

The Impact of Post-9/11 Visa Policies on Travel to the United States*

Brent Neiman[†]
University of Chicago
Booth School of Business

Phillip Swagel[‡]

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Abstract

American and foreign businesses, politicians, and media have all pointed to post-9/11 changes in visa policies as being responsible for the sharp decline in travel to the United States following the attacks. Using an empirical model which distinguishes the impact of visa policy from economic and country-specific factors, we find that changes in visa policy were not important contributors to the decrease in travel to the United States. Rather, the reduction in entries was largest among travelers who were not required to obtain a visa.

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[†]Brent Neiman, Brent.Neiman@ChicagoBooth.edu

[‡]Phillip Swagel, Phillip.Swagel@ChicagoBooth.edu

1 Introduction

The number of business and leisure travelers arriving in the United States dropped sharply following the terror attacks of September 11, 2001. The number of non-immigrant visitors fell by more than 17 percent for the period October 2002 to September 2003 (the government's fiscal year 2003) compared to the number of visitors in fiscal year 2000, and it took until fiscal 2007 for travel to the United States to meaningfully surpass its previous high level (Figure 1).¹ This paper examines whether changes in visa policy, which applied only to visitors from certain countries, were the key contributors to the decline in short-term travel to the United States, or whether economic, psychological, or other factors such as more stringent airport security had a larger impact on travel.²

In the wake of the attacks, the U.S. government enhanced a wide range of border security policies. Steps taken since 2001 include both visible changes in security at airports and other entry points that affect all visitors, as well as changes in visa policies that affect only travelers who require a visa to enter the United States. The changes to visa policy were not surprising in the wake of the attacks, since the 9/11 terrorists entered the United States with legitimate visas.

Commentators in both the public and private sectors have claimed that tougher visa policies are an important factor behind the decline in travel to the United States, and expressed concern that post-9/11 visa policies strain relationships with customers of U.S. firms and hinder opportunities for new business. A recent article on the decline in business travel to the United States after 9/11, published by the *International Herald Tribune* under the headline of "Fortress America' visa system scaring businesses away," referenced an estimate by the National Foreign Trade Council that tougher entry rules cost U.S. businesses \$31 billion from 2002 to 2004.³ In late 2006, the *London Times* estimated the cost of the decline

¹Cyclical components are removed from all graphs labeled "seasonally adjusted" by adjusting the raw series by the coefficients from regressions of the log series on month dummies, though the short time series of the data make this procedure imperfect.

²We emphasize that our results apply to non-immigrant entries on the whole, rather than to particular groups such as foreigners seeking to work or study in the United States.

³Bliss, Jeff and John Hughes, 2006. "'Fortress America' visa system scaring business away," *International Herald Tribune*. December 26.

in foreign tourists coming to the United States since 9/11 at \$286 billion, and ties a large share of this cost to “the stricter security introduced since September 2001.”⁴ A Federal Reserve Bank of Dallas publication highlights companies’ concerns about the new post-9/11 visa requirements (Orrenius 2003). Foreign officials similarly have voiced complaints about post-9/11 changes in visa policy.⁵

This paper provides empirical evidence on the impact of changes in visa policy in contributing to the decline in temporary visitors to the United States. We are aware of no recent economic analysis in the research literature that examines the impact of economic, geopolitical, and security-related factors on business and leisure travel.⁶ To distinguish the effects of visa policy from other security, economic, and psychological factors, we compare the impact of the 9/11 attacks on travel by visitors who require a visa to enter the United States with the impact on visitors who do not. To do this, we group countries by their participation in the visa waiver program, a section of the U.S. legal code under which citizens of 35 countries (as of early 2009) are allowed to visit the United States temporarily without a visa. Table 1 lists the countries that have at one time been included in the visa waiver program (two have been removed). Citizens in these countries are deemed unlikely to pose a security threat and likely to leave after their visit in line with immigration rules.

In 2003, entries under this program represented roughly half of all overseas visitors to the United States (Siskin 2004). All travelers are subject to routine security restrictions such as examination of their passports and luggage upon arrival into the United States, but only visitors from countries that do not participate in the visa waiver program are affected by changes in non-immigrant visa policy. This difference in treatment provides a policy-induced variation by which to assess the impact of changes in the visa regime that took place in the wake of the 9/11 attacks.

The empirical results suggest that stricter visa policy did not play a salient role in reducing

⁴Kay, William, (2006). "Tourism slump worries U.S.," *London Times*. September 17.

⁵This assertion received attention in Congress as well, including at a House Government Reform Committee hearing in 2006, where cello virtuoso Yo-Yo Ma testified that visa policies were stifling cultural interchange.

⁶There is a large literature on the economic influences of permanent immigration patterns. We also note that Orrenius (2003) analyzes related issues for a general audience outside of a research framework.

travel to the United States in the two years following the September 11 attacks. After taking into account economic effects, non-immigrant entries from countries requiring a visa did not fall by more after 9/11 than visits by people not needing a visa. In fact, in the majority of our specifications, the results indicate just the opposite – the decline was larger for travelers from countries whose citizens do not require a visa to enter the United States, and in many specifications, significantly larger. Several factors could explain this large post-attack decline in visitors who did not require a visa, including a larger psychological impact or the disproportionate incremental cost from increases in general security to those travelers that previously entered the country without any hassles at all.

2 Border Security Policy

Airport security changed substantially following 9/11, with passengers traveling to the United States facing much greater scrutiny than before the attacks. Airlines are required to send passenger lists in advance, exclusions lists are more vigorously maintained (though still by no means error-free), and non-citizens are required to provide digital fingerprints and have their photograph taken on arrival. Potential travelers requiring a visa are also affected by changes in consular procedures and higher fees that make the visa process more arduous and expensive, and visa approval now takes longer than before 2001. Other security procedures affect all visitors, regardless of whether or not they need a visa, such as the heightened airport security and more involved screening for U.S.-bound flights that make travel less pleasant. Raising the cost of boarding a plane and obtaining a visa would be expected, on the margin, to discourage foreigners from undertaking business or leisure travel to the United States.

