than ZIP codes with a smaller population." A sudden and massive influx of Chinese FREI should then "lead to greater local demand for housing and thus larger increases in home prices in treated ZIP codes." On the contrary, a more significant price increase in ZIP codes with a larger initial population of placebo international students unconnected to the shock should be less likely. Furthermore, the positive effect of exposure to Chinese FREI on home prices should be less pronounced prior to the anti-corruption campaign shock. Overall, the identification strategy is similar to Card (1992)—the treatment effect varies depending on the "bite" of the treatment in different geographical units.

Since Zillow has discontinued its index on median home value per square feet ($) used in Liao, Malhotra, and Newman (2020), I instead rely on the newly revised Zillow Home Value Index (ZHVI). The index measures the typical home value across a given region and housing type. Zillow provides several index versions, and I use the most-cited version, a smoothed and seasonally adjusted measure for all homes with home values at the middle tier (35–65%). For more details about the index's methodology and the 2019 revision, see https://www.zillow.com/research/zhvi-methodology-2019-deep-26226/. Zillow's index improves upon existing median sale price or repeat-sales indices that are easy to construct and interpret but fail to adjust for the quality and composition of properties on the market in different time periods (Ghysels et al. 2013, 520–525). For example, median sale price indices provided by the Census Bureau and the NAR would characterize a market as experiencing price appreciation when expensive homes sell at a disproportionately higher rate than less expensive homes, even when the true value of homes is unchanged. Repeat-sales prices from the Case-Shiller index and the Federal Housing Finance Agency (FHFA) can also be biased when limited repeat sales data exists in small geographical units. Zillow's index mitigates these biases. In the analyses below, I use annual estimates based on the average of Zillow's monthly estimates and focus on states in the contiguous United States and the District of Columbia, which according to the 2010 U.S. census, had 32,657 possible ZIP codes (ZCTAs). Within this sample, Zillow provides data for 30,086 ZCTAs. Note that most missing ZIP codes are unpopulated areas.

To conduct the validity checks, I fit a DiD model that is equivalent to equation (1) in Liao, Malhotra, and Newman (2020, 486):

$$V_{it} \;=\; \alpha_i + P_t + \tau_p(P_t D_{i,2012}) + \boldsymbol{\gamma}(P_t \boldsymbol{S}_{i,2012}) + \delta \boldsymbol{U}_{it} + \kappa P_t \boldsymbol{U}_{it} + \epsilon_{it}, \tag{5}$$

where the outcome variable V_{it} is the typical home value (ZHVI) in ZIP code i and year t (logged). The treatment-condition variable $D_{i,2012}$ is a dichotomous version of the exposure measure (per square mile 2012-population of Chinese international undergraduate students) with above-median values equal 1 (High) and 0 otherwise (Low). Again, this guards against excessive model dependency and the potential lack of common support in the data. The treatment-period variable P_t is a dichotomous variable assigned the value of 1 for post-treatment years (2013 and after) and 0 otherwise. The variable α_i indicates ZIP-code fixed-effects, accounting for *time-invariant* characteristics of ZIP codes that may correlate with Chinese international undergraduate students' existence. The variables $\boldsymbol{S}_{i,2012}$ represent measures of placebo international students explained in the main text and are also discretized. The variables $\boldsymbol{U}_{it}$ are time-*varying* covariates that account for potential imbalances between treated and control units. These include ZIP-code level measures of all control variables discussed in footnote (21), except for trade exposure measured at the county level. The interaction term $P_t\boldsymbol{U}_{it}$ accounts for potential trending effects stemming from these control covariates. Standard errors are clustered by ZIP codes to allow for within-unit correlations of errors. τ_p is the coefficient of interest and represents the DiD estimate for the effect of Chinese FREI exposure on home values. The left panel of Figure B.1 demonstrates that the parallel trends assumption of the DiD design is satisfied even without the various control variables.

I fit several versions of equation (5) to the data. The baseline model only includes the interaction

