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DO GOLDEN VISAS AFFECT HOUSING MARKETS? EVIDENCE FROM SYNTHETIC CONTROLS

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To Fernando and Paula,
whose support made this journey possible.

GLOSSARY

ATT Average Treatment Effect on the Treated. ii, 10, 17, 20, 21, 23–25

CBI Citizenship by Investment. ii, 1, 2, 4, 13

CRBI Citizenship and Residency by Investment. ii, 4, 14

DGP Data Generating Process. ii, 9

DiD Difference-in-differences. ii, 2, 6, 18, 26

FDI Foreign Direct Investment. ii, 1, 5, 6

GSCM Generalized Synthetic Control Method. ii, 8, 12, 17, 18, 20, 23, 26

HWNIs High-Net-Worth individuals. ii, 1, 4

IMF International Monetary Fund. ii, 2, 5–7

MSPE Mean Squared Prediction Error. ii, 10, 11

OECD Organisation for Economic Co-operation and Development. ii, 2–4, 7, 13, 26, 27

RBI Residency by Investment. ii, 1, 2, 4, 6, 13

SCM Synthetic Control Method. ii, 8

AI Disclaimer

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Eduardo Laranjeiro de Almeida, July 2025

Abstract, Keywords, and JEL Codes

This dissertation provides new insights on the causal impact of the so-called "Golden Visa" programs on housing markets in OECD countries. We address this gap in the literature by employing a synthetic control approach adapted for cases where countries adopt the policy at different times, to estimate what would have happened had the country not implemented the program. The impact was measured through four key housing indicators (House Price Index, Rent Price Index, Price-to-Income ratio and Price-to-Rent ratio) capturing price dynamics, rental trends, and affordability conditions while controlling for observable and unobservable heterogeneity. Our findings reveal heterogeneous effects, with upward price pressures found across several treated countries, suggesting that these programs can significantly drive housing price growth. These results extend the literature by providing a cross-country causal evaluation and offering new insights for real estate policy and investment migration governance.

Keywords: Golden Visa; Synthetic Control; Staggered Adoption; Housing Market.

JEL Codes: C31; C33; R21; R38.

Resumo, Palavras-Chave e Códigos JEL

Esta dissertação oferece novas perspetivas sobre o impacto causal dos chamados programas "Visto Gold" nos mercados imobiliários dos países da OCDE. Procuramos colmatar esta lacuna na literatura aplicando Controlos Sintéticos adaptado a casos em que os países adotam a política em momentos distintos, de forma a estimar o que teria acontecido caso o país não tivesse implementado o programa. O impacto foi medido com base em quatro indicadores habitacionais fundamentais (Índice de Preços da Habitação, Índice de Preços de Arrendamento, rácio Preço/Rendimento e rácio Preço/Arrendamento), captando a dinâmica dos preços, as tendências no arrendamento e as condições de acessibilidade, controlando simultaneamente a heterogeneidade observável e não observável. Os nossos resultados revelam efeitos díspares, com pressões ascendentes sobre os preços em vários países analisados, sugerindo que estes programas podem contribuir significativamente para o aumento dos preços da habitação. Estes resultados ampliam a literatura existente ao fornecer uma avaliação causal de âmbito multinacional e oferecem novas perspetivas para a política de habitação e a governação da migração por investimento.

KEYWORDS: Vistos Gold; Controlos Sintéticos; Tratamento Faseado; Mercado Imobiliário.

CÓDIGOS JEL: C31; C33; R21; R38.

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Acknowledgements

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This work is as much theirs as it is mine.

1 Introduction

Investment migration programs are becoming one of the most popular and used tools for countries aiming to attract foreign capital. In theory, these programs promise Foreign Direct Investment (FDI) and economic development, but in practice their impact remains poorly understood, especially in the housing market. This concern has been raising interest in researchers, with recent studies suggesting that these programs may instead drive speculative real estate activity and exacerbate housing affordability.

This dissertation provides new causal evidence on the economic impact of Golden Visa programs, using a manually built dataset and implementing a Synthetic Control Method design with staggered adoption. Specifically, we address three central questions: (1) To what extent do Golden Visa programs provoke distortions in the housing prices? (2) Do they only affect housing prices, or do they also affect the rental market? (3) Do these programs worsen housing affordability? As we shall see in section 5, our findings indicate that these programs do have a significant upward impact on housing prices and do worsen affordability conditions, as per rental dynamics, no robust effects were found, suggesting that the primary distortions occur in property purchasing markets rather than rental markets.

To contextualise this study, it is vital to understand the nature of Golden Visa programs, which can be disaggregated into Citizenship by Investment (CBI) and Residency by Investment (RBI) programs. These schemes offer foreign nationals citizenship or residency rights in exchange for financial contributions. Typical forms of investment include real estate purchases, business investments, and government donations. CBI programs grant instant or fast-tracked citizenship, most of the time faster than other methods and do not require extended physical residence. This makes them particularly appealing to High-Net-Worth individuals (HWNIs) looking for increased mobility, tax benefits, or financial diversification. On the other hand, RBI programs grant foreign investors temporary or permanent residency rather than immediate citizenship. While some RBI programs eventually lead to citizenship, they generally require longer residence periods, language proficiency, and ongoing financial contributions. In sum, the key distinction lies in the immediacy and scope of rights conceived: citizenship versus residency.

As mentioned, both program types are meant to attract foreign capital and stimulate growth. Supporters argue that wealthy investors bring rapid capital, create jobs, and overall boost the host country's economy. However, the effectiveness of these programs in delivering sustained and productive FDI remains unclear. Much of CBI and RBI-related investment flows into real estate, rapidly increasing housing prices (see Santos

& Strohmaier (2024)). Many argue these investments are merely motivated by the program's benefits, with investors meeting program requirements through passive high-end property investments that offer little to no contributions to innovation or employment (see Clerides & Kotsogiannis (2025)).

The design of these programs plays a crucial role in their outcomes. CBI programs often allow direct contribution to the government's national development projects, while RBI programs usually require tangible investments and physical presence. However, many RBI programs also offer alternative routes, such as job-creating enterprises or government fund/bond investments, to reduce the pressure on the real estate market. These alternatives suggest that the design can mitigate and limit the market distortions - Thirion & Scherrer (2018). Whether or not physical presence is required also affects the engagement with the host country. CBI programs that do not require investors to physically stay in the country may lead to more passive forms of investment and create opportunities for regulatory arbitrage, which can reduce the desired long-term economic benefits (OECD/FATF (2023)).

Despite the growing policy interest, empirical work on Golden Visa programs remains limited in scope and rigour. Most studies are merely descriptive, or when aiming to assess their impact, they are conducted at a single-country level, relying solely on correlational evidence, which makes it difficult to isolate the causal effect of policy adoption. For example, Xu et al. (2015) provided one of the first comprehensive overviews, highlighting risks to macroeconomic stability but without empirical impact analysis. Similarly, Surak & Tsuzuki (2021) and Thirion & Scherrer (2018) described European RBI schemes and their market significance but did not engage in any causal estimation. Surak (2022) also focused on supply and demand dynamics, offering rich context but again no econometric evaluation. Collectively, these studies serve as a foundation but leave open the question of the programs' true effects.

More recent empirical work includes Santos & Strohmaier (2024), who employed a Difference-in-differences (DiD) approach to demonstrate the impact of the Portuguese Golden Visa on the real estate market, driven by foreign buyers. However, this single-country focus limits broader conclusions. The International Monetary Fund (IMF) latest multi-country study (Clerides & Kotsogiannis 2025) employs panel OLS and staggered DiD methods to analyse housing market effects. Still, their approach faces limitations and falls short in the housing market assessment. Our study aims to address these gaps by offering robust comparative insights across Organisation for Economic Co-operation and Development (OECD) countries and highlighting how these programs affect the different housing indicators.

To directly address these issues, we manually construct a dataset focusing on OECD countries, tracking both the adoption and, where applicable, stoppage years of the programs. By employing a synthetic control method with staggered adoption, we estimate the impact of these programs on the housing market. This method allows us to build counterfactuals for each treated country while accounting for both observable and unobservable differences, eliminating possible bias. The method also allows a multiple outcome analysis (House Price Index, Rent Price Index, Price-to-Income ratio, and Price-to-Rent ratio), providing a clearer understanding of whether and how Golden Visa schemes affect housing dynamics beyond prices alone.

The rest of the study is organised as follows. Section 2 presents the current literature regarding golden visa programs and presents their main findings. Section 3 presents and outlines the methodological framework. Section 4 provides an overview of the data and key studied variables. Section 5 presents the main empirical results along with robustness checks. Finally, Section 6 presents the main conclusions and policy implications.

2 LITERATURE REVIEW

Investment migration programs have grown significantly in recent years and are now one of the most used instruments for policymakers seeking to attract foreign investment, according to the OECD in OECD/FATF (2023). They allow individuals to purchase citizenship or temporary/permanent residency in a jurisdiction by investing in the host economy. Depending on the design of each program, the required investments can be made in different ways, with the most popular options being real estate purchases, business investments, and direct contributions to government bonds or funds.

As mentioned, Golden Visa programs can be further divided into two main categories: RBI and CBI, and are also commonly jointly referred to as Citizenship and Residency by Investment (CRBI) programs. They differ in legal status, rights granted, and obligations. Understanding the difference between them is crucial to comprehending their implications.

CS Global Partners (2024) and Imperial Citizenship (2024) explain that the CBI programs grant full citizenship, including civic rights, sometimes even a passport, and do not obligate applicants to reside in or maintain lasting connections with the host country. They also state that these types of programs are usually a faster route to obtain citizenship when compared to other existing methods, making them more attractive for individuals. In contrast, RBI programs, offer residence rights in exchange for investment, depending on the program design the rights may be temporary or permanent. Some of these schemes may also provide a potential pathway to citizenship under specific conditions, such as physical presence for specific periods. While both CBI and RBI offer international mobility and provide a new legal status under a new jurisdiction, CBI programs tend to be more associated with immediate benefits and passport acquisition, whereas RBI supports a more gradual process.