While some of the changes in visa policy are easily identifiable and were broadly/fully implemented on a specific date, others have not been publicly disclosed or were implemented over time, including being applied differently in different regions. Many of the changes, however, were instituted shortly after the terrorist attacks. A State Department report notes that "the post-September 11, 2001 era witnessed immediate efforts to effect dramatic changes in CA's [Consular Affairs'] direction of the visa process" (Office of the Inspector

General 2004).

Foreigners now pay more to apply for a visa to visit the United States, as visa fees rose from \$45 before September 11 to \$65 in June 2002 and to \$100 in November 2002 (Rose 2004). Further, the time it takes to get approved for a visa increased for many applicants. Shortly after the attacks, the Department of Justice requested that two new name check procedures be added to the visa application process, requiring 20 and 30 days each. The 30-day check under the so-called “Visa Condor” program applies to visa applicants from a list of countries that is classified for national security reasons (GAO 2002). In early 2002, American consulates began to collect a supplemental application form from male visa applicants aged 16 to 45 from every country. Visa applicants from certain countries must now undergo an in-person interview at a U.S. embassy or consulate.

These changes in visa policy, however, do not impact visitors from all foreign countries because some visitors do not require a visa in the first place. The Immigration Reform and Control Act of 1986 created the Visa Waiver Program to facilitate the entry of temporary visitors from countries whose citizens were perceived as particularly unlikely to threaten U.S. national security. Under the program, foreign nationals from participating countries are able to enter the United States as temporary visitors for up to 90 days without obtaining a visa (nations participating in the visa waiver program must extend reciprocal treatment to Americans). Visitors are still checked against an exclusion list and must provide proof of a return ticket out of the United States and adequate financial resources for their stay. A small number of visitors from visa waiver countries are required to obtain non-immigrant visas (for example, travelers wishing to stay in the United States on a temporary basis for more than 90 days, exchange visitors, intracompany transferees, visitors with criminal backgrounds, and anyone who was previously denied a visa), and occasionally foreigners unaware of their eligibility for the visa waiver program unnecessarily apply for a visa. These travelers, however, account for less than 15 percent of the non-immigrant entries from most visa waiver countries in recent years. Based on this, we assume in the regression analysis that all visitors from visa waiver countries are without a visa, while all entrants who are citizens of countries not included in the visa waiver program require one.

Refusal rates for visa applicants did not change significantly in the wake of the attacks. Though the refusal rate reached 35.1 percent in fiscal year 2002 (following the attacks), the 31.7 percent rejection rate in fiscal 2003 was below the rate during fiscal 2001 (Clemens 2004). These variations in visa rejection rates explain only a small portion of the 17 percent drop in the total number of entries: the modestly higher rejection rate meant that several hundred thousand more visa applications were refused in the year after the attack than in the year before it, but the total number of entries fell by several million.

3 Influences on Business and Leisure Travel

A change in visa policy was only one of several factors affecting foreign travel to the United States. For instance, the 9/11 attacks surely had a significant “psychological” impact in making people reluctant to fly in the aftermath of the attacks. This is consistent with the sharply reduced airplane travel within the United States, a finding that is independent of visa-related security measures. According to Department of Transportation data, the number of passengers on domestic non-stop segments in the United States was 11 percent lower in fiscal 2002 than in fiscal 2000. Economic factors affect travel as well. Strong U.S. and foreign growth would likely increase the opportunities for business and leisure travel to the United States, and exchange rate fluctuations change the effective cost, denominated in foreign consumption units, for foreigners considering travel to the United States. Finally, the onset of the war in Iraq appears to have led to a drop-off in travel in early 2003 as potential visitors awaited the outcome of the conflict.

There has been limited research on the economic factors that influence business and leisure travel. Rhomberg and Boissonneault (1964) and Gray (1966) estimate elasticities of demand for spending by international travelers with respect to national and per capita income and exchange rates and find that the response of spending to these factors is statistically significant. Kwack (1971) likewise shows that spending abroad by travelers responds substantially to changes in national incomes and real exchange rates. More recently, Di Matteo (1993) and Vilasuso and Menz (1998) find that national income and the exchange

rate are the key determinants of spending by Canadians traveling in the United States.

4 Non-immigrant Entry Data

The econometric analysis is carried out on a panel of monthly data on non-immigrant entries to the United States that was obtained from the U.S. Department of Homeland Security (DHS). The data include I-94 admissions only, meaning it excludes the majority of short-term land-border entries from Canada and Mexico. We focus on the I-94 data because the large number of Canadian and Mexican entries would swamp non-immigration trends from the rest of the world that are more relevant to visa policy. Further, the motivating factors for crossing a land border may differ from the factors behind longer-distance travel of nationals beyond the immediate U.S. neighbors.

These unpublished data are compiled by DHS's Office of Immigration Statistics and include the monthly number of entries for fiscal years 1996 and 1998 to 2003.⁷ There are some gaps in the data, with only annual figures available for 1995 and no data at all for 1997. We have not been able to obtain monthly data for years after 2003. The data include the number of non-immigrant entries, a category which includes temporary business and pleasure visitors and short-term residents such as students and temporary workers. The temporary business and pleasure travelers constitute about 90 percent of total entrants during the years in our data, though this share is higher for visa waiver countries than non-visa waiver countries. While it would be desirable to separate out the various classes of non-immigrant entries in our analyses, such data are not available to us at the monthly frequency. The entries are classified by country of citizenship. A French national arriving in the United States, for example, is counted as French regardless of whether he or she resides in France and regardless of the port of embarkation.

We drop countries lacking economic data such as measures of output and inflation on at least a quarterly frequency. The resulting sample includes 65 countries and covers over

⁷Most of these data have been aggregated and published in the Yearbook of Immigration Statistics (Office of Immigration Statistics, 2005), but the monthly statistics on country-by-country entries are not published separately (the unpublished data we use are from DHS Table 607).

86 percent of all entries in fiscal year 2003. Our sample contains 23 countries that were participants in the visa waiver program at some point up to 2003 – a few countries move in or out of the visa waiver program during the years in our sample.⁸ We have tested our results with a constant panel, omitting these countries as well as others that enter the dataset after 1995. This leaves out many observations, but has little qualitative impact on our results.