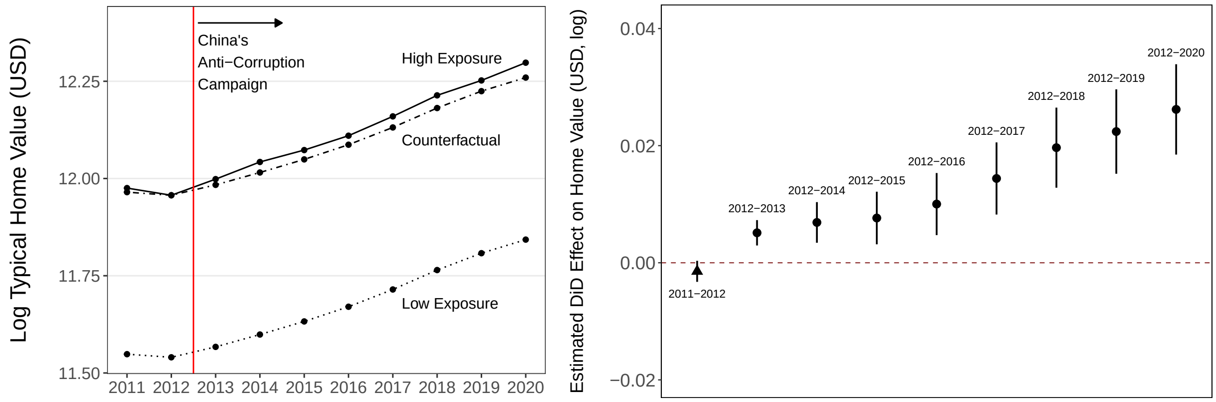

Figure B.1: **Parallel Trends and DiD Estimates**. The left panel shows that pre-treatment trends were similar between the control group (Low Exposure) and the treatment group (High Exposure). After the shock, ZIP codes more exposed to Chinese FREI increased more in their typical home value (ZHVI). The vertical red line indicates the onset of China's anti-corruption campaign in December 2012 that triggered a massive flow of Chinese FREI to the United States. The right panel presents DiD point-estimates and 95% confidence intervals on the effect of Chinese FREI exposure on U.S. home values. It shows that ZIP codes more exposed to Chinese FREI right after the start of China's anti-corruption campaign saw a larger increase in typical home values (ZHVI) between 2012 and 2013. Furthermore, the positive effect grew steadily since the 2013 anti-corruption campaign but was small and imprecisely estimated between 2011 and 2012, prior to the shock.

term $P_t D_{i,2012}$, the treatment-period dummy variable P_t, and ZIP-code fixed effects. The full model adds interaction terms for placebo international students, time-varying controls, and interaction terms to capture the trending effects of the time-varying controls. Lastly, I fit equation (5) to different periods before and after the shock to examine temporal variation in the effect.

Consistent with Liao, Malhotra, and Newman (2020), I find that exposure to Chinese FREI after the shock increased typical home values at the ZIP-code level, even after accounting for the array of time-invariant and time-varying characteristics of ZIP-codes. As shown in the right panel of Figure B.1, high exposure ZIP codes saw an approximately 0.5% larger increase in their typical home values in 2013 right after the shock [$100 \times (\exp^{0.005} - 1) \approx 0.5$]. Furthermore, this positive effect increased as Chinese capital flight accelerated—growing to 2.65% by 2020, a more than five-fold growth compared to its effect size in 2013. Additionally, the effect was absent prior to China's anti-corruption campaign (2011–2012). The point estimate was close to zero and imprecisely estimated. The results from this time placebo test ensure the validity of the parallel trend assumption. Overall, these results suggest that the positive effect stems from the anti-corruption campaign *per se* and not simply from the presence of Chinese international undergraduate students or general trends in locations housing these students.

Finally, I also examine the effect of the two placebo measures of Chinese FREI exposure discussed in the main text. Results show that the point estimates associated with Indian international undergraduate students are generally small and imprecisely estimated, especially in later years. Meanwhile, the point estimates associated with Chinese international graduate students tend to be precisely estimated but small and oppositely signed. These results contrast the positive and precisely estimated point estimates for Chinese international undergraduate students. Given that the positive home-value effect is limited to Chinese international undergraduate students intrinsically tied to Chinese investments, the results suggest that the effect is because of Chinese FREI and not driven by unexplained local factors or general Chinese international student characteristics. Due to the journal's page limits on appendices, these results are made available upon request.

In sum, these findings should increase confidence in the validity of the study's research design. They also support the claim that Chinese FREI increased local home prices in the United States through their affluent international students. The findings are consistent with empirical studies that find a positive effect of immigrants on local housing prices (e.g., Saiz 2007).

Appendix C Descriptive Statistics

Table C.1: **Descriptive Statistics for County-Level Analysis, 2012 vs. 2016**

Variable	N	Mean	Median	St. Dev.	Min	Max
Two-Party Democratic Vote Share	6,214	0.36	0.34	0.16	0.03	0.96
2013 Anti-corruption Campaign (Dummy)	6,214	0.50	0.5	0.50	0	1
2012 # CHN Int'l Undergraduate Students per Sq. Mi.	6,214	0.14	0.003	1.79	0.00	61.04
2012 # CHN Int'l Graduate Students per Sq. Mi.	6,214	0.20	0.002	3.05	0.00	143.73
2012 # IND Int'l Undergraduate Students per Sq. Mi.	6,214	0.03	0.0005	0.43	0.00	21.87
Share of Female Pop.	6,214	0.50	0.50	0.02	0.22	0.58
Share of Pop. 5–17	6,214	0.17	0.17	0.02	0.00	0.28
Share of Pop. 18–24	6,214	0.09	0.08	0.04	0.01	0.56
Share of Pop. 25–34	6,214	0.12	0.11	0.02	0.00	0.34
Share of Pop. 35–44	6,214	0.12	0.12	0.02	0.02	0.23
Share of Pop. 45–54	6,214	0.14	0.14	0.02	0.03	0.28
Share of Pop. 55–64	6,214	0.14	0.13	0.02	0.03	0.45
Share of Pop. 65–	6,214	0.17	0.16	0.04	0.04	0.53
Share of White Pop.	6,214	0.84	0.90	0.16	0.09	1.00
Share of Black Pop.	6,214	0.09	0.02	0.15	0.00	0.86
Share of Hispanic Pop.	6,214	0.09	0.04	0.13	0.00	0.99
Share of Asian Pop.	6,214	0.01	0.01	0.02	0.00	0.34
Share of CN Pop.	6,214	0.002	0.001	0.01	0.00	0.21
Share of Foreign-Born Pop.	6,214	0.05	0.03	0.06	0.00	0.52
Share of Pop. with BA Degree or Above	6,214	0.20	0.18	0.09	0.03	0.80
Share of Pop. Enrolled in College or Above	6,214	0.05	0.04	0.04	0.00	0.56
Population Density (log)	6,214	3.81	3.82	1.73	−2.17	11.18
Effective Tax Rate (%)	6,212	1.04	0.93	0.50	0.15	3.98
Trade Exposure (IPW)	6,213	0.38	0.19	0.83	0.00	25.55
Employment Rate	6,214	0.55	0.55	0.08	0.14	0.79
Median Household Income ($10,000)	6,214	4.66	4.49	1.22	1.90	12.57
Share of Vacant Houses	6,214	0.18	0.15	0.11	0.02	0.86