Interest in these programs has increased significantly over time, as both individuals and governments can largely benefit from them. At the individual level, their notable investment amount requirements mean that they mainly appeal to HWNIs. HWNIs often apply to these programs for reasons such as increased international mobility, better educational and healthcare conditions, political stability, retirement planning and others (see EuroEducation (2024)). From the host country's perspective, these programs are essentially viewed as a means to generate capital without incurring debt, particularly during periods of economic recession, as noted by Surak & Tsuzuki (2021). They also allow governments to direct the earned capital directly into needed sectors, such as real estate and business development, thereby supporting the employment markets and, in some cases, funding national projects.

As previously concluded, the core idea behind Golden Visa programs is to attract economically advantaged individuals by offering residence or citizenship rights in return for financial contributions. By lowering the entry barriers for foreign investors, these programs are expected to increase FDI in the host country.

However, while these schemes do generate FDI, it is often not the desired type. Unlike traditional FDI, which is aimed at a long-term perspective and focused on promoting productivity, the Golden Visa-linked investment is frequently passive, concentrated in high-end real estate, and predominantly motivated by the benefits. As a result, applicants may sometimes overpay or accept smaller returns solely to meet the program requirements, which can lead to speculative bubbles and market distortions. Santos & Strohmaier (2024) found out in their study about Portugal's Golden Visa program that the scheme has led to a significant increase in house prices. In agreement, the European Parliamentary Research Service in their research about these programs, also warns that rapid capital inflows through these types of programs can make it harder for authorities to monitor projects. In sum, although their funds may help boost the host country's economy and reduce unemployment, their long-term effects remain a debate.

In a study conducted by the OECD/FATF (2023), some of these schemes may also be vulnerable to misuse by individuals aiming to legitimise illicit funds through real estate investments or shell companies. These international institutions also state that these schemes are susceptible of "identity laundering", allowing applicants to obscure their criminal histories and even transfer funds earned from corruption or fraud across borders.

The IMF raises another concern regarding these programs in their research. In Xu et al. (2015), they alert that Golden Visas can serve as a tool for tax evasion, particularly when applicants utilise newly acquired citizenship rights to avoid international tax transparency measures. By opening offshore accounts under alternative identities, applicants may avoid reporting rules under the Common Reporting Standard. The IMF also notes that some countries offering these programs also experience a rapid increase in deposits from tax havens.

All these problems associated with these programs have interested researchers and international institutions in assessing the actual impacts of Golden Visa programs. Although growing, only a limited body of empirical literature exists, with few studies quantifying the effects of these programs on the housing market and other economic outcomes.

Most early studies on these programs were conducted only at the descriptive level, primarily mapping the programs and reporting inflows rather than estimating the actual effects. Xu et al. (2015) wrote a working paper for the IMF, with one of the first comprehensive overviews. It discusses the implications of Economic Citizenship Programs for small economies, mainly focusing on the risks to macroeconomic and financial stability and does not attempt to calculate the impact of the programs on any economic or social outcomes. Similarly, Surak & Tsuzuki (2021) compile a comparative quantitative description of European RBI schemes. They concluded that, although their influence may be significant in certain markets, such as Greece, where RBI program applicants accounted for up to 72% of foreign real-estate purchases, their impact remains negligible relative to overall FDI. However, their study stops short and does not test whether the observed property purchases or investor flows would have occurred in the absence of the policy. Thirion & Scherrer (2018), in a technical report for the European Parliamentary Research Service, also describes the diverse designs across EU member states, highlights the differences in investment requirements and pathways to citizenship, but again provides no empirical analysis for assessing the impacts. In 2022, the Centre on Migration, Policy & Society (COMPAS) published its working paper, "Investment Migration Globally: The Dynamics of Supply and Demand," which provided an overview of these programs, primarily focusing on their supply and demand dynamics over time. While rich in context and descriptive statistics, it too avoids econometric estimation, focusing instead on the overall panorama of the investment migration market. Collectively, these descriptive studies provide essential groundwork, but they do not answer the key question of what would have happened had the policy not been implemented.

Santos & Strohmaier (2024) investigated the Portugal's Golden Visa program by first estimating bunching effects at the €500.000 minimum investment rule and then conducting a DiD approach to assess the impact of the policy. Their findings confirm that the introduction of the policy significantly increased property values by more than 10% in high-end properties, driven by foreign buyers. Although the design credibly isolates a causal effect, the study is only done on a single-country basis. Therefore, unable to conclude whether similar programs produce comparable results.

More recently, the IMF published a working paper by Clerides & Kotsogiannis (2025) measuring the impact of these programs on a wide range of countries and across multiple outcomes. Regarding the housing market, their analysis is the closest in spirit to ours, but it faces some limitations that we aim to address. First, their empirical strategy relies primarily on panel OLS, which remains vulnerable to endogeneity bias. Subsequently, a staggered DiD estimator (Callaway & Sant'Anna (2021)) was used as a robustness check, and, although it provides valuable causal insights, it heavily relies on the parallel trends assumption, which is enforced after conditioning on observed covariates. However, this approach does not fully account for unobservable heterogeneity, which is particularly important in a diverse set of countries and policy regimes. Second, their outcome analysis on the housing market was focused only on a single dependent variable (house price growth), excluding any other possible determinants or consequences arising from the implementation of Golden Visa programs. Lastly, though not specified, their control group may include countries with very different fiscal and monetary policy regimes, which, again, may bias the estimates.

To conclude, there are few studies on this topic, and those that exist either focus on a single country or provide merely descriptive accounts of many. To date, and aside from the IMF working paper by Clerides & Kotsogiannis (2025), no other study combines (i) a credible and robust counterfactual design, (ii) staggered adoption, allowing for a multi-program evaluation, and (iii) an analysis of multiple housing market indicators.

The present dissertation aims to fill this gap in the literature and complements and extends others' findings in several ways. First, it focuses on OECD countries, which are more comparable in terms of economic development and policy frameworks. Second, it applies the synthetic control method with staggered adoption to construct a counterfactual for each treated unit, explicitly controlling for both observable and unobservable differences and enabling a visual assessment of pre-treatment fit and avoiding the need to impose assumptions like parallel trends. Furthermore, we consider multiple outcome variables (House Price Index, Rent Price Index, Price-to-Income ratio, and Price-to-Rent ratio), offering a richer and more direct understanding of how Golden Visa programs shape housing markets beyond housing prices.

3 METHODOLOGY

The comparative case study is one of the most used approaches for evaluating the causal impacts of policy introductions. As highlighted by Abadie et al. (2010), this approach allows a comparison of outcomes of units (be it countries, regions, or cities) exposed to a policy or event (the treated group) with those of similar units that were not exposed to that intervention (the control group). The main idea behind this approach is to use the outcome of the control group as a proxy for what would have happened in the treated group had the policy (treatment) not been implemented.

This study is based on the Synthetic Control Method (SCM), an approach developed by Abadie & Gardeazabal (2003) and further advanced by Abadie et al. (2010). SCM essentially allows researchers to build a "doppleganger" by assigning a combination of weights to control units (untreated) such that the resulting synthetic control closely replicates the pre-intervention trends of the treated unit. This synthetic control is then used as a robust counterfactual, providing a basis for estimating and comparing post-intervention outcomes.

Furthermore, this study adopts the Generalized Synthetic Control Method (GSCM), which extends the capabilities of the traditional approach and allows for staggered treatment adoption. This is particularly relevant in our study since the countries implemented the program at different time points. In simple terms, this method builds a weighted average of countries that did not introduce Golden Visa programs to simulate what would have happened in a treated country had it not implemented the policy. In what follows, I will provide a brief description on how the GSCM works, closely following the original paper proposed by Xu (2017).

Let Y_{it} be the outcome of interest for country i at time t, τ the set of treated countries and C the set of control countries (i.e., those that are not treated during the observation period).. The sample consists of $N = N_{tr} + N_{co}$ countries, where N_{tr} is the number of treated countries and N_{co} is the number of countries in the control group. Each country is observed over T time periods. For each treated country $i \in \tau$, the number of pre-treatment periods is denoted $T_{0,i}$, and the exposure to treatment is observed for $T - T_{0,i}$ periods. Whereas countries in the control group $i \in C$ are never exposed to treatment over the observed period.

We express the outcome Y_{it} through a linear factor model as follows:

$$Y_{it} = \delta_{it} D_{it} + x'_{it}\beta + \lambda'_i f_t + \varepsilon_{it}, \tag{1}$$

where δ_{it} is a parameter that captures the specific treatment effect for each country i and at each period of time t; D_{it} is a binary variable that represents the treatment variable and assumes the value of 1 when a country i is under treatment at time t, and 0 otherwise; x'_{it} is a column vector with k observed covariates for country i at time t and $\beta = [\beta_1, \ldots, \beta_k]'$ is a $(k \times 1)$ vector of unknown coefficients that quantify the impact of each observed covariate in x_{it} on the outcome Y_{it}; the factor component of the model, $\lambda'_i f_t$, is specified as a linear additive structure [1], $\lambda'_i f_t = \lambda_{i1} f_{1t} + \lambda_{i2} f_{2t} + \cdots + \lambda_{ir} f_{rt}$, where $f_t = [f_{1t}, \ldots, f_{rt}]'$ is an $(r \times 1)$ vector that represent unobserved common factors that vary over time and affect all countries in the sample and $\lambda_i = [\lambda_{i1}, \ldots, \lambda_{ir}]'$ is an $(r \times 1)$ vector of unknown factor loadings, which measure the weight of each common factor impact on each country i; ε_{it} represents idiosyncratic shocks, meaning, it represents unobserved variations that are specific to country i at time t.

Under the traditional notation for causal inference (Neyman (1923);Rubin (1974);Holland (1986)) Let $Y_{it}(1)$ and $Y_{it}(0)$ denote the potential outcomes for country i at time t, under treatment ($D_{it} = 1$) and no treatment ($D_{it} = 0$), respectively. In other words, $Y_{it}(1)$ is the outcome observed when country i is treated at time t, and $Y_{it}(0)$ is the outcome when it is not treated.