We further exclude three countries from our primary specifications, though full results with their inclusion do not change our basic conclusions and are available on request. First, we exclude Argentina because the very close timing of its economic and political crisis, its removal from the visa waiver program, and the 9/11 attacks potentially pose problems for our identification strategy. We also exclude Canadian and Mexican entrants because the I-94 data exclude land-border crossings and hence capture only the small share of entrants from those nations who arrive from another country or via air travel. Further, the number of Mexican visitors requiring visas is extremely large relative to the number of other entries that require visas, so it has the potential to swamp developments in other countries.

The declines in entries from fiscal year 2000 to 2003 are listed for the countries in our dataset in the final column of Panel A of Table 2 and shown in Figure 2, with the vertical axis giving the percentage change in entries over this period.⁹ The horizontal axis indexes each of the countries by their level of PPP-adjusted per capita GDP in 2000, with the poorest countries on the left of the figure and the richest toward the right. Finally, each country is represented by a circle which has its diameter sized in proportion to the number of entries in 2000 and which is shaded for countries outside the visa waiver program. As would be expected, the visa waiver countries are generally richer and tend to have a larger number of entrants than non-visa waiver countries. Consistent with the data in Table 2, the mass of countries unaffected by changes in visa policy seems to be lower in the plot (closer to the x-axis), suggesting a more pronounced decline in entries after 9/11. There is significant heterogeneity in the plot, however, and each group contains some large countries with big

⁸Argentina was removed, while Australia, Slovenia, Portugal, and Singapore were added.

⁹Figure 2 excludes Argentina, Canada, and Mexico. It also excludes Macau due to lack of data on its PPP-adjusted per capita GDP.

declines and others that remained relatively unaffected.

Our sample of countries includes a high percentage of total entries from visa waiver countries, which implies that the 22 percent drop in entries from these countries in our data is close to the decline in the aggregate data for all visa waiver countries (that is, including countries not in our sample). After subtracting out Argentina, Canada, and Mexico, total entries dropped 20 percent. Combined with the fact that visa waiver countries accounted for about 65 percent of all non-excluded entries over this period, this implies that the aggregate drop from all non-visa waiver countries was about 16 percent – a similar, but somewhat steeper, decline than the 11 percent listed in Table 2 for our sample. Further, given that our selection criteria is the availability of economic data, non-visa waiver countries excluded from our dataset are likely to be countries that most differ from the visa waiver countries in terms of their level of development. We view these summary statistics as suggesting our dataset is representative of entries from both visa waiver and non-visa waiver countries, particularly for the purposes of our difference-in-differences estimates.

Figure 3 plots (in levels and log levels) seasonally adjusted entry data from a constant panel of all countries whose visa waiver program status did not change from 1996 on (including those for which we have no economic data). The figure indicates that both the trend growth before 9/11 and the subsequent response to the attacks are remarkably similar for citizens of the two groups of countries. Consistent with the summary statistics in Table 2, travel from countries not requiring visas shows, if anything, a more negative response. Hence, just from looking at these data, one might conclude that the reduction in business and pleasure travel resulted from the psychological or economic effects of the terrorist attacks that were common to all countries, or from general security measures impacting all travelers rather than from changes in visa policy.

5 Specification

While these plots and summary statistics are suggestive, a multivariate regression framework is required to distinguish the influence of visa policies from economic and other factors

impacting travel decisions. Following the derivation of the gravity model for trade in Anderson and Van Wincoop (2003), we motivate our empirical work by considering a sub-utility function where consumers in foreign country i have identical homothetic CES preferences over their travel to other countries j , $Entries_{ij}$.

These consumers maximize:

$$\left(\sum_j \left(\delta_{ij} e^{-(\chi + V_{ij} + V_{ij}^\chi)} \right)^{1/\sigma} Entries_{ij}^{(\sigma-1)/\sigma} \right)^{\sigma/(\sigma-1)}$$

subject to the budget constraint

$$\sum_j s_{ij} p_j Entries_{ij} = \alpha_i GDP_i^N,$$

where δ_{ij} is a log-normally distributed taste shock for travel from country i to j and GDP_i^N denotes nominal GDP for country i . (Note that our empirical analysis will only include entries to the United States, or the case where $j = U.S.$) χ indexes the additional costs from post-9/11 security changes unrelated to visa policy, V_{ij} indexes the "hassle factor" from the original requirements for obtaining a visa to travel to j , and V_{ij}^χ indexes the additional costs of obtaining a visa imposed after the attacks. For all countries j , $\chi = 0$ before 9/11 and $\chi > 0$ thereafter. For those countries k that require a visa from the citizens of i , $V_{ik} > V_{ik}^\chi = 0$ before the attacks, and $V_{ik}, V_{ik}^\chi > 0$ after 9/11. For countries l that have a visa waiver program with i , $V_{il} = V_{il}^\chi = 0$. α_i is a constant travel expenditure share parameter that would emerge if, for example, the travel and other sub-utility functions were aggregated in Cobb-Douglas consumer preferences. s_{ij} is the nominal exchange rate (in i 's currency per unit of j 's currency), and p_j is the j -currency denominated price of travel to country j .

These preferences generate the demand function:

$$Entries_{ij} = \alpha \delta_{ij} \left(\frac{s_{ij} p_j}{P_i} \right)^{-\sigma} e^{-\chi} e^{-(V_{ij} + V_{ij}^\chi)} \frac{GDP_i^N}{P_i} \quad (1)$$

where P_i is the standard i -currency denominated CES price index of travel expenditures to

the rest of the world and where the unobserved preference shock, δ_{ij} , enters multiplicatively. We assume that p_j and P_i are equal to the CPIs in countries j and i , respectively, assume supply is perfectly elastic, and take logs to derive the basis for our estimation:

$$\ln(\text{Entries}_{ij}) = \ln \alpha_i - \chi - V_{ij} + \ln GDP_i - \sigma \ln RER_{ij} - V_{ij}^\chi + \ln \delta_{ij}, \quad (2)$$

where RER_{ij} is the bilateral real exchange rate and GDP_i is real output. Log entries to all countries j , whether visas are needed or not, are reduced by χ when comparing post-9/11 to pre-9/11, and log entries to countries requiring a visa are reduced by V_{ij} before 9/11 compared to those countries not requiring a visa and before 9/11. The parameter we will focus on in the empirical sections below is V_{ij}^χ , which determines the post-9/11 drop in log entries that is specifically related to visa policy changes after 9/11.