Table C.2: **Descriptive Statistics for County-Level Analysis, 2012 vs. 2020**

Variable	N	Mean	Median	St. Dev.	Min	Max
Two-Party Democratic Vote Share	6,214	0.36	0.34	0.16	0.03	0.94
2013 Anti-corruption Campaign (Dummy)	6,214	0.50	0.5	0.50	0	1
2012 # CHN Int'l Undergraduate Students per Sq. Mi.	6,214	0.14	0.003	1.79	0.00	61.04
2012 # CHN Int'l Graduate Students per Sq. Mi.	6,214	0.20	0.002	3.05	0.00	143.73
2012 # IND Int'l Undergraduate Students per Sq. Mi.	6,214	0.03	0.0005	0.43	0.00	21.87
Share of Female Pop.	6,214	0.50	0.50	0.02	0.24	0.58
Share of Pop. 5–17	6,214	0.17	0.17	0.03	0.00	0.29
Share of Pop. 18–24	6,214	0.09	0.08	0.04	0.01	0.52
Share of Pop. 25–34	6,214	0.12	0.11	0.02	0.03	0.34
Share of Pop. 35–44	6,214	0.12	0.12	0.02	0.02	0.23
Share of Pop. 45–54	6,214	0.14	0.14	0.02	0.00	0.28
Share of Pop. 55–64	6,214	0.14	0.14	0.02	0.03	0.30
Share of Pop. 65–	6,214	0.17	0.17	0.05	0.03	0.57
Share of White Pop.	6,214	0.84	0.90	0.16	0.08	1.00
Share of Black Pop.	6,214	0.09	0.02	0.15	0.00	0.87
Share of Hispanic Pop.	6,214	0.09	0.04	0.14	0.00	0.99
Share of Asian Pop.	6,214	0.01	0.01	0.02	0.00	0.36
Share of CN Pop.	6,214	0.002	0.001	0.01	0.00	0.21
Share of Foreign-Born Pop.	6,214	0.05	0.03	0.06	0.00	0.54
Share of Pop. with BA Degree or Above	6,214	0.21	0.18	0.09	0.00	0.78
Share of Pop. Enrolled in College or Above	6,214	0.05	0.04	0.04	0.00	0.52
Population Density (log)	6,214	3.81	3.81	1.73	−2.06	11.18
Effective Tax Rate (%)	6,211	1.04	0.92	0.50	0.14	3.65
Trade Exposure (IPW)	6,186	0.28	0.10	0.77	0.00	25.55
Employment Rate	6,214	0.55	0.56	0.08	0.15	0.79
Median Household Income ($10,000)	6,214	4.94	4.74	1.36	1.96	14.23
Share of Vacant Houses	6,214	0.18	0.15	0.11	0.02	0.87

Table C.3: **Descriptive Statistics for County-Level Analysis, 2008 vs. 2012**

Variable	N	Mean	Median	St. Dev.	Min	Max
Two-Party Democratic Vote Share	6,214	0.41	0.40	0.15	0.03	0.93
2013 Anti-corruption Campaign (Dummy)	6,214	0.00	0	0.00	0	0
2012 # CHN Int'l Undergraduate Students per Sq. Mi.	6,214	0.14	0.003	1.79	0.00	61.04
2012 # CHN Int'l Graduate Students per Sq. Mi.	6,214	0.20	0.002	3.05	0.00	143.73
2012 # IND Int'l Undergraduate Students per Sq. Mi.	6,214	0.03	0.0005	0.43	0.00	21.87
Share of Female Pop.	4,920	0.50	0.51	0.02	0.24	0.58
Share of Pop. 5–17	4,920	0.17	0.17	0.02	0.00	0.28
Share of Pop. 18–24	4,920	0.09	0.09	0.04	0.01	0.52
Share of Pop. 25–34	4,920	0.12	0.12	0.02	0.04	0.34
Share of Pop. 35–44	4,920	0.13	0.13	0.02	0.02	0.23
Share of Pop. 45–54	4,920	0.15	0.15	0.02	0.04	0.28
Share of Pop. 55–64	4,920	0.13	0.12	0.02	0.03	0.29
Share of Pop. 65–	4,920	0.15	0.15	0.04	0.04	0.45
Share of White Pop.	4,920	0.84	0.90	0.16	0.10	1.00
Share of Black Pop.	4,920	0.09	0.03	0.14	0.00	0.86
Share of Hispanic Pop.	4,920	0.08	0.03	0.13	0.00	0.98
Share of Asian Pop.	4,920	0.01	0.01	0.02	0.00	0.33
Share of CN Pop.	4,920	0.002	0.001	0.01	0.00	0.21
Share of Foreign-Born Pop.	4,920	0.05	0.03	0.06	0.00	0.51
Share of Pop. with BA Degree or Above	4,920	0.20	0.18	0.09	0.04	0.73
Share of Pop. Enrolled in College or Above	4,920	0.06	0.05	0.04	0.00	0.52
Population Density (log)	4,920	4.15	4.11	1.64	−2.06	11.17
Effective Tax Rate (%)	4,920	0.99	0.86	0.50	0.11	3.34
Trade Exposure (IPW)	6,212	0.36	0.18	0.83	0.00	25.55
Employment Rate	4,897	0.56	0.57	0.08	0.21	0.79
Median Household Income ($10,000)	4,920	4.60	4.41	1.20	1.89	12.28
Share of Vacant Houses	4,920	0.16	0.14	0.10	0.02	0.79