Assuming a linear structure, the Data Generating Process (DGP) for each country i can be written as:

$$Y_i = D_i \circ \delta_i + X_i\beta + \lambda_i F + \varepsilon_i, \quad i = 1, 2, \ldots, N, \tag{2}$$

here, $Y_i = [Y_{i1}, Y_{i2}, \ldots, Y_{iT}]'$ is the observed outcome vector for country i; the treatment indicator vector is $D_i = [D_{i1}, D_{i2}, \ldots, D_{iT}]'$, the corresponding treatment effect is $\delta_i = [\delta_{i1}, \delta_{i2}, \ldots, \delta_{iT}]'$ and, $D_i \circ \delta_i$ is the point-wise product of the treatment indicator and the treatment effect vectors for country i; the matrix of observed covariates for country i is $X_i = [x_{i1}, x_{i2}, \ldots, x_{iT})'$ with dimension $T \times k$, and the matrix of unobserved common factors over time is $[F = [f_1, f_2, \ldots, f_T]'$ with dimension $T \times r$; $\varepsilon_i = [\varepsilon_{i1}, \varepsilon_{i2}, \ldots, \varepsilon_{iT}]'$ is a $(T \times 1)$ vector of idiosyncratic shocks.

The indices of the control units range from 1 to N_{co}, while those of the treated units range from $N_{\text{co}}+1$ to N. The DGP of a control unit is given by: $Y_i = X_i\beta + F\lambda_i + \varepsilon_i, \quad i \in \{1, 2, \ldots, N_{\text{co}}\}$. By stacking all control units together, we obtain the outcome of interest

[1]The inclusion of the term $\lambda'_i f_t$ helps control for unobserved time-varying factors common across countries. This term is important given that it could influence both the likelihood of adopting Golden Visa programs and the outcomes studied. By capturing these latent factors, the model helps mitigate concerns about selection bias and unobserved confounders, thus improving the robustness of the estimated treatment effects.

for $i \in C$ as follows:

$$Y_{\text{co}} = X_{\text{co}}\beta + F\Lambda'_{\text{co}} + \varepsilon_{\text{co}}, \tag{3}$$

in which $Y_{\text{co}} = [Y_{i1}, Y_{i2}, \ldots, Y_{N_{\text{co}}}]'$ and $\varepsilon_{\text{co}} = [\varepsilon_1, \varepsilon_2, \ldots, \varepsilon_{N_{\text{co}}}]'$ are $(T \times N_{\text{co}})$ matrices where each column represents the full time series for a single control country; X_{co} is a three-dimensional array with dimensions $T \times N_{\text{co}} \times p$, where each p-dimensional vector corresponds to the observed covariates for a given country i at time t; and $\Lambda'_{\text{co}} = [\lambda_1, \lambda_2, \ldots, \lambda_{N_{\text{co}}}]$ is a $(N_{\text{co}} \times r)$ matrix. The selection of the optimal number of latent factors r is conducted through a cross-validation procedure that minimises the Mean Squared Prediction Error (MSPE).

As shown in equation (3), this specification applies only to the control group, whose outcomes are unaffected by treatment. This control structure is then used to construct the counterfactual outcomes for the treated units, allowing for the estimation of the Average Treatment Effect on the Treated (ATT) at times $t > T_0$.

$$ATT_{t,\, t>T_0} = \frac{1}{N_{tr}} \sum_{i \in \tau} [Y_{it}(1) - Y_{it}(0)] = \frac{1}{N_{tr}} \sum_{i \in \tau} \delta_{it}. \tag{4}$$

In the context of this study, where the treatment corresponds to the implementation of Golden Visa programs, the observed outcome $Y_{it}(1)$ reflects the actual post-treatment behaviour of treated countries. The main goal is to estimate the unobserved counterfactual outcome $\hat{Y}_{it}(0)$, which represents how those countries would have evolved in the absence of these programs. Estimating this counterfactual relies on five technical assumptions:

The first assumption is given in equation (1) and is called the functional form assumption. Among the other, the following is considered by the author to be the most important.

Assumption 2. *Strict exogeneity*

$$\varepsilon_{it} \perp\!\!\!\perp D_{js}, X_{js}, \lambda_j, f_s \quad \forall i, j, t, s.$$

Assumption 2 requires that the error term ε_{it} is independent of treatment assignments D_{js}, covariates X_{js}, and unobserved temporal heterogeneities (λ_j, f_s). This implies that ε_{it} is uncorrelated with past, present, and future treatments and covariates across all units, which is stronger than conditional mean independence.

Assumption 3. *Weak serial dependence of the error terms*

$$\text{Cov}(\varepsilon_{it}, \varepsilon_{is}) \to 0 \quad as \quad |t - s| \to \infty.$$

This is the same as saying that the error terms are assumed to be weakly dependent over time. This condition can be verified by testing for stationarity in the data, for instance, through unit root tests. It ensures that persistent autocorrelation does not bias the estimation.

Assumption 4. *Regularity conditions*

$$\frac{1}{NT}\sum_{i=1}^{N}\sum_{t=1}^{T} X_{it}^{\top} X_{it} \xrightarrow{p} Q.$$

These are the moment conditions for convergence and say that for some finite and positive-definite matrix Q. This regularity condition guarantees the convergence of the estimator as both the cross-sectional and temporal dimensions increase.

Assumption 5. *Cross-sectional independence and homoscedasticity of the error terms*

$$Cov(\varepsilon_{it}, \varepsilon_{jt}) = 0 \quad for\ i \neq j, \quad and \quad Var(\varepsilon_{it}) = \sigma^2.$$

This assumption assures that the error terms are uncorrelated across units and that they have a constant variance, which is crucial for a valid inference.

As previously noted, the goal is to estimate the counterfactual $\hat{Y}_{it}(0)$ which is the predicted outcome that unit i would have experienced in the absence of treatment, to then compute the treatment effect: $\hat{\delta}_{it} = Y_{it}(1) - \hat{Y}_{it}(0)$.

However, in order to estimate this counterfactual, we must first obtain the parameters $\hat{\beta}$, $\hat{F}$, and $\hat{\Lambda}_{co}$ using an Interactive Fixed Effects (IFE) model, from the control group data:

$$\hat{\beta}, \hat{F}, \hat{\Lambda}_{co} = \arg\min_{\beta, \tilde{F}, \tilde{\Lambda}_{co}} \sum_{i \in C} \left(Y_i - X_i\beta - \tilde{F}\tilde{\lambda}_i\right)' \left(Y_i - X_i\beta - \tilde{F}\tilde{\lambda}_i\right), \tag{5}$$

where, the normalisation condition $\frac{\tilde{F}'\tilde{F}}{T} = I_r$ is imposed, and $\tilde{\Lambda}_{co}'\tilde{\Lambda}_{co}$ is diagonal.

The next step consists of estimating the factor loadings for each treated unit i, considering solely pre-treatment data. These loadings are what link each treated country to the common factors, which are chosen to minimise the MSPE of the model. This process ensures that the synthetic control accurately reflects each treated country's pre-treatment trajectory and allows for both positive and negative weights.

$$\begin{aligned}\hat{\lambda}_i &= \arg\min_{\tilde{\lambda}_i}(Y_i^0 - X_i^0\hat{\beta} - \hat{F}^0\tilde{\lambda}_i)'(Y_i^0 - X_i^0\hat{\beta} - \hat{F}^0\tilde{\lambda}_i) \\ &= (\hat{F}^{0\prime}\hat{F}^0)^{-1}\hat{F}^{0\prime}(Y_i^0 - X_i^0\hat{\beta}), \quad i \in \tau.\end{aligned} \tag{6}$$

In which the superscripts “0”s denote the pretreatment periods. Once we obtain $\hat{\beta}$, $\hat{F}$, and $\hat{\lambda}_i$, the third and last step is to estimate the counterfactual outcomes for each treated country in the post-treatment period, which can be calculated as follows:

$$\hat{Y}_{it}(0) = x'_{it}\hat{\beta} + \hat{\lambda}'_i \hat{f}_t, \quad \text{for } i \in \tau,\ t > T_0. \tag{7}$$

Finally, to assess the actual impact, the GSCM computes the treatment effect for each treated unit i at time t as the difference between the observed outcome and the estimated counterfactual:

$$\hat{\delta}_{it} = Y_{it}(1) - \hat{Y}_{it}(0), \tag{8}$$

where $\hat{Y}_{it}(0)$ is the predicted outcome that unit i would have experienced in the absence of treatment.

Then, ATT_t is calculated by averaging the individual treatment effects across all treated countries. That is, it corresponds to the mean difference between the observed outcomes and the estimated counterfactuals in the post-treatment period:

$$\widehat{ATT}_t = \frac{1}{N_{tr}} \sum_{i \in \tau} \left(Y_{it}(1) - \hat{Y}_{it}(0) \right), \quad \text{for } t > T_0. \tag{9}$$

Unlike the classical synthetic control method, which typically relies on placebo or permutation inference to approximate the sampling distribution of treatment effects (Abadie et al. (2010); Abadie & Cattaneo (2018)), the generalised synthetic control framework assesses the statistical significance using a bootstrap-based approach. Specifically, it uses a parametric IFE model with our choice of 3,000 bootstrap replications to compute standard errors, 95% confidence intervals, and p-values for the estimates. Briefly explaining, in each replication, the residuals are resampled to generate counterfactual outcomes, producing an empirical distribution of the estimates. This procedure accounts for both cross-sectional and temporal dependence in the panel data, making it well-suited for our study that involves staggered treatment adoption, where traditional methods may bias the results.

4 Data

4.1 Overview

This study was conducted under a manually built dataset, combining different sources. It encompasses macroeconomic indicators collected from the World Bank, World Inequality and OECD Data Explorer. Housing and rental market indications were collected solely from OECD Data Explorer.