Our empirical implementation also allows for monthly fixed effects (and hence will subsume χ). In addition to security-related changes, these time fixed effects are meant to capture, for example, global changes in the price of oil or other common factors impacting travel demand across all countries. We restrict $j = U.S.$ and estimate (2) in our panel by specifying the number of entries from i at time t as:

$$\ln(\text{Entries}_{it}) = \beta_1 \text{Country}_i + \beta_2 \text{Time}_t + \psi \begin{bmatrix} VWP \\ \ln GDP \\ \ln RER \end{bmatrix}_{it} + \beta_3 (VWP * \text{Post} - 9/11)_{it} + \varepsilon_{it}. \quad (3)$$

The coefficient β_3 is an estimate of V_{ij}^χ , the opposite of the elasticity of entries to additional visa costs added after 9/11. Intuitively, this parameter captures the additional impact of post-9/11 changes on entries from visa waiver countries compared to non-visa waiver countries. If δ_{it} is log-normally distributed as assumed, then $\varepsilon_{it} = \ln(\delta_{it})$ will be normally distributed, and the coefficients can be consistently estimated with OLS.

Data on real GDP, inflation, and nominal exchange rates are from the IMF's International Financial Statistics (IFS) database. The economic and indicator variables are at a monthly frequency; for real GDP, we linearly interpolate quarterly data to obtain monthly

values.¹⁰ The real exchange rate, RER, is calculated using the nominal exchange rate (in foreign currency per dollar) and U.S. and foreign consumer price inflation; a larger value thus represents a real appreciation of the U.S. dollar. The VWP variable equals one for countries participating in the visa waiver program and zero otherwise. Since the specification includes country fixed effects, the VWP coefficient by itself is estimated entirely from time series variation in countries that transition in or out of the program (but, again, we are interested in the interaction of this coefficient with the post-9/11 indicator, not the VWP variable by itself). The specification uses the log of the number of entries along with country fixed effects, so it matches changes in entrants in percentage terms against percent changes in economic variables. As such, the coefficients on economic variables provide an easily interpreted elasticity.

Estimating with the log of entries is problematic in that it gives the same weight in the regression to a 10 percent change in visits from a country with many travelers such as Japan as it does a 10 percent change in visits from a country with relatively few entries such as Hungary. Since our goal is to understand the relationship between changes in visa policy and the number of non-immigrant entrants to the United States, in our baseline specification we weight the regressions by the number of entrants for each country in fiscal year 1996, which is the first year in our dataset.¹¹ This approach makes sense when one is concerned about heterogeneity in the true underlying relationships across countries and wants to put more weight on the estimated relationships for the larger countries (that is, to give more weight to the GDP elasticity of the many visits from Japan than the few visit from Hungary). Estimating without weights, on the other hand, makes sense if the source of error in the estimates is a country-month specific unobservable shock because each observation is treated as yielding the same amount of information about the impact on visa policies. We consider the weighted regressions more relevant to this analysis, but report both weighted

¹⁰ Australia's CPI is only available quarterly in some years and is linearly interpolated to obtain monthly values.

¹¹ Using weights that are in any way related to the independent variable can be problematic, particularly if there is a time-series element to the weights that allows them to be correlated to the error term in the regressions. Here, we worry less about this because the empirical specification is in percent changes at a monthly frequency, and hence the error term is unlikely to be correlated to the weights, which are the levels of entrants in only the first year of our dataset.

and unweighted results for all specifications.¹²

In all of the regressions, we assume that the causality runs from macroeconomic variables of GDP growth and the exchange rate to the number of entries, and that the number of visits from any one country is not large enough to affect aggregate growth rates or currency markets. Even the high water mark of roughly 3.5 million temporary visits from all countries in July 2000 is modest compared to the U.S. population or labor force, and the spending of these visitors and thus their impact on the exchange value of the dollar is dwarfed by the trillions of dollars in daily turnover in foreign exchange markets.

The “tough visa hypothesis” would predict a quantitatively large and statistically significant positive coefficient on the interaction term β_3 , suggesting that stricter visa policies applying only to non-VWP entries affected the level of entries. In fact, the majority of our results generate a negative coefficient on this interaction term, suggesting that the decline in travel to the United States after 9/11 was at least as pronounced for visitors from countries whose citizens did not require a visa. This negative coefficient is statistically significant in many specifications, including both weighted and unweighted regressions. To be sure, we do not interpret this negative coefficient to mean that stricter visa policy led to increased entries from affected countries – it is not reasonable to believe that the demand for travel increases when it becomes more costly. Rather, we are suggesting that the factors behind the decline in visits to the United States were either unrelated or negatively correlated to the tightening in visa policies after September 11.

6 Regression Results

The left panel of Table 3 provides results from our least squares regression (3) on the influences of the number of entries to the United States. Column (1) shows the baseline specification in which we weight by the number of entries in 1996, and column (2) shows the specification with no weighting. In addition, we include results for specifications that include country-specific linear trends in columns (3) and (4). We consider these specifica-

¹²Similar results are obtained when using alternative weights including population, PPP-adjusted per capita GDP levels, and bilateral trade with the United States in 1995.

tions less meaningful, since the overall pattern of entries shown in Figure 3 does not suggest important differences between the groups in terms of long-term trends and the limited time series is not sufficient to precisely estimate a stable differential trend between the two groups of countries. Further, since countries appear in our dataset over different periods of time and we are entirely missing the data for fiscal 1997, these trends may impose unrealistically large differences in country growth rates. Standard errors in all specifications are robust to heteroskedasticity and are clustered by country to control for serial correlation within each country.¹³

None of these baseline four specifications show a positive and statistically significant interaction term (the “difference-in-differences” coefficient), as would be expected if visa policies were an important contributor to the drop in entries. If anything, the results suggest the opposite. The interaction coefficient is negative in three of the four specifications and significant at the 10 percent level in two of these three. For example, the interaction coefficient in column (1) suggests that, after taking into account country-specific economic variables and other common influences, countries in the visa waiver program on average had a 12 percentage point larger decline in visitors to the United States than countries for which visa policy would be expected to matter. The lone positive interaction coefficient, in column (4), is very small quantitatively and not statistically significant. These results imply that changed visa policies were not a particularly important driver of reduced entries, or even that unobserved factors contributing to the drop in travel to the United States are negatively correlated with changes in visa policies. This could be the case, for example, if the increased “fear of flying” after 9/11 had a larger impact on potential travelers from advanced economies who did not need a visa to enter the United States than on travelers from countries whose citizens faced tighter visa policy.