Appendix D Difference-in-Differences Regression

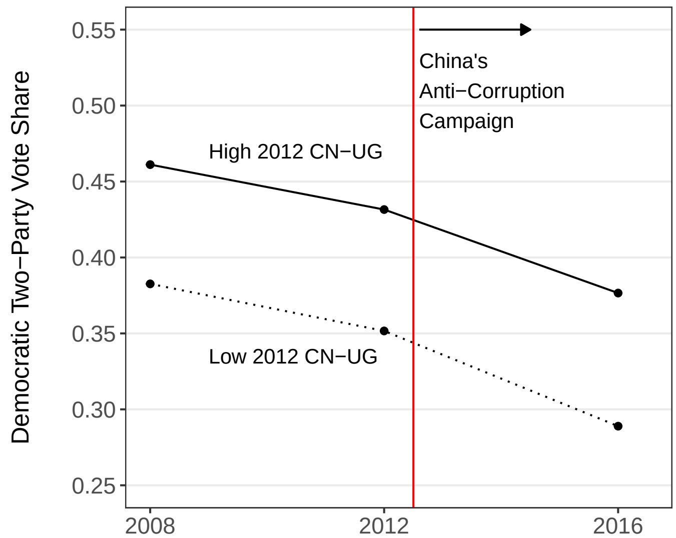

Figure D.1: **Pre-treatment Parallel Trends**. This figure shows that pre-treatment trends were quite similar between the treatment and control groups, increasing confidence in the parallel trends assumption. The pre-treatment trends are consistent with the small and imprecise point estimates from the time placebo DID regression in column (1) of Appendix Table D.2. Note that plotting the raw data without accounting for the effects of similar international student groups or pre-existing county-level characteristics shows a small positive correlation between the treatment and Democratic vote shares in 2016 compared to 2012 (i.e., the slope is slightly less steep). As discussed in Section 5, such baseline results can be biased and misleading. Once county-level confounders are controlled for in the DiD regressions, the results consistently show that counties with higher exposure to Chinese FREI *decreased* more in Democratic two-party vote share between 2012 and 2016.

Table D.1: **DiD Regression Results: County-Level Presidential Vote**

	Dependent Variable: Two-Party Democratic Vote Share							
	2012 vs. 2016					2012 vs. 2020		
	Base (1)	Ext. (2)	Full (3)	Full-Tri. (4)	Full-Con. (5)	Full (6)	Full-Tri. (7)	Full-Con. (8)
2013 Anti-corruption Campaign (Dummy)	-0.063^{***}	-0.067^{***}	-0.296^{*}	-0.328^{**}	-0.306^{*}	-0.456^{*}	-0.481^{**}	-0.501^{**}
	(0.001)	(0.001)	(0.126)	(0.124)	(0.128)	(0.186)	(0.184)	(0.186)
2013 Anti-corruption × CN UG in 2012 (Dummy, High)	0.008^{***}	-0.014^{***}	-0.010^{***}			-0.012^{***}		
	(0.002)	(0.003)	(0.002)			(0.002)		
2013 Anti-corruption × CN UG in 2012 (Tri., Medium)				-0.006^{**}			-0.008^{**}	
				(0.002)			(0.003)	
2013 Anti-corruption × CN UG in 2012 (Tri., High)				-0.016^{***}			-0.020^{***}	
				(0.003)			(0.004)	
2013 Anti-corruption × CN UG in 2012 (Continuous)					-0.002			-0.017^{***}
					(0.004)			(0.005)
2013 Anti-corruption × CN G in 2012 (Dummy, High)		0.011^{***}	0.005^{*}		-0.0002	0.004		-0.002
		(0.003)	(0.002)		(0.002)	(0.002)		(0.002)
2013 Anti-corruption × CN G in 2012 (Tri., Medium)				0.004^{+}			0.001	
				(0.002)			(0.003)	
2013 Anti-corruption × CN G in 2012 (Tri., High)				0.006^{*}			0.005	
				(0.003)			(0.004)	
2013 Anti-corruption × IND UG in 2012 (Dummy, High)		0.019^{***}	0.001		-0.001	0.003		-0.0003
		(0.003)	(0.002)		(0.002)	(0.002)		(0.002)
2013 Anti-corruption × IND UG in 2012 (Tri., Medium)				-0.006^{**}			-0.007^{*}	
				(0.002)			(0.003)	
2013 Anti-corruption × IND UG in 2012 (Tri., High)				0.003			0.007^{*}	
				(0.003)			(0.004)	
Fixed Effects: County	✓	✓	✓	✓	✓	✓	✓	✓
Number of Counties	3,107	3,107	3,107	3,107	3,107	3,106	3,106	3,106
Observations	6,214	6,214	6,211	6,211	6,211	6,186	6,186	6,186
R^2	0.973	0.974	0.991	0.991	0.991	0.985	0.985	0.985
Adjusted R^2	0.946	0.947	0.981	0.981	0.981	0.969	0.970	0.969
Residual Std. Error	0.037	0.036	0.022	0.022	0.022	0.028	0.027	0.028