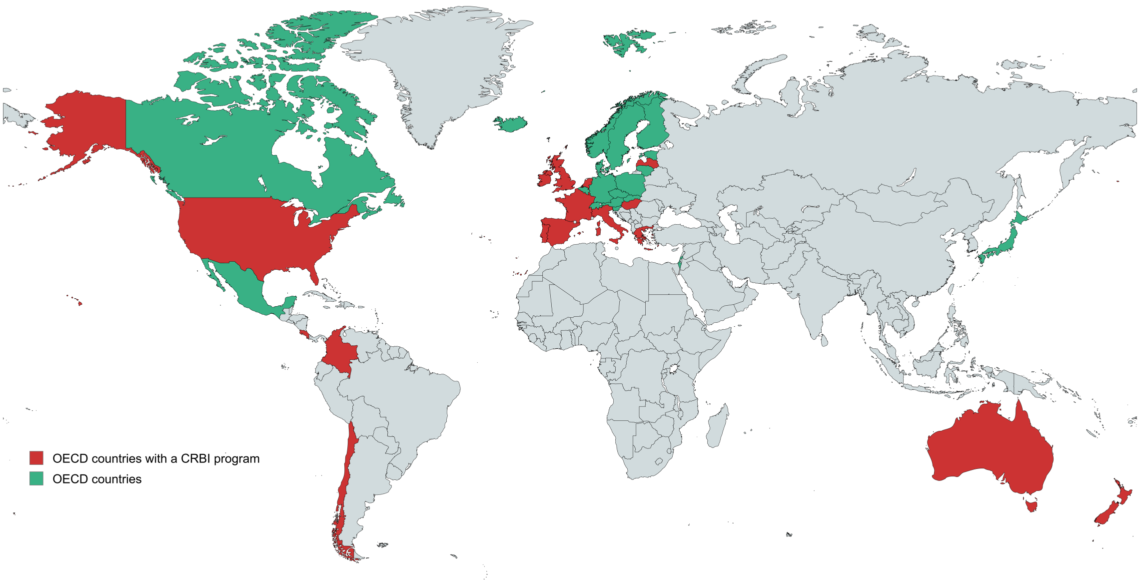
Because there is no centralised or standardised repository for these investment programs, gathering the data required consulting a wide range of sources. It was mainly based on information compiled by firms active in the investment migration industry who aggregate and disseminate program details through their online platforms, such as Henley & Partners, Arton Capital, CS Global Partners, Best Citizenships, and Investment Migration Insider (IMI).

IMI was particularly useful due to not only providing detailed coverage of the program designs but also their legal foundation. Building the dataset involved several challenges: inconsistencies across sources, changes in program features over time, and difficulties identifying the implementation and, in some cases, closure dates. To address these gaps, searches were conducted not only across official government portals but also through press releases, media reports, and industry publications. Despite these efforts, some historical information, particularly for less-documented programs, remains incomplete and potentially inaccurate.

Our initial aim was to cover all countries offering golden visa programs. However, due to severe data limitations, particularly missing observations for housing market variables, we restricted the analysis only to OECD countries. The final dataset covers a total of 36 jurisdictions, of which 17 implemented a Golden Visa program at some point. We consider this sample size adequate for a cross-country empirical analysis. Despite all the limitations, we are still able to capture a wide range of geographic, political, legislative, and economic contexts, allowing for different comparisons across different regions and wealth conditions. The collected data is annual and it ranges from 2007 to 2023.

4.2 Investment Programs Policy Database

Since there is no available dataset containing all the detailed information about these programs, it was crucial to construct one from scratch. Initially, our aim was to cover and discriminate between both CBI and RBI programs, as these two categories differ in their legal implications and potential economic effects. However, because our final empirical sample was largely reduced in size, we treat and analyse their impact together, considering CRBI programs. In our dataset, we define a binary indicator variable, *crbi_treated*, which assumes the value of 1 if a country offers either type of Golden Visa program and 0 otherwise.



Source: https://www.mapchart.net/

FIGURE 1: Distribution of Golden Visas in OECD countries

Another challenge arises when it comes to finding the dates of implementation and, in some cases, discontinuation. Since each country had full autonomy to introduce the policy whenever it made sense for them, this collection consisted of a vast research in official government releases, investment migration industry websites, FATF/OECD publications, other studies and credible media. Still, for some programs, we couldn't find the exact start date, therefore, in those cases, we considered the year of the official policy announcement or the year it was legally documented.

Country	Start Date	End Date
Australia	2012	2024
Chile	2022*	Active
Colombia	2017*	Active
Costa Rica	2021*	Active
France	2016	Active
Greece	2014	Active
Hungary	2024	Active
Ireland	2012	2023
Italy	2017	Active
Latvia	2010	Active
Luxembourg	2017	Active
Netherlands	2013	Active
New Zealand	2022	Active
Portugal	2012	Active
Spain	2013	2025
United Kingdom	1994	2022
United States	1990	Active

Note: "*" refers to the year of the official policy release considered; Please find the program detailed table in appendixA

TABLE I: Start and End Years of Golden Visa Programs in OECD Countries

4.3 Variable Description

To evaluate the impact of Golden Visa programs on the housing market, a set of outcome and control variables was collected. The outcome variables studied were collected from the OECD Data Explorer and are expressed as indices with 2015 as the base year (=100), seasonally adjusted and not calendar adjusted, and observed at an annual frequency.

- House Price Index (HPI) - reflects real house price dynamics and allows us to assess whether Golden Visa programs induce distortions or price pressures in property dwellings.

- Rent Price Index (RPI) - measures the evolution of residential rental prices, capturing potential spillover effects of Golden Visa programs on rental markets.

- Price-to-Income Ratio (PTI) - serves as an affordability indicator comparing house prices to household incomes. It was included since it allows us to assess whether Golden Visa programs worsen affordability conditions.

- Price-to-Rent Ratio (PTR) - represents the housing market equilibrium by comparing property purchase prices to rental prices, showing whether Golden Visa programs influence buy-versus-rent incentives.

Table II presents summary statistics for the outcome variables over the period 2007–2023, excluding missing observations. The House Price Index (HPI) and Price-to-Rent Ratio (PTR) exhibit similar mean values (around 109 and 110, respectively) and relatively high dispersion, indicating substantial variation across countries and time. The Rent Price Index (RPI) is more stable, with a mean close to 101 and a lower standard deviation, while the Price-to-Income Ratio (PTI) averages around 106, reflecting moderate variability in housing affordability. These descriptive patterns provide initial insights into cross-country differences in housing market dynamics prior to causal estimation.

Variable	N	Mean	Median	SD	P5	P95
HPI	594	109.12	105.28	21.15	78.51	148.87
RPI	612	100.98	100.00	15.06	78.87	125.89
PTI	560	105.84	102.92	15.06	85.16	133.64
PTR	594	110.26	106.43	19.97	82.33	148.90

TABLE II: Summary statistics of outcome variables

To isolate the causal effect of Golden Visa programs and avoid the problem of omitted variable bias, a set of macroeconomic control variables was included. These variables help account for other factors that might influence housing markets besides the implementation of the programs. From the OECD Data Explorer, we extracted Gross Domestic Product per capita (GDPpc), included to capture the overall development and productivity levels of the economy. Then, from the World Bank - World Development Indicators (WDI) we extracted: Labour Force (LF), which measures the size of a country's active workforce since it may influence housing demand and overall economic activity; Government Spending (GS) was used to control for fiscal policies that may potentially influence household's disposable income and, subsequently, housing affordability. Lastly, Gross Capital Formation (GCF), which captures investment in physical assets such as infrastructure and buildings. These indicators help control for conditions that may also shape the housing market.

Variable	N	Mean	Median	SD	P5	P95
GDPpc	612.00	47,910.80	47,135.45	20,108.33	20,777.14	73,934.89
LF	612.00	16,559,867.32	4,945,937.00	29,282,708.12	325,216.10	65,750,440.30
GS	610.00	241,683.18	77,156.57	495,288.56	5,393.75	932,568.94
GCF	612.00	296,057.23	90,970.72	693,525.83	6,935.38	1,153,488.37

Note: GS and GCF are in 10^6 USD

TABLE III: Summary Statistics of Control Variables

5 EMPIRICAL RESULTS

This chapter begins by presenting the baseline estimates of the impact of Golden Visa programs on the treated countries. Using the GSCM, we estimate the counterfactual outcomes in the absence of the program and compute the ATT across the defined outcome variables. Following the baseline estimation, we assess the robustness of the results, section 5.2.1 explores if the results are sensitive to the timing of treatment by re-estimating effects with treatment delayed by three years, accounting for potential policy implementation lags and market adjustment dynamics.

5.1 Baseline Results

Before beginning the main empirical analysis, we first need to examine the structure of the dataset. Figure 2 provides a visual representation of the treatment assignment across countries and years, as well as missing data patterns. As previously explained, countries implemented the program at different times, and, since we do not expect the economic shocks and policy effects to occur instantaneously, we consider a one-year lag in their commencement year. The different shades represent the treatment status of countries over time. The dark blue cells mark the post-treatment periods for countries that implemented the policy, whereas the bright blue cells refer to the pre-treatment periods for these same treated countries. Light blue cells correspond to control group observations, which include countries that were never treated throughout the observed period. Grey cells identify treated countries that were later excluded from the estimation due to insufficient pre-treatment periods. Finally, white cells indicate missing data.