Other coefficients are consistent with the existing literature and what one would expect on the determinants of travel spending. Stronger GDP growth in each home country is

¹³Bertrand et al. (2004) discusses how positive serial correlation can lead to underestimated standard errors on differences-in-differences interaction coefficients. While clustering at the group level alleviates the problem, their critique makes our result that there are no positive and significant coefficients in our specifications even more striking.

always associated with increased travel to the United States, and the elasticity in some specifications is large. The coefficients on the real exchange rate are uniformly negative as expected, implying that a stronger dollar (a real depreciation of the currency of the origin country) leads to less travel to the United States (an import of U.S. services) because dollar-denominated costs – including the fee to apply for a visa – become more expensive for foreigners. The positive coefficient on log GDP and the negative coefficient on log exchange rates are statistically significant across most estimates. The visa waiver dummy is positive in most specifications, as expected, but is not statistically significant. This is not surprising given that it is identified entirely from time series variation in the few countries that enter the visa waiver program in the middle of our dataset.

Santos Silva and Tenreyro (2006) argue that estimates of equations in the form of (3) are often biased and result in misleading interpretations of parameter values as elasticities. If our assumption of log-normality of δ_{ij} does not hold, for instance, and there were heteroskedasticity in the levels equation (1), the elasticity estimates will not be consistent. To generate consistent estimates under a broader set of distributions for δ_{ij} , they propose Poisson pseudo maximum likelihood (PPML) estimation of models in their multiplicative, or levels, form.^{14,15}

The middle panel of Table 3 with columns labeled (5) through (8) uses PPML to estimate equation (3) in levels form.¹⁶ Estimates for the elasticity of entries with respect to log foreign GDP and log real exchange rate growth are of the same sign and generally of a similar magnitude as in the least squares estimation. Focusing on the key interaction coefficient, we find a negative estimate for all four specifications, two of which are statistically significant at the 5 percent level. This suggests that the key result we emphasize – that entries from countries unaffected by visa policy dropped after 9/11 by at least as much as entries impacted

¹⁴An additional benefit of this technique, often used with count data, is its ability to handle “zeros” in the independent variable, a frequent occurrence in bilateral trade data. The logarithm of zero cannot be taken, so OLS estimates in log-linear form generally discard such observations. This is not relevant for us, since there are no months with zero entries in our dataset (the smallest number of entrants in any month is 16, from Macau).

¹⁵Head and Ries (2008) use this procedure to estimate bilateral FDI stocks.

¹⁶Though we estimate with PPML, we are not necessarily assuming a poisson distribution. Rather, the first order conditions defining the estimator gives consistent results under a broad range of error term distributions. See equation (9) in Santos Silva and Tenreyro (2006).

by policy changes – is not the result of heteroskedasticity in the multiplicative error term of a model for entries that is expressed in levels but estimated in logs. In fact, these alternative estimates increase the magnitude and statistical significance of the negative coefficients in three of the four specifications.

As applied to our data, the efficiency of PPML is based on the assumption that the conditional variance in the number of entries is proportional to the conditional mean number of entries. Following Head, Mayer, and Ries (2007), we also consider the Gamma quasi maximum likelihood estimator, which is more appropriate under an alternative assumption that the conditional standard deviation in entries, rather than the variance, is proportional to the conditional mean. Columns (8) through (12) in the right panel of Table 3 report results from these Gamma regressions. These estimates are similar to those in the left panel, both qualitatively and quantitatively, with the key interaction coefficient always within one percentage point of the baseline estimates.

In sum, our baseline results are either unchanged or strengthened by use of alternative estimators that consider differing distributions for the error term. As such, we revert below to using the baseline OLS specification and further explore the robustness of our results.

7 Robustness

Our findings do not support the assertion that post-9/11 changes in visa policy had a quantitatively important impact on travel to the United States. It should be kept in mind, however that our empirical approach is affected by a number of statistical issues that affect difference-in-differences regression specifications. An ideal setup for such analyses would involve a truly random selection of the “treatment” group – in our case, the group of countries not in the visa waiver program whose travelers were thus affected by changes in visa rules. In this section, we report results from regressions on subsets of the countries, from including additional conditioning variables, and results using another related dataset with somewhat different coverage. All these results also fail to produce a positive and significant coefficient on the interaction of 9/11 and the visa waiver program.

First, we trim the dataset to create a more comparable set of countries and reduce the possibility of treatment selection bias. In principle, to identify the impact of tougher visa policies, one would want to directly observe a large set of outcomes when the set receiving treatment (the tougher visa policy) is randomly generated. Selection into the visa waiver program, however, is clearly not random. For instance, the poorest country in the dataset would be a highly unlikely addition to the visa waiver program in the same way that it would be unusual for the richest country to be excluded. One might assume, however, that the probability of selection reflects a vector of observable covariates, \tilde{X} , and that conditional on these determinants of selection and before 9/11, the countries outside the visa waiver program are not systematically different from those in it.

We start with the assumption that $\tilde{X}_i = \{GDP_i\}$ because, as mentioned above, there is a clear positive relationship between levels of economic development and participation in the visa waiver country. Hence, we estimate specifications that include only those countries with purchasing-power-parity (PPP) adjusted per capita GDP levels in 2000 ranging from 39 to 66 percent of the per capita GDP of the United States. This includes countries with per capita incomes from the Czech Republic to Finland, inclusive. This is among the only ranges in the data for which there are comparable numbers of visa waiver and non-visa waiver countries. Table 4 lists the 12 countries falling in this range, organized by membership in the visa waiver program (5 in the program, 7 not in it), as well as the regression results for this restricted set. Once again, none of the point estimates on the interaction terms are positive and significant: three of the four are negative and one is significantly so at the 5 percent level.