Note: County fixed-effects subsume time-invariant county-level measures of international students in 2012. Column (3)–(9) results for control covariates (see footnote (21)) are omitted due to the journal's page limits on appendices. Full tables will be available in the replication materials. Robust standard errors clustered by counties in parentheses. $^{+}p<0.1$; $^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$

Table D.2: **Placebo DiD Regression Results: County-Level Presidential Vote**

	Dependent Variable: Two-Party Democratic Vote Share, 2008 vs. 2012		
	Base (1)	Full (2)	Full-tri (3)
Placebo Anti-corruption (2012)	-0.031^{***}	-0.637^{***}	-0.583^{**}
	(0.001)	(0.187)	(0.186)
Placebo Anti-corruption (2012) × CN UG in 2012 (Dummy, High)	0.001	0.001	
	(0.001)	(0.002)	
Placebo Anti-corruption (2012) × CN UG in 2012 (Tri., Medium)			-0.005^{*}
			(0.002)
Placebo Anti-corruption (2012) × CN UG in 2012 (Tri., High)			-0.0005
			(0.003)
Placebo Anti-corruption (2012) × CN G in 2012 (Dummy, High)		0.001	
		(0.002)	
Placebo Anti-corruption (2012) × CN G in 2012 (Tri., Medium)			0.005^{*}
			(0.002)
Placebo Anti-corruption (2012) × CN G in 2012 (Tri., High)			0.005^{+}
			(0.003)
Placebo Anti-corruption (2012) × IND UG in 2012 (Dummy, High)		-0.003	
		(0.002)	
Placebo Anti-corruption (2012) × IND UG in 2012 (Tri., Medium)			0.0002
			(0.002)
Placebo Anti-corruption (2012) × IND UG in 2012 (Tri., High)			-0.002
			(0.002)
Fixed Effects: County	✓	✓	✓
Number of Counties	3,107	3,106	3,106
Observations	6,214	4,896	4,896
R^2	0.989	0.995	0.996
Adjusted R^2	0.977	0.987	0.987
Residual Std. Error	0.022	0.016	0.016

Note: County fixed-effects subsume time-invariant county-level measures of international students in 2012. The smaller sample size in the full model is mainly due to demographic data missingness for some counties in 2008, which was before the more comprehensive American Community Survey (ACS) 5-year data was available (2009–). Columns (2)–(3) results for control covariates (see footnote (21)) are omitted due to the journal's page limits on appendices. Full tables will be available in the replication materials. Robust standard errors clustered by counties in parentheses. $^{+}p<0.1$; $^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$

Table D.3: **DiD Regression Results: Excluding Outlier Counties**

	Dependent Variable: Two-Party Democratic Vote Share			
	2012 vs. 2016		2012 vs. 2020	
	Binary	Tri.	Binary	Tri.
	(1)	(2)	(3)	(4)
2013 Anti-corruption Campaign (Dummy)	-0.449^{***} (0.124)	-0.484^{***} (0.123)	-0.721^{***} (0.187)	-0.752^{***} (0.184)
2013 Anti-corruption × CN UG in 2012 (Dummy, High)	-0.010^{***} (0.002)		-0.012^{***} (0.002)	
2013 Anti-corruption × CN UG in 2012 (Tri., Medium)		-0.005^{*} (0.002)		-0.008^{**} (0.003)
2013 Anti-corruption × CN UG in 2012 (Tri., High)		-0.015^{***} (0.003)		-0.020^{***} (0.004)
2013 Anti-corruption × CN G in 2012 (Dummy, High)	0.004^{*} (0.002)		0.003 (0.002)	
2013 Anti-corruption × CN G in 2012 (Tri., Medium)		0.003 (0.002)		0.001 (0.003)
2013 Anti-corruption × CN G in 2012 (Tri., High)		0.005 (0.003)		0.003 (0.004)
2013 Anti-corruption × IND UG in 2012 (Dummy, High)	0.001 (0.002)		0.002 (0.002)	
2013 Anti-corruption × IND UG in 2012 (Tri., Medium)		-0.006^{**} (0.002)		-0.006^{*} (0.003)
2013 Anti-corruption × IND UG in 2012 (Tri., High)		0.003 (0.003)		0.006^{+} (0.004)
Fixed Effects: County	✓	✓	✓	✓
Number of Counties	2,620	2,620	2,619	2,619
Observations	5,237	5,237	5,212	5,212
R^2	0.989	0.989	0.982	0.982
Adjusted R^2	0.977	0.978	0.963	0.964
Residual Std. Error	0.022	0.022	0.028	0.027