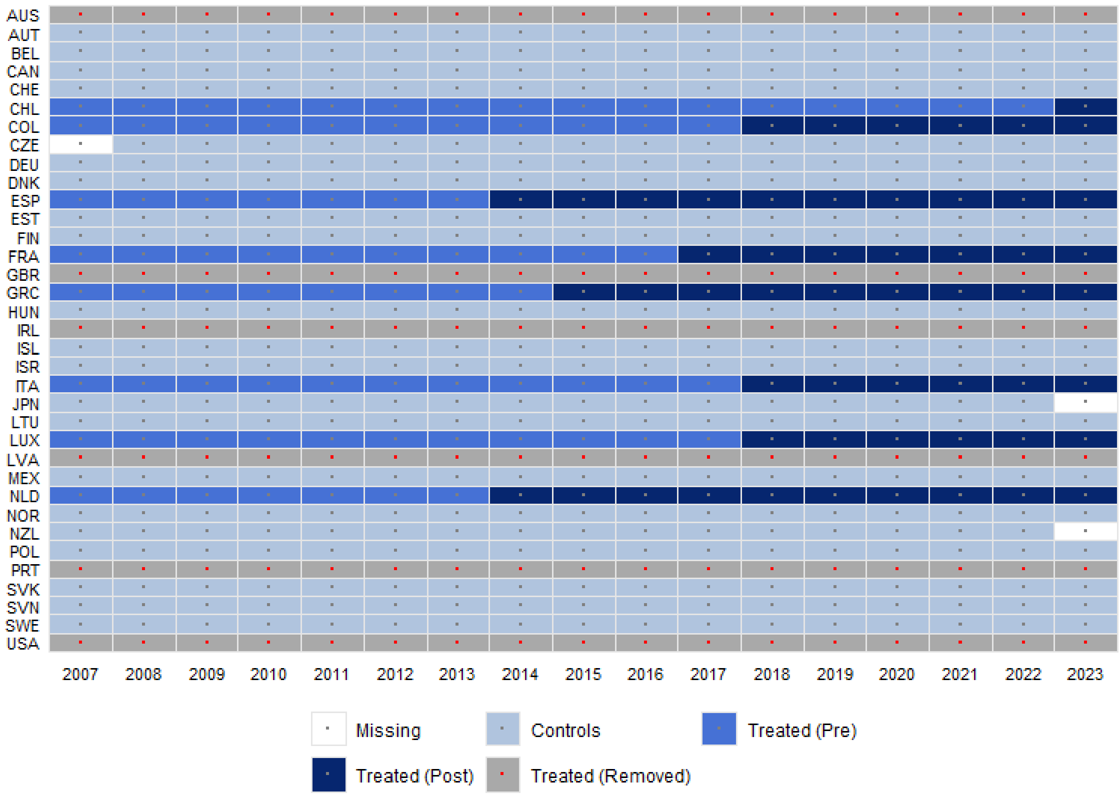


FIGURE 2: HPI - Sample Coverage and Intervention Periods

As observed we ended up with a total of eight treated units, twenty-one control units and a 17 year horizon ($N_{tr} = 8; N_{co} = 21; T = 17$). In order to construct a reliable model and proceed with our analysis, we set the minimum pre-treatment periods as 7. This criterion led to the elimination of six treated units, in particular: Australia ($T_{0,Australia} = 6$); United Kingdom ($T_{0,UnitedKingdom} = 0$); Ireland ($T_{0,Ireland} = 6$); Latvia ($T_{0,Latvia} = 4$); Portugal ($T_{0,Portugal} = 6$); United Stated ($T_{0,UnitedStated} = 6$).

We expect that the GSCM may yield an imperfect pre-treatment fit for some treated countries, particularly given the limitations imposed by data availability and the presence of missing values in certain periods. Despite these constraints, the GSCM approach still provides a significant improvement over other methods, such as the traditional DiD, which are strongly restricted by the parallel trends assumption (Ferman & Pinto (2021)). Nonetheless, we believe this is the most suitable approach to better study the problem at hand.

Figure 3 exhibits the average counterfactual trajectories for the studied outcome variables five periods before and after. Each panel displays the real outcome (solid black line) and its corresponding synthetic counterfactual (dashed blue line), the vertical grey line marks the treatment period. This analysis allows us to assess the quality of the pre-treatment fit as well as the divergence between actual and counterfactual paths after the program implementation.

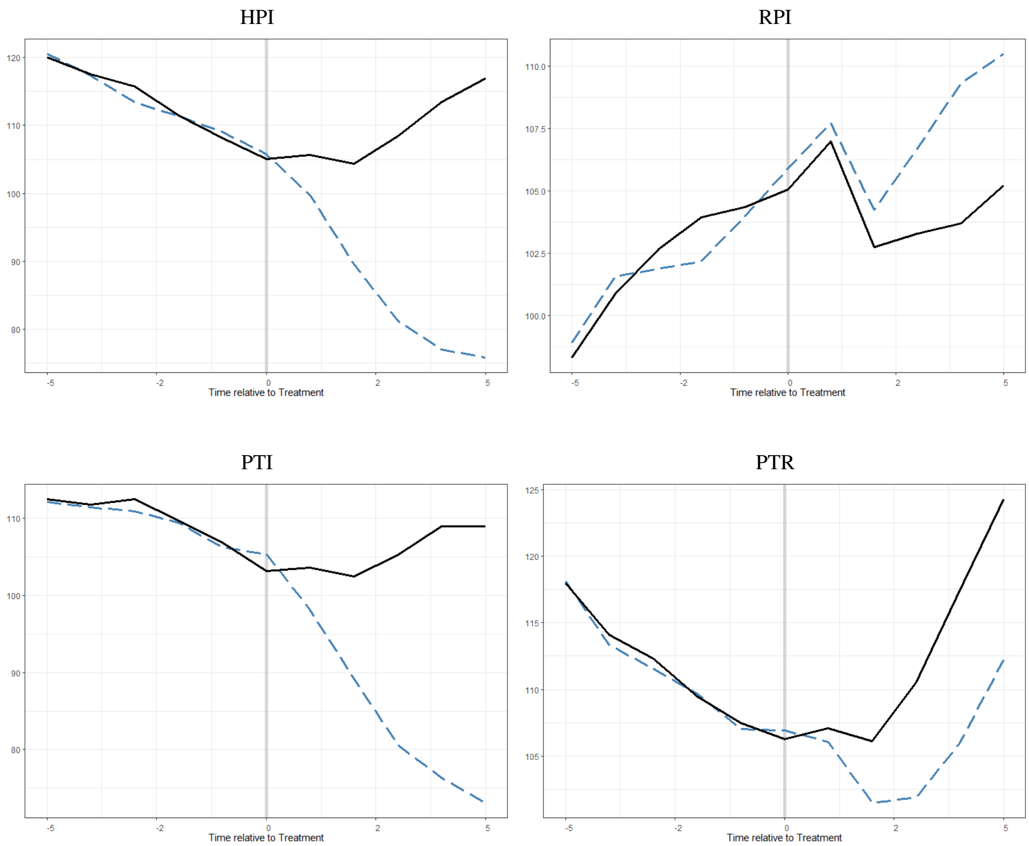


FIGURE 3: Average Counterfactual Trajectories

Consistent with expectations, despite RPI, the other synthetic trajectories almost overlap the actual observed path prior to treatment, which tells us that the model is well-built and reliable, giving credibility for the analysis. As displayed in HPI's panel, after the treatment implementation (grey vertical line), the synthetic counterfactual significantly diverges from the observed outcome. While the real outcome stabilises in the first two periods and increases afterwards, the synthetic counterfactual continues to decrease after the treatment. This informs us that these programs do have a significant impact on HPI. As noted, in the case of RPI, the fit is not as good as the other outcome variables, meaning the model is not capable of capturing rental price dynamics with the same level of accuracy as the others. Despite that, after the treatment, the comparison of the actual outcome and the counterfactual tells us that the rental prices grow at a slower pace in the real outcome, possibly hinting to the fact that the impact of these programs on rental prices might be weaker, contrasting with purchasing prices. For PTI, the pre-treatment fit is very good, almost identical between both trajectories. With a similar behaviour as HPI, after the treatment, the synthetic counterfactual continues to decrease, while the observed PTI stabilises and remains higher, indicating that housing affordability has worsened compared to the counterfactual scenario in the absence of the program. Finally, PTR shows a similar pattern, with the observed series rising sharply after the treatment while the synthetic counterfactual remains relatively flat. This divergence suggests that property purchase prices increased disproportionately compared to rents, reinforcing the evidence of market distortions caused by these programs.

To complement the visual analysis and to quantify the effects, IV presents the estimated ATT for each outcome variable. The ATT is calculated as the difference between the actual and synthetic series after the implementation year for each treated country. Alongside with the estimates, we present their standard errors, 95% confidence intervals, and bootstrap p-values.

	Estimate	S.E.	CI.lower	CI.upper	p.value
HPI	50.6	22.7	6.1	95.2	0.026**
RPI	-5.7	6.9	-19.2	7.8	0.411
PTI	46.8	19.52	8.5	85.1	0.016**
PTR	15.9	20.6	-24.5	56.3	0.441

TABLE IV: ATT per outcome variable

Our findings indicate that only HPI and PTI exhibit a statistically significant effect at the 5% level. Both of them have a positive sign and an estimated increase of 50.6 and 46.8 points, respectively. These values align with the visual evidence, suggesting that the introduction of these programs did increase house prices and worsen affordability conditions. However, when it comes to RPI and PTR, they show no statistically significant effects, with both p-values surpassing the 10% level, indicating that the model was not able to accurately assess the impact of these programs on rental dynamics.

5.1.1 Country Case Studies

Another useful tool of the GSCM is that we can assess the individual effect for each country. In what follows, we will present the country-level case studies to visualise and assess the estimated treatment effects obtained. In sum, and as explained in section 3, the model estimates a synthetic counterfactual for each treated country by assigning an optimal set of weights of the donor pool, these weights can be found in appendix A. To better understand the dynamics of the policy's impact, each treated country's observed outcome trajectory is compared with its synthetic counterfactual across the four mentioned outcomes: HPI, RPI, PTI and PTR. Same as in Figure 3, the solid black line represents the actual outcome, the dashed blue line is the synthetic counterfactual and the grey vertical line indicates the treatment year.

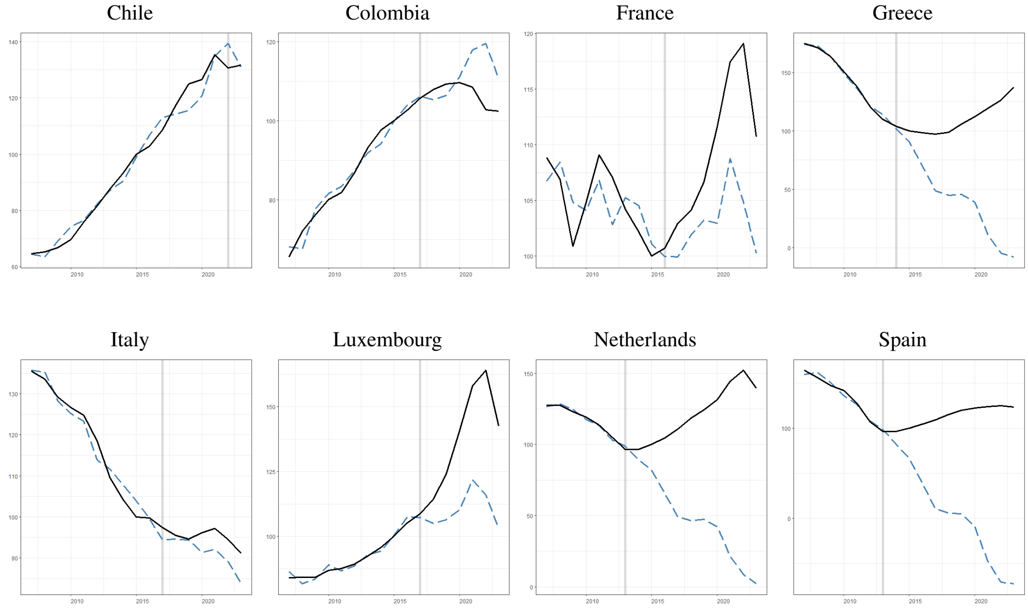


FIGURE 4: HPI - Synthetic Control Results by Country

Figure 4 displays the observed and estimated counterfactual trajectory of HPI for all treated countries. In the majority of cases, the synthetic control closely tracks the treated country's outcome in the pre-treatment period, indicating a good model fit and giving credibility for the conclusions. However, taking a closer look at each specific panel, some conclusions can be made. Though the ATT for HPI was large in magnitude, we can see it is not the case for all countries, and even, in Colombia, the behaviour of the counterfactual tells us the treatment effect was negative. Additionally, France does not display a good pre-treatment fit, therefore, no credible conclusion about it can be made with credibility.