Next, we consider the case where selection reflects a combination of many factors and employ a more formal procedure to achieve the same goal of generating a more comparable set of visa waiver and non-visa waiver countries. We assume that the likelihood that each country is included in the visa waiver program reflects the population in 1995, the amount of trade with the United States in 1995, the PPP-adjusted GDP in 1995, and the natural log of the physical distance from the United States. More formally, this assumption can be written as $\tilde{X}_i = \{Pop_i, Trade_i, GDP_i, \ln(Dist_i)\}$. Following Rosenbaum and Rubin (1983),

we define the propensity score as the conditional probability of selection into the visa waiver program:¹⁷

$$p\left(\tilde{X}_i\right) = Prob\left(VWP_i = 1|\tilde{X}_i\right) = E\left(VWP_i|\tilde{X}_i\right).$$

Similar to the case with a univariate \tilde{X}_i equal to GDP in 2000, we use the propensity scores as a robustness check to identify the subset of visa and non-visa waiver countries that are most similar. We omit from our regressions all countries in the visa waiver program with propensity scores above that of the country outside of the visa waiver program with the highest scores, as well as countries requiring visas that have scores lower than the lowest visa waiver country. This eliminates about half of the countries in the dataset but assures that the included cases more likely lack systematic pre-treatment differences.

Panel A of Table 5 reports results from estimating the propensity scores using a probit regression. As one might expect, this regression indicates that countries that are larger, have more trade with the United States, higher per capita GDP, and that are closer to the United States are more likely to be members of the visa waiver program. Panel B gives the list of countries with sufficiently close propensity scores to be kept in the dataset as well as the results from the difference-in-differences regressions we run on this filtered dataset. As was the case with all other specifications, there is again no regression in which the interaction between the 9/11 and visa waiver indicators is positive and significant.

We next show in Figure 4 plots of the seasonally adjusted and demeaned time fixed effects when we estimate (3) with no interaction term, and allow for distinct time fixed effects for the visa waiver countries and the non-visa waiver countries. These series were calculated as the residuals of regressions of the actual fixed effects on month dummies and a constant term. Hence, the relative vertical levels of the series have no information, but the time series variation indicates how the seasonally adjusted common component of each group changes over time. As before, this method for seasonal adjustment on a short time-series is somewhat crude and too much emphasis should not be placed on small scale or high frequency movements. Aside from the general upward trends in the lines, one can clearly

¹⁷This definition is actually the complement of the standard definition, since participation in the visa waiver program implies exclusion from “treatment” in our data.

see sharp drops in the wake of the 9/11 attacks and during the 3 months of major combat operations in Iraq in early 2003.

Panel A of Figure 4 contains our baseline specification where observations from each country are weighted by the level of entrants from that country in 1996. The plot shows that both series had essentially the same long term trend, rising about 30 percent from early 1996 to late 2000 and early 2001. During 2001, before the 9/11 attacks, both series declined somewhat, with the decline in travel by citizens of visa waiver countries somewhat steeper than from countries requiring visas. The real plunge in entries in both groups of countries, however, occurs after the 9/11 attacks. Though the gap is not striking relative to differences at other time periods, this plot makes it clear that the visa waiver countries saw at least as large a negative impact on short-term travel to the United States after 9/11 as the non-visa waiver countries. From levels that look highly similar from early 2000 to late 2001, entries not requiring visas are far below levels in countries that do need visas during much of the post-9/11 period. This is what results in the negative interaction term in our difference-in-differences estimates.

Our focus on the question of assessing the impact of visa policy on total travel to the United States leads us to emphasize the weighted regressions we have presented as a baseline, attributing more attention to the larger number of visitors from, say, Spain and Chile, than the much smaller number of travelers from, say, Luxembourg or Georgia. Interest in entries from smaller countries, however, might call for greater attention to the unweighted regressions. Panel B of Figure 4 plots the separate time fixed effects for this unweighted case. Differences between the cases in Panel A and B necessarily reflect differences in entry patterns between countries with many entries and countries with few. Indeed, though the two series move largely in parallel from around 2000, the earlier differential trend in the unweighted case suggests perhaps there is a noticeable “visa effect” for some of the smaller countries. In essence, because some small countries, predominantly in the non-visa waiver set, have faster entry growth rates, one might have expected their fixed effect series to rise even further above the visa waiver series in a counterfactual post-9/11 era without tighter visa policy. The gap, however, remains quite small (and shrinks in the final year), implying

that visa policies may have restricted growth in non-visa waiver program entries from these small countries – travel from these nations was growing rapidly but this was throttled back after 9/11. Hence, while we find no meaningful contribution of visa policies to the overall decline in travel to the United States, our analysis does suggest that what would otherwise have been rapid growth in entries from a few small countries was hampered by post-9/11 changes in visa policies.

Next, we consider the addition of an index representing the log-level of real expenditures on travel and tourism by citizens in each of the foreign countries as a new covariate. Using data compiled annually by the World Travel and Tourism Council along with the consulting firm Accenture, we condition our results on country-level expenditures by domestic business and pleasure travelers on domestic and foreign travel. Conditioning on demand for total travel does little to change the results – the key interaction coefficient remains negative in most of the specifications, and is never significant and positive.

Finally, we run our baseline estimate on a different, but related, dataset provided by the U.S. Department of Commerce’s Office of Travel and Tourism Industries (OTTI).¹⁸ These monthly data are derived from the same underlying source as the DHS data, but only start in 1998 and are sorted by country of residence, rather than by country of citizenship. Given there are fewer months of pre-9/11 data available and that participation in the visa waiver program is driven by citizenship, the OTTI data are less suited for our purposes.¹⁹ This robustness check allows us to address two concerns. First, one might reasonably be concerned that our dataset ends in 2003.^{20,21} The OTTI data include monthly entries through the

¹⁸We thank Bryan Roberts and Derekh Cornwell for suggesting we run our specifications on these data and for providing several details in this paragraph.