Note: County fixed-effects subsume time-invariant county-level measures of international students in 2012. Excluded outlier counties are identified using the interquartile range (IQR) criterion (Q3 + 1.5 × IQR) on the number of Chinese international undergraduate students per square mile in 2012. Results for control covariates (see footnote (21)) are omitted due to the journal's page limits on appendices. Full tables will be available in the replication materials. Robust standard errors clustered by counties in parentheses. $^{+}$p<0.1; *p<0.05; **p<0.01; ***p<0.001

Table D.4: **Heterogeneous Treatment Effects: Nativism Hypotheses**

	Dependent Variable: Two-Party Democratic Vote Share			
	Initial White Pop.		Initial College Educated Pop.	
	2012 vs. 2016 (1)	2012 vs. 2020 (2)	2012 vs. 2016 (3)	2012 vs. 2020 (4)
2013 Anti-corruption Campaign (Dummy)	-0.270^{*} (0.125)	-0.390^{*} (0.182)	-0.276^{*} (0.125)	-0.449^{*} (0.184)
2013 Anti-corruption × CN UG in 2012 (Dummy, High)	-0.005^{*} (0.002)	-0.005^{+} (0.003)	-0.019^{***} (0.003)	-0.022^{***} (0.003)
2013 Anti-corruption × 2012 White Pop. (Medium)	-0.002 (0.002)	-0.003 (0.003)		
2013 Anti-corruption × 2012 White Pop. (Large)	-0.009^{**} (0.003)	-0.015^{***} (0.004)		
2013 Anti-corruption × 2012 College Educated Pop. (Medium)			-0.003 (0.002)	0.007^{*} (0.003)
2013 Anti-corruption × 2012 College Educated Pop. (Large)			-0.001 (0.003)	0.014^{***} (0.004)
2013 Anti-corruption × CN UG in 2012 (Dummy, High) × 2012 White Population (Medium)	-0.002 (0.003)	-0.004 (0.003)		
2013 Anti-corruption × CN UG in 2012 (Dummy, High) × 2012 White Population (Large)	-0.009^{**} (0.003)	-0.012^{**} (0.004)		
2013 Anti-corruption × CN UG in 2012 (Dummy, High) × 2012 College Educated Pop. (Medium)			0.012^{***} (0.003)	0.011^{**} (0.004)
2013 Anti-corruption × CN UG in 2012 (Dummy, High) × 2012 College Educated Pop. (Large)			0.019^{***} (0.003)	0.019^{***} (0.004)
2013 Anti-corruption × CN G in 2012 (Dummy, High)	0.005^{**} (0.002)	0.004^{+} (0.002)	0.005^{**} (0.002)	0.005^{*} (0.002)
2013 Anti-corruption × IND UG in 2012 (Dummy, High)	0.001 (0.002)	0.002 (0.002)	0.002 (0.002)	0.002 (0.002)
Fixed Effects: County	✓	✓	✓	✓
Number of Counties	3,107	3,107	3,107	3,106
Observations	6,211	6,186	6,211	6,186
R^2	0.991	0.985	0.991	0.985
Adjusted R^2	0.981	0.970	0.981	0.970
Residual Std. Error	0.022	0.027	0.022	0.027

Note: County fixed-effects subsume county-level measures of the initial population of international students, white population, college-educated population, and two-way interactions between these time-invariant variables. The interaction term 2013 Anti-corruption × CN UG in 2012 (Dummy, High) represents the baseline effect of Chinese FREI when the initial white population or population with at least a college degree is small. Column (1)–(4) results for control covariates (see footnote (21)) are omitted due to the journal's page limits on appendices. Full tables will be available in the replication materials. Robust standard errors clustered by counties in parentheses. $^{+}$p<0.1; *p<0.05; **p<0.01; ***p<0.001