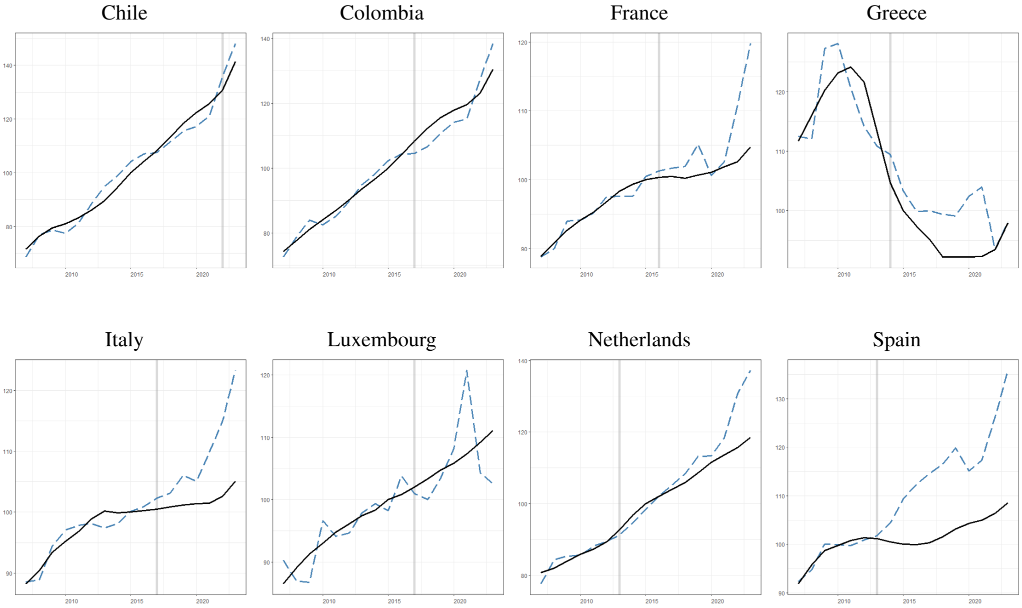


FIGURE 5: RPI - Synthetic Control Results by Country

As for RPI, we saw before that the average pre-treatment fit was not that good, the estimate had a negative coefficient and no staistical significance was achieved. In Figure 5, we observe that for France, Greece, Italy and Luxembourg, the poor fit explains the average bad fit. Moreover, it may be the reason why the estimate is not significant. As for the sign of the coefficient, besides Colombia that only around 2021 surpasses the synthetic trajectory, it seems consistent across all countries.

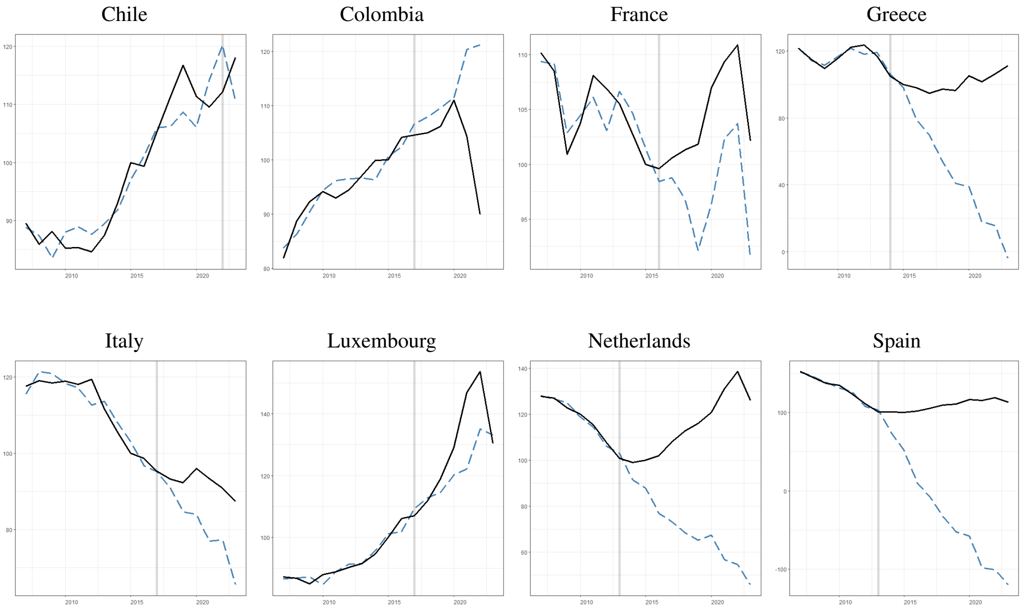


FIGURE 6: PTI - Synthetic Control Results by Country

Figure 6 presents the plots for PTI. In the average panorama, the pre-treatment fit was very good, with the synthetic trajectory almost overlapping the actual outcome. However, at the country-level, we observe that for some countries the same is not observed. Chile, Colombia and France present a poor approximation, probably due to the erratic pre-treatment behaviour. Additionally, we observe that for Colombia, the estimated outcome was positive , contrarily to the conclusion from the average effect.

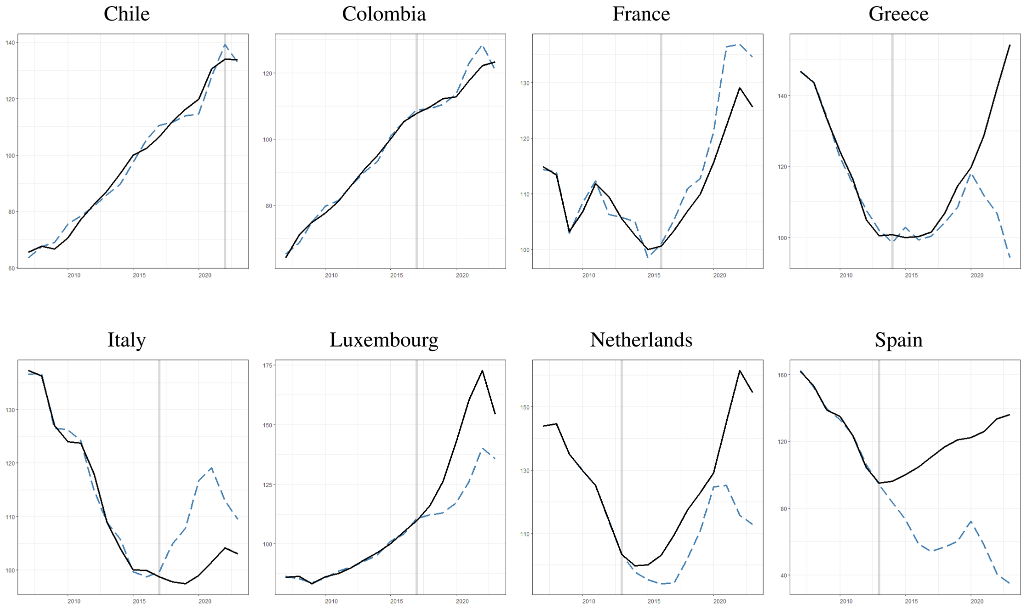


FIGURE 7: PTR - Synthetic Control Results by Country

Figure 7 presents the same analysis for PTR. Despite an excellent pre-treatment fit, with some synthetic controls exactly overlapping the actual outcomes prior to the policy intervention, the ATT was not statistically significant. This is particularly interesting and happens because the effect size may be too small, the variance could be too large, or even the short number of post-treatment periods could be in play. In GSCM, the statistical significance depends not only on model fit but also on the consistency and magnitude of post-treatment divergence from the synthetic counterfactual.

Overall, these country-level case studies demonstrate the importance of analysing beyond average treatment effects. This way we better understand the heterogeneity of policy impacts since no country has the same monetary, fiscal and housing regimes. As expected, while some countries display clear and substantial divergences between actual and synthetic trajectories, others show little to no effect or suffer from poor pre-treatment fit, which constrains inference. This variability highlights the need for caution when generalising results across contexts and underscores the value of disaggregated studies.

5.2 Robustness Checks

5.2.1 Sensitivity to Treatment Timing

A common concern in policy evaluation that aligns with economic reasoning is that treatment effects may not manifest immediately after program adoption. In particular, housing markets may often exhibit significant lags in adjustment as it takes time for policies to gain recognition and it also takes time for buyers, sellers, and investors to adjust their decisions. With this in mind, it is crucial to assess whether our results are sensitive to the considered timing of treatment, we rerun alternative specifications with the treatment set to begin three years after the official policy introduction ($t_0 + 3$).

	Estimate	S.E.	CI.lower	CI.upper	p.value
HPI	27.8	14.8	-1.2	56.9	0.06*
RPI	-15.4	6.8	-28.7	-2.1	0.024**
PTI	21.4	29.2	-35.7	78.5	0.463
PTR	36.6	20.2	-3.1	76.3	0.071*

TABLE V: ATT per outcome variable at $T_0 + 3$

Table V displays the ATT for the studied outcome variables considering the three-year delay. Comparing them with the baseline ATT results, Table IV, we observe that the sign of the coefficient remains consistent across all outcome variables, reinforcing the qualitative robustness of our findings. However, the statistical significance of the estimates suffered some variation:

- HPI and PTR now remain significant only at the 10% level;
- PTI loses significance entirely, although its positive sign remains;
- RPI becomes statistically significant whilst maintaining a negative coefficient.