¹⁹Additionally, the OTTI data involve an imputation in order to estimate the share of in-transit visitors (which are then subtracted from the totals), and we prefer to use the actual total number of entrants in the DHS data.

²⁰When we run our baseline regression but divide the post-9/11 variable into two separate variables for 2002 and 2003, we find an even more significant difference between the two groups of countries in fiscal year 2002, with the sharpest drops in entries from visa waiver countries in 2002. This difference is substantially attenuated, though, by the second year after the attacks and its statistical significance is often lost. This still means that by the end of fiscal 2003, there was no support for the idea that tighter visa policy was instrumental in reducing travel, but if the trend were to continue, it is plausible that in later years one might detect an impact of these visa policies on aggregate entries to the United States.

²¹Some visas have long lives and hence, some travelers that were previously approved under a cheap visa regime can continue visiting the U.S. after 9/11 without undergoing the new visa procedures. We think this

year 2007, and so help us to consider the impact of post-2003 events. Second, given that the composition of non-immigrant entries differs between visa waiver and non-visa waiver countries, it is helpful that the OTTI dataset considers a subset of the DHS entries. These data are restricted primarily to temporary business and pleasure travelers and students, and exclude, for example, short-term workers and foreign diplomats that are included in our primary dataset. Estimating equation (3) with these data generates very similar results to those from our baseline data. Again, the interaction term of interest is generally negative and is never positive and significant.²²

8 Other Factors Influencing Post-9/11 Travel

We have shown that for a wide variety of specifications, and with only a few minor caveats, there is no evidence that entries of travelers requiring visas dropped by more after 9/11 than travel by people not requiring visas. The evidence suggests that, if anything, it is the number of entrants from countries outside of the visa waiver program that dropped by less. Since it is implausible that tighter visa policies led to more visitors, we next consider factors unrelated to visa policy that might help explain some of the cross-country variation in the response of potential travelers after the attacks.

We start by considering heterogeneity across countries in terms of the composition of travelers between businesspeople and tourists. Our dataset includes an estimate for each country of the number of business travelers as a percent of total temporary business and pleasure entries in 2000, listed in Panel B of Table 2. We assume that this composition is relatively stable over time and so include it as an interaction term with the 9/11 indicator that equals zero prior to 9/11 and one after the attacks. Columns (1) to (4) of Table 6 show the regression results when this business traveler interaction term is included. Since the demand for business travel is likely to be less elastic than pleasure travel, the positive coefficient on this interaction term is unsurprising. As travel became more expensive for everyone after 9/11, business travelers were less likely to respond and change their behavior

applies to a small number of entrants.

²²Results available upon request from the authors.

than tourists were, so that business travel overall declined by less than non-business travel. This estimate is insignificant for the baseline case without country-specific time trends and does not change the sign on the visa waiver interaction term that is the focus of the paper.

Next we consider the impact of different attitudes toward the United States in accounting for changes in the demand for travel. Ideally, one would like to have a time series on attitudes before and after 9/11. We do not have such data, but instead must use a snapshot of foreign views of the United States taken in early 2002 by the Pew Global Attitudes Project. This project asked citizens of 44 countries if they held a favorable view of the United States (among many other things). We assume that the 9/11 attacks did not change the cross-country ordering of views toward the United States but rather increased the dispersion in such views, so a snapshot of post-9/11 views would be highly correlated with the change in views from an earlier period. The average survey response for each country on this question is a number between 1.0 and 4.0, with a score of 3.9, for instance, representing a case where most recipients held very unfavorable views of the United States. Unfortunately, the survey data, listed in Panel B of Table 2, cover only 18 of the countries in our dataset. Nonetheless, as shown in columns (5) through (8) of Table 6, this interaction coefficient enters negatively and with a high statistical significance in most of our specifications. The negative coefficient implies that travelers from countries with a less favorable view of the United States entered with a lower frequency following 9/11 compared with before the attacks. This is suggestive that differing perceptions mattered for travelers' decisions. With only 18 countries included in the regression, however, we cannot provide more definitive evidence. Further, inclusion of this variable pushes the 9/11 and visa waiver program interaction coefficient even further negative, and hence, itself cannot explain the lack of an observable "visa effect."

Following on this result, and in light of many articles suggesting that the view of the United States after 9/11 has worsened in countries with large Muslim communities, we add a variable that captures the percent of the population that is Muslim in each of the countries in our dataset. The data, listed in Panel B of Table 2, are taken from the website www.islamicpopulation.com, which generally provides a specific percentage for the Muslim population of any given country in 2006. For the few cases in which the exact share is not

listed (typically countries in South America), we list the Muslim population share as zero.²³

Columns (9) to (12) of Table 6 show that the term capturing the share of Muslim populations interacted with post-9/11 is negative in every specification and is highly significant in the majority of cases. Here it is worth noting that this interaction cannot be a particularly significant quantitative driver of the post-9/11 drop in entries since most of the countries with many entries have small Muslim populations. This coefficient does indicate, however, that the few large Muslim countries generally saw relatively large declines in entries, even when compared to similar countries that also required visas.²⁴ As with the “view of U.S.” variable, however, accounting for variation in Muslim populations also fails to reverse the sign on the visa waiver program interaction term. Further discussion of these relationships is outside the scope of our analysis, but we conclude that factors far broader than visa policy, such as changes in the non-visa costs of travel, whether a traveler is a business or pleasure traveler, or travelers’ views of the United States, are more likely to explain the differences across countries in the drop in travel to the United States after 9/11 than changes in visa policy.

9 Conclusion

We find that the decline in visits to the United States from countries requiring a visa was not larger than the decline in entries from countries exempt from visa requirements. This suggests that tighter visa policy was not the cause of the sharp drop in business and leisure travel to the United States in the wake of the 9/11 attacks. Changes in the visa process enacted with a delay after 9/11 could be connected with the complaints voiced about visa problems, and these changes perhaps reduced entries somewhat from countries outside the visa waiver

²³Further, there are two cases in which a country is not listed at all, in which case we take the percentage from the CIA factbook, available online.

²⁴We attempted to formalize this by including a triple interaction term in these regressions, capturing countries in the visa waiver program, after 9/11, and by the size of their Muslim population, but this test has very little power since there are few countries in the visa waiver program with substantial Muslim populations. The large unconditional declines from a country like Singapore, though, without the need for visas and with a medium sized Muslim population, suggests that the tendency for countries with large Muslim populations to have less entries after 9/11 may be independent of visa policy.

program. The timing of this impact, however, does not correspond with the immediate drop in entries, nor does its magnitude stand out when compared with the impact of changes unrelated to visa policy.

It is difficult to say for sure why the travel plans of people who did not require a visa to visit the United States were affected more (or, at least, not less) after 9/11 than travel by people who needed to obtain a visa. In addition to some of the factors considered in section 8, another explanation for the larger drop in visitors who do not require visas might be connected to the increase in non-monetary costs, such as waiting times and other aggravations associated with increased security involved in traveling to the United States. Before 9/11, short-term visits to the United States were nearly hassle-free for citizens of countries participating in the visa waiver program. In contrast, nationals of other countries, mainly less-developed ones, faced the hazards of the visa application process. After 9/11, visa applicants might well have received greater scrutiny, and indeed, many had to wait longer for visas and travel further for an in-person interview at a U.S. diplomatic post. Travelers who did not require a visa, however, were also now facing new costs – that is, new hassles – to enter the United States as a result of post-9/11 changes in security. Compared to the previous near-zero amount of hassle, the added aggravation for travelers from these visa waiver countries was proportionately enormous. While changes in visa policy might have affected travelers needing them, this appears to have been a secondary factor in accounting for the overall decline in short-term business and tourist travel to the United States.

Future research could shed light on the causes of the steep reduction in temporary entries of businesspersons and tourists to the United States after the 9/11 attacks. The results in this paper, however, lead us to dismiss the idea that changes in visa policy are primarily to blame.

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Tables

	Country	Date of Inclusion		Date of Removal	
		Month	Year	Month	Year
(1)	United Kingdom	July	1988		
(2)	Japan	December	1988		
(3)	France	July	1989		
(4)	Switzerland	July	1989		
(5)	Germany	July	1989		
(6)	Sweden	July	1989		
(7)	Italy	July	1989		
(8)	Netherlands	July	1989		
(9)	Andorra	October	1991		
(10)	Austria	October	1991		
(11)	Belgium	October	1991		
(12)	Denmark	October	1991		
(13)	Finland	October	1991		
(14)	Iceland	October	1991		
(15)	Lichtenstein	October	1991		
(16)	Luxembourg	October	1991		
(17)	Monaco	October	1991		
(18)	New Zealand	October	1991		
(19)	Norway	October	1991		
(20)	San Marino	October	1991		
(21)	Spain	October	1991		
(22)	Brunei	July	1993		
(23)	Ireland	April	1995		
(24)	Argentina	July	1996	February	2002
(25)	Australia	July	1996		
(26)	Slovenia	September	1997		
(27)	Portugal	August	1999		
(28)	Singapore	August	1999		
(29)	Uruguay	August	1999	April	2003
(30)	Czech Republic	November	2008		
(31)	Estonia	November	2008		
(32)	Hungary	November	2008		
(33)	Latvia	November	2008		
(34)	Lithuania	November	2008		
(35)	Korea	November	2008		
(36)	Slovak Republic	November	2008		
(37)	Malta	December	2008		

	Panel A				Panel B		
	Number of Countries	Number of Non-Immigrant Entries		Decline	Business Travelers in 2000	Attitude Toward the United States	Muslim Population
		Fiscal Year 2000	Fiscal Year 2003		Percent of Total	1 (Best) to 4 (Worst)	Percent of Total
Aggregate Data	204	34,113,528	28,214,826	-17%			
Full Sample	65	28,943,462	24,109,035	-17%			
Visa-Waiver	22	18,896,232	14,805,762	-22%			
(1) Australia		593,246	552,916	-7%	22.9		1.5
(2) Austria		213,384	131,340	-38%	18.3		2.2
(3) Belgium		258,904	179,559	-31%	27.5		3.6
(4) Denmark		178,349	163,408	-8%	30.6		3.0
(5) Finland		118,369	86,742	-27%	34.1		0.2
(6) France		1,329,169	1,040,949	-22%	20.7	2.35	10.0
(7) Germany		2,146,442	1,444,665	-33%	19.0	2.33	3.7
(8) Iceland		31,803	25,229	-21%	16.4		0.0
(9) Ireland		405,583	372,137	-8%	16.1		0.0
(10) Italy		810,613	641,216	-21%	19.4	2.15	2.4
(11) Japan		5,259,703	3,593,469	-32%	7.8	2.18	0.1
(12) Luxembourg		12,913	7,640	-41%	16.1		1.1
(13) Netherlands		684,041	546,191	-20%	24.2		5.4
(14) New Zealand		200,147	204,219	2%	19.5		1.2
(15) Norway		166,300	142,935	-14%	27.2		1.0
(16) Portugal		114,701	84,436	-26%	19.2		0.5
(17) Singapore		119,632	81,919	-32%	39.1		15.0
(18) Slovenia		18,035	11,732	-35%	21.6		1.6
(19) Spain		466,168	430,070	-8%	17.5		1.2
(20) Sweden		377,000	257,899	-32%	29.1		3.1
(21) Switzerland		390,237	257,898	-34%	16.9		3.1
(22) United Kingdom		5,001,493	4,549,193	-9%	15.7	1.93	2.5
Non Visa-Waiver	40	5,033,989	4,456,899	-11%			
(1) Belize		30,859	26,587	-14%	24.5		0.0
(2) Bolivia		53,405	34,825	-35%	16.8	2.40	0.0
(3) Botswana		2,392	1,945	-19%	30.0		3.0
(4) Chile		211,738	140,553	-34%	20.4		0.0
(5) Colombia		478,142	389,768	-18%	17.1		0.1
(6) Costa Rica		173,112	149,998	-13%	23.8		0.0
(7) Croatia		24,925	19,960	-20%	26.9		3.0
(8) Cyprus		13,345	9,554	-28%	17.2		23.0
(9) Czech Republic