Table D.5: **Heterogeneous Treatment Effects: Pocketbook Voting Hypotheses**

	Dependent Variable: Two-Party Democratic Vote Share			
	Initial Homeownership Rate		Initial Vacancy Rate	
	2012 vs. 2016 (1)	2012 vs. 2020 (2)	2012 vs. 2016 (3)	2012 vs. 2020 (4)
2013 Anti-corruption Campaign (Dummy)	-0.294^{*} (0.125)	-0.499^{**} (0.184)	-0.306^{*} (0.125)	-0.480^{**} (0.186)
2013 Anti-corruption × CN UG in 2012 (Dummy, High)	-0.005^{+} (0.002)	-0.010^{**} (0.003)	0.0005 (0.003)	-0.004 (0.003)
2013 Anti-corruption × 2012 Homeownership Rate (Medium)	0.004^{*} (0.002)	-0.002 (0.003)		
2013 Anti-corruption × 2012 Homeownership Rate (High)	-0.002 (0.002)	-0.013^{***} (0.003)		
2013 Anti-corruption × 2012 Vacancy Rate (Medium)			0.006^{*} (0.003)	0.002 (0.003)
2013 Anti-corruption × 2012 Vacancy Rate (High)			0.006^{+} (0.003)	-0.004 (0.004)
2013 Anti-corruption × CN UG in 2012 (Dummy, High) × 2012 Homeownership Rate (Medium)	-0.010^{***} (0.003)	-0.007^{*} (0.004)		
2013 Anti-corruption × CN UG in 2012 (Dummy, High) × 2012 Homeownership Rate (High)	-0.006^{*} (0.003)	-0.0001 (0.004)		
2013 Anti-corruption × CN UG in 2012 (Dummy, High) × 2012 Vacancy Rate (Medium)			-0.010^{***} (0.003)	-0.008^{+} (0.004)
2013 Anti-corruption × CN UG in 2012 (Dummy, High) × 2012 Vacancy Rate (High)			-0.010^{**} (0.003)	-0.005 (0.004)
2013 Anti-corruption × CN G in 2012 (Dummy, High)	0.005^{**} (0.002)	0.004^{+} (0.002)		
2013 Anti-corruption × IND UG in 2012 (Dummy, High)	0.002 (0.002)	0.003 (0.002)		
Fixed Effects: County	✓	✓	✓	✓
Number of Counties	3,107	3,106	3,107	3,106
Observations	6,211	6,186	6,211	6,186
R^2	0.991	0.985	0.991	0.985
Adjusted R^2	0.981	0.970	0.981	0.969
Residual Std. Error	0.022	0.027	0.022	0.028

Note: County fixed-effects subsume county-level measures of the initial population of international students, the initial homeownership rate, the initial vacancy rate, and two-way interactions between these time-invariant variables. The interaction term 2013 Anti-corruption × CN UG in 2012 (Dummy, High) represents the baseline effect of Chinese FREI when the initial homeownership or vacancy rate is small. Column (1)–(4) results for control covariates (see footnote (21)) are omitted due to the journal's page limits on appendices. Full tables will be available in the replication materials. Robust standard errors clustered by counties in parentheses. $^{+}$p<0.1; *p<0.05; **p<0.01; ***p<0.001

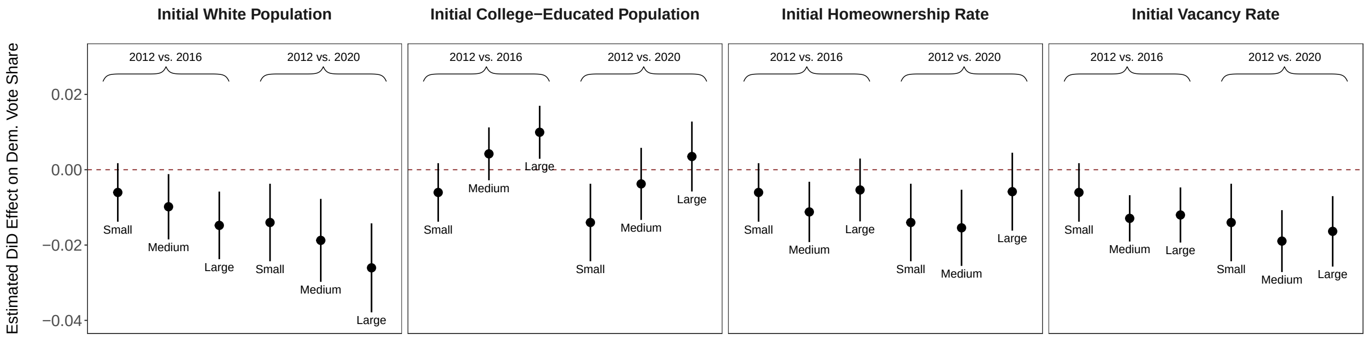

Figure D.2: **Heterogeneous Treatment Effects: Joint Model Results.** Instead of analyzing moderation effects separately as done in equation (2), this figure presents DiD point-estimates and 95% confidence intervals based on coefficients from one joint model that includes all the moderators discussed in Section 6 (initial white pop., initial college-educated pop., initial homeownership rate, and initial vacancy rate) and their interaction terms. As shown in the figure, the joint model results are substantively similar to those based on separate models (cf. Figures 3 & 4). To conserve space, the full table results will be available in the replication materials.

Appendix E Mediation Analysis

Following Acharya, Blackwell, and Sen (2016), I implement the method in two simple steps. In the first stage, I estimate the effect of the mediator (election-year GDP growth or employment rate) on the outcome (two-party Democratic vote share) by regressing the outcome on the mediator, the treatment (Chinese FREI exposure), their interaction (to allow the effect of the mediator to vary by the values of the treatment), and all other covariates:

$$\begin{aligned} Y_{it} = \alpha_i + P_t + \lambda_1(P_t E_{i,2016}) + \lambda_2(P_t D_{i,2012} E_{i,2016}) + \tau(P_t D_{i,2012}) \\ + \boldsymbol{\gamma}(P_t \boldsymbol{S}_{i,2012}) + \boldsymbol{\delta}\boldsymbol{U}_{it} + \boldsymbol{\kappa} P_t \boldsymbol{U}_{it} + \epsilon_{it}, \end{aligned} \tag{6}$$

where $E_{i,2016}$ represents a county's economic strength in 2016 recentered to the mean. All remaining variables are the same as those in equation (1). For 2012–2020 analyses, I fit the same equations here (and below) to the data but replace $E_{i,2016}$ with 2020 measures, $E_{i,2020}$.