These shifts can be explained by the extension of the pre-treatment period, which led to the incorporation of more observations, resulting in two main implications: (i) improves the model's capacity to construct accurate counterfactuals; (ii) adds more treated units that meet the minimum seven pre-treatment period requirement, introducing additional variation and possibly some cross-country heterogeneity.

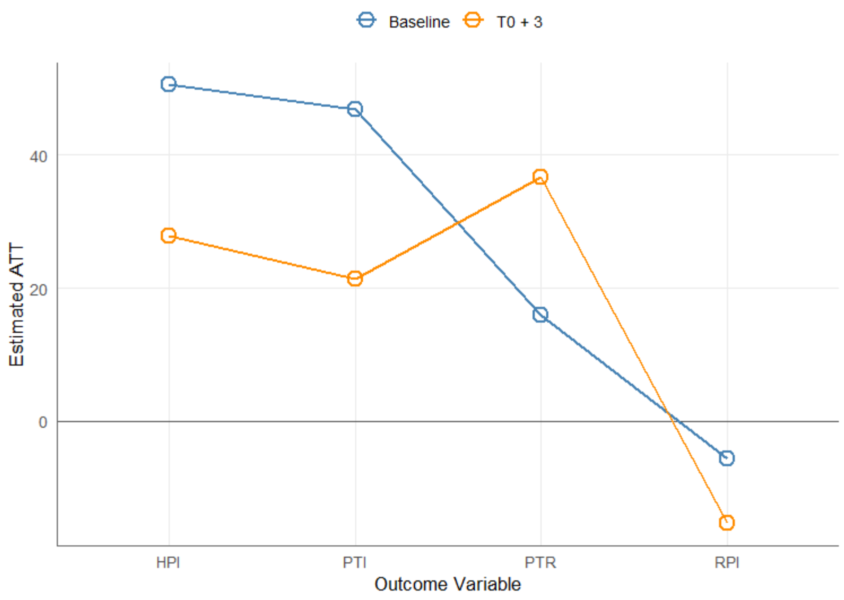


FIGURE 8: Magnitude comparison

Figure 8 provides a visual comparison of the ATT estimates's magnitude across the baseline and delayed treatment specification ($T_0 + 3$). As mentioned, while the direction of the effects remains consistent across all four outcome variables, notable differences in magnitude are observed. For instance, the estimated impact on HPI drops from 50.6 to 27.8, nearly halving the baseline effect, while the PTR estimate more than doubles, increasing from 15.9 to 36.6. These shifts could be explained by the fact that the timing of observed effects may vary across market segments too. Overall, these differences reinforces the importance of this test and considering temporal factors in policy

6 CONCLUSION

There is a growing interest in Citizenship by Investment and Residency by Investment programs from policymakers as tools to attract rapid foreign capital and stimulate economic growth, which they undoubtedly do. But at what cost? What impact do they have on the housing markets? This dissertation had the goal to measure the isolated causal impact of these programs on housing markets, with a closer examination of property prices, rental markets, and affordability conditions. By manually constructing a new cross-country dataset and employing the synthetic control method with staggered adoption, we were able to estimate credible scenarios simulating what would have happened had the country not implemented these programs, and, subsequently, quantify the actual effects of these programs across OECD countries.

Our findings reveal that Golden Visa programs contribute to a significant rise in housing prices and worsen affordability conditions, as evidenced by the increase in both the House Price Index and Price-to-Income ratio. In contrast, we find no statistically significant effects on rental prices or the Price-to-Rent ratio, yielding no valid results and concluding that either the model was unable to capture rental dynamics or rental markets may be less sensitive to the investment migration effects. Among the eight countries examined, Chile and Greece displayed the most notable housing price distortions, while Colombia showed indications of opposite trends. Additionally, the poor pre-treatment fit for France limits the reliability of inferences drawn. These findings serve as an important tool since they give emphasis to the heterogeneous nature of the impacts across countries and underscore the importance of country-specific analyses. Nevertheless, the overall picture is consistent with the existing literature that investment migration schemes may amplify housing market imbalances.

This research contributes to the debate in several ways. First, unlike earlier studies that were either descriptive or focused on a single country, we move beyond and conduct a cross-country analysis with a robust counterfactual design. Second, by adopting a GSCM framework, we account for both observable and unobservable heterogeneity, avoiding omitted-variable bias and the parallel-trends assumptions of standard panel and DiD models. Third, our study considers not only housing prices but also includes rents and affordability, offering a more complete overview.

Our findings also raise some policy implications. While investment migration can attract capital, it may be doing so at the loss of access to housing, especially for local residents. Policymakers should not ignore these trade-offs when designing or reforming such programs.

Though this study provides robust causal evidence, several limitations remain. Due to data availability, the focus on OECD countries may limit the generalisability of the results to developing economies where investment migration programmes are also prominent. Furthermore, conducting micro-level evaluations at the city or property level may also reveal localised effects. Another limitation is the variation in program design. Since not all Golden Visa schemes require real estate investment (some allow alternative contributions such as government bond purchases, job creation, or donations to state funds), it could be beneficial to disaggregate programs by type of investment and explore if this policy heterogeneity may influence the housing effects. Future research could also extend this analysis by exploring alternative econometric approaches to assess the impact.

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A Appendices

Country	Program Name	Source
Australia	BIIP Investors Streams	Migration Regulations 1994 Statutory Rules No. 268 1994 S. 2 Sc. 188
Chile	Investor Visa	Law No. 21.325 Migration and Aliens Act 2021
Colombia	Investor Visas	Art. 66 S. 3 of Resolution 6045 of 2017
Costa Rica	Investor Visa Program	Costa Rica Immigration Law 9996
France	Talent Passport - Business Investor	CESEDA November 2004
Greece	Golden Visa Program	Greek “Golden Visa” Law 4251/2014
Hungary	Guest Investor Program	T/6079/12
Ireland	Immigrant Investor Program	S.I. No. 258/2012 – Immigration Act 2004
Italy	Residence by Investment Program	Art. 26-bis, part. 1 of Legislative Decree 286/1998 (TUI)
Latvia	Residence by Investment Program	Immigration Law S. 23, Paragraph 1, Clause 28
Luxembourg	Residence for Investors from Third-Countries	Free Movement of Persons and Immigration Art. 53 quater of the modified law of 29 August 2008
Netherlands	Investor Visa Program	Aliens Act 2000 Part 3. Section 14
New Zealand	Active Investor Plus Visa	Immigration Act 2009 Part 3 Item 45
Portugal	Golden Visa Program	Art. 90-A, P. 2 of The Aliens Act, Art. 63, P. 1 and 3 and 65-E Of Decree N.84/07 Of 05/1
Spain	Golden Visa Program	Spanish Law 14/2013 of September 27
United Kingdom	Investor Visa	Immigration Rules 2021, Sec 245E to 245EF
United States	EB-5 Immigrant Investor Program	Aliens and Nationality Act Chapter 12: 1153: Allocation Of Immigrant Visas (5)

Table VI: Program’s Detailed Information

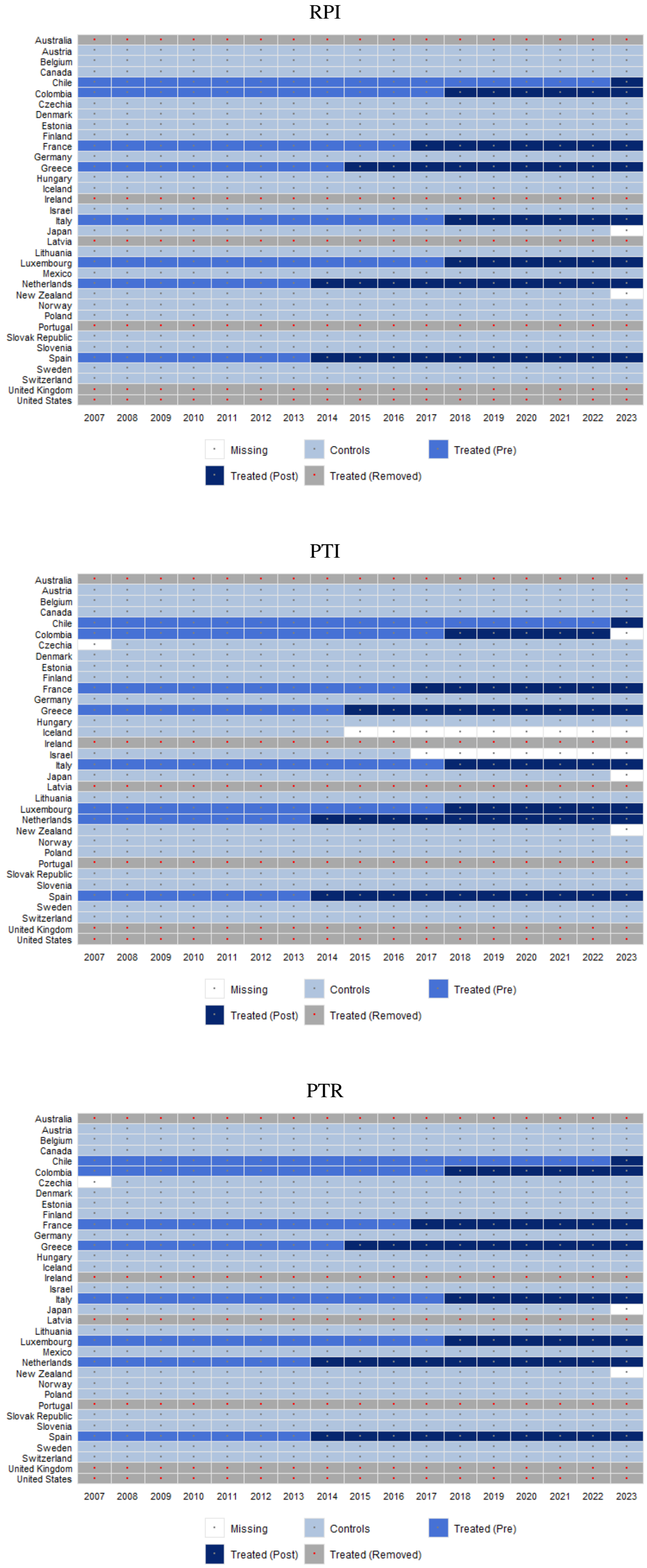

FIGURE 9: Panel Visualisation for RPI, PTI and PTR

Donor	Chile	Colombia	France	Greece	Italy	Luxembourg	Netherlands	Spain
Austria	0.193	0.231	0.112	−0.074	0.069	0.247	0.046	−0.060
Belgium	0.143	0.182	0.102	0.001	0.078	0.196	0.087	0.033
Canada	0.138	0.162	0.075	−0.070	0.041	0.173	0.017	−0.066
Czechia	−0.130	−0.162	−0.086	0.019	−0.062	−0.174	−0.061	−0.003
Denmark	−0.055	−0.065	−0.029	0.030	−0.016	−0.069	−0.005	0.029
Estonia	−0.236	−0.290	−0.149	0.054	−0.102	−0.311	−0.090	0.024
Finland	0.198	0.252	0.141	0.001	0.108	0.272	0.120	0.044
Germany	0.109	0.134	0.070	−0.020	0.049	0.144	0.046	−0.004
Hungary	−0.274	−0.343	−0.185	0.028	−0.135	−0.369	−0.138	−0.020
Iceland	−0.205	−0.259	−0.143	0.007	−0.108	−0.279	−0.117	−0.035
Israel	0.293	0.354	0.175	−0.098	0.112	0.379	0.084	−0.071
Japan	0.058	0.074	0.042	0.005	0.033	0.080	0.039	0.019
Lithuania	−0.424	−0.514	−0.258	0.129	−0.168	−0.551	−0.133	0.086
Mexico	0.041	0.053	0.030	0.001	0.023	0.057	0.026	0.011
New Zealand	0.038	0.036	0.005	−0.066	−0.011	0.037	−0.039	−0.082
Norway	0.252	0.309	0.159	−0.057	0.109	0.331	0.097	−0.024
Poland	−0.217	−0.256	−0.118	0.108	−0.066	−0.273	−0.029	0.101
Slovak Republic	−0.152	−0.184	−0.092	0.047	−0.060	−0.197	−0.047	0.032
Slovenia	−0.196	−0.235	−0.114	0.075	−0.070	−0.251	−0.047	0.060
Sweden	0.241	0.294	0.149	−0.067	0.098	0.315	0.082	−0.040
Switzerland	0.186	0.226	0.114	−0.053	0.075	0.242	0.062	−0.032

TABLE VII: HPI - Synthetic Control Weights by Treated Country

Donor	Chile	Colombia	France	Greece	Italy	Luxembourg	Netherlands	Spain
Austria	0.362	0.267	-0.172	-0.029	0.221	0.414	0.172	0.146
Belgium	-0.116	-0.078	0.036	0.094	-0.023	-0.038	-0.041	-0.025
Canada	-0.076	-0.058	0.041	-0.016	-0.058	-0.111	-0.040	-0.036
Czechia	0.130	0.116	-0.113	0.223	0.209	0.406	0.101	0.112
Denmark	-0.128	-0.072	0.006	0.263	0.063	0.133	-0.019	0.013
Estonia	0.406	0.246	-0.061	-0.638	-0.090	-0.206	0.091	0.009
Finland	0.140	0.111	-0.087	0.085	0.139	0.266	0.082	0.081
Germany	-0.199	-0.150	0.103	-0.025	-0.144	-0.272	-0.101	-0.091
Hungary	0.220	0.153	-0.081	-0.122	0.076	0.135	0.086	0.062
Iceland	0.682	0.524	-0.377	0.193	0.553	1.051	0.364	0.338
Israel	-0.116	-0.071	0.018	0.181	0.025	0.057	-0.026	-0.003
Japan	-0.485	-0.344	0.198	0.187	-0.213	-0.389	-0.205	-0.158
Lithuania	0.169	0.080	0.031	-0.522	-0.180	-0.370	-0.005	-0.062
Mexico	0.218	0.156	-0.093	-0.063	0.107	0.198	0.096	0.076
New Zealand	-0.079	-0.062	0.046	-0.031	-0.069	-0.131	-0.044	-0.041
Norway	0.106	0.093	-0.087	0.162	0.159	0.309	0.079	0.086
Poland	-0.168	-0.142	0.126	-0.197	-0.220	-0.424	-0.115	-0.122
Slovak Republic	-0.476	-0.341	0.203	0.143	-0.231	-0.426	-0.208	-0.165
Slovenia	-0.105	-0.096	0.098	-0.212	-0.187	-0.363	-0.086	-0.099
Sweden	-0.154	-0.104	0.049	0.123	-0.033	-0.054	-0.055	-0.034
Switzerland	-0.329	-0.227	0.118	0.201	-0.103	-0.182	-0.126	-0.088

TABLE VIII: RPI - Synthetic Control Weights by Treated Country

Donor	Chile	Colombia	France	Greece	Italy	Luxembourg	Netherlands	Spain
Austria	0.490	0.425	0.329	0.229	0.310	0.044	−0.068	−0.357
Belgium	0.309	0.283	0.217	0.176	0.231	0.020	0.000	−0.152
Canada	0.266	0.214	0.185	0.148	0.142	0.027	−0.075	−0.177
Czechia	−0.082	0.116	−0.118	−0.281	0.241	−0.041	0.428	−0.100
Denmark	−0.385	−0.369	−0.257	−0.172	−0.307	−0.024	−0.031	0.248
Estonia	−0.718	−1.171	−0.341	0.217	−1.354	0.051	−1.155	0.722
Finland	0.160	0.145	0.123	0.130	0.128	0.007	0.007	−0.015
Germany	0.382	0.368	0.255	0.171	0.310	0.023	0.038	−0.241
Hungary	−0.433	−0.010	−0.411	−0.666	0.304	−0.106	0.879	0.024
Iceland	0.016	−0.036	0.025	0.065	−0.070	0.011	−0.114	0.019
Israel	0.829	0.599	0.613	0.600	0.350	0.090	−0.371	−0.406
Japan	0.387	0.081	0.355	0.546	−0.147	0.077	−0.614	−0.008
Lithuania	−0.978	−0.988	−0.638	−0.377	−0.861	−0.052	−0.195	0.662
New Zealand	0.154	0.048	0.119	0.134	−0.049	0.034	−0.222	−0.122
Norway	0.193	0.122	0.149	0.163	0.054	0.024	−0.126	−0.080
Poland	−0.804	−0.372	−0.642	−0.773	0.009	−0.133	0.843	0.353
Slovak Republic	−0.221	−0.016	−0.204	−0.317	0.141	−0.053	0.427	0.037
Slovenia	−0.373	0.018	−0.355	−0.579	0.310	−0.099	0.822	0.047
Sweden	0.414	0.215	0.326	0.380	0.035	0.063	−0.380	−0.181
Switzerland	0.395	0.327	0.271	0.206	0.225	0.038	−0.090	−0.273

TABLE IX: PTR - Synthetic Control Weights by Treated Country

Donor	Chile	Colombia	France	Greece	Italy	Luxembourg	Netherlands	Spain
Austria	0.243	0.226	0.226	−0.319	0.177	0.065	0.021	0.088
Belgium	0.297	0.355	0.144	−0.131	0.160	0.076	0.005	0.178
Canada	0.335	0.415	−0.184	0.341	−0.068	0.062	0.066	−0.204
Czechia	−0.628	−0.822	−0.288	0.387	−0.252	−0.209	0.289	−0.478
Denmark	−0.293	−0.203	−0.255	0.293	−0.238	−0.041	−0.246	0.069
Estonia	−0.111	−0.232	0.387	−0.581	0.252	−0.020	0.060	0.233
Finland	0.378	0.502	0.339	−0.418	0.297	0.130	−0.165	0.538
Germany	0.368	0.423	−0.032	0.173	0.068	0.063	0.140	−0.076
Hungary	−0.380	−0.608	−0.307	0.524	−0.191	−0.192	0.544	−0.700
Iceland	−0.854	−0.810	−0.410	0.251	−0.525	−0.128	−0.563	−0.010
Israel	0.425	0.447	0.260	−0.417	0.185	0.128	−0.089	0.093
Japan	−0.082	0.302	−0.369	0.304	−0.392	0.114	−0.915	0.486
Lithuania	−1.001	−1.150	0.581	−1.297	0.098	−0.104	−0.671	0.774
Mexico	0.312	0.314	−0.034	0.263	0.111	0.016	0.339	−0.137
New Zealand	0.108	0.043	−0.115	0.169	−0.071	−0.007	0.179	−0.381
Norway	0.246	0.326	0.088	−0.159	0.058	0.091	−0.169	0.147
Poland	−0.120	−0.109	−0.073	0.226	0.006	−0.055	0.132	0.067
Slovak Republic	0.036	−0.188	−0.168	0.478	0.017	−0.125	0.774	−0.708
Slovenia	0.096	−0.033	0.261	−0.210	0.281	−0.032	0.395	0.009
Sweden	0.398	0.560	−0.243	0.423	−0.108	0.095	−0.060	−0.105
Switzerland	0.228	0.240	0.192	−0.300	0.135	0.074	−0.065	0.116

TABLE X: PTR - Synthetic Control Weights by Treated Country

Event Time	HPI ATT Est.	RPI ATT Est.	PTI ATT Est.	PTR ATT Est.
$t_0 - 5$	-0.501	-0.598	0.305	-0.147
$t_0 - 4$	0.249	-0.698	0.364	0.729
$t_0 - 3$	2.345	0.792	1.530	0.750
$t_0 - 2$	0.098	1.752	0.263	-0.208
$t_0 - 1$	-0.92	0.390	0.599	0.440
t_0	-0.718	-0.843	-2.181	-0.699
$t_0 + 1$	5.955	-0.757	5.397	1.020
$t_0 + 2$	14.733	-1.502	13.278	4.608
$t_0 + 3$	27.222	-3.420	24.680	8.663
$t_0 + 4$	36.440	-5.633	32.542	11.477
$t_0 + 5$	43.031	-5.268	35.821	12.067

TABLE XI: Average Treatment Effects on the Treated (Event Time)