In the second stage, I use estimates from the first stage to "demediate" the outcome variable by "removing from it the effect of the mediator and then estimating the [controlled direct] effect of the treatment on this demediated outcome" (Acharya, Blackwell, and Sen 2016):

$$Y^*_{it} = Y_{it} - \lambda_1(P_t E_{i,2016}) - \lambda_2(P_t D_{i,2012} E_{i,2016}), \tag{7}$$

$$Y^*_{it} = \alpha_i + P_t + \tau_e(P_t D_{i,2012}) + \boldsymbol{\gamma}(P_t \boldsymbol{S}_{i,2012}) + \delta \boldsymbol{U}_{it} + \kappa P_t \boldsymbol{U}_{it} + \epsilon_{it}, \tag{8}$$

where Y^*_{it} is the outcome two-party Democratic vote share demediated of the effects of the local economic strength in the election year, and equation (8) is the same as equation (1) but with the demediated outcome on the left-hand side. Since measures of local economic strength are recentered to their means, equation (8) will then estimate the controlled direct effect of Chinese FREI exposure for the average local economy ($E_{i,2016} = 0$). The indirect effect is thus the difference between the total effect [estimated previously from equation (1)] and the controlled direct effect [estimated here from equation (8)]. Following the authors, I obtain estimates using a nonparametric block-bootstrap by counties and 1,000 resamples.

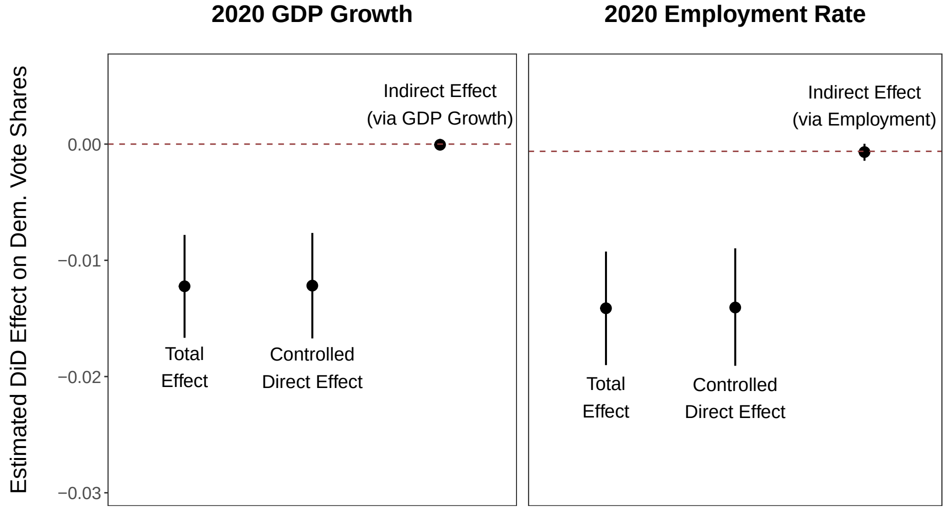

Figure E.1: **Effect Decomposition: Post-shock Election-year (2020) Local Economic Conditions as a Mediator.** The figure displays DiD point-estimates and 95% confidence intervals using 1,000 block-bootstraps at the county level. It shows the decomposition of the negative total effect of Chinese FREI exposure on changes in 2012–2020 Democratic vote shares when local GDP growth and employment rates in 2020 are set at their mean.

Table E.1: **Effect Decomposition: Sequential g-Estimation**

Quantity of Interest	Mean Estimate	95% Block-Bootstrapped C.I.
Total Effect of Chinese FREI Exposure, 2012 vs. 2016	−0.0099	[-0.01351, -0.0065]
Controlled Direct Effect	−0.0096	[-0.01328, -0.00609]
Indirect Effect via 2016 Local GDP Growth	−0.0003	[-0.00076, 0.00014]
Controlled Direct Effect	−0.0096	[-0.01328, -0.00608]
Indirect Effect via 2016 Local Employment Rate	−0.0003	[-0.00086, 0.00017]
Total Effect of Chinese FREI Exposure, 2012 vs. 2020	−0.0122	[-0.01666, -0.00781]
Controlled Direct Effect	−0.0122	[-0.01672, -0.00764]
Indirect Effect via 2020 Local GDP Growth	−0.0001	[-0.00056, 0.00045]
Controlled Direct Effect	−0.0122	[-0.01672, -0.00756]
Indirect Effect via 2020 Local Employment Rate	−0.0001	[-0.00073, 0.00059]

Note: This table presents estimates based on post-shock local economic conditions as a mediator between Chinese FREI exposure and two-party Democratic vote shares. 95% confidence intervals are based on 1,000 block-bootstraps at the county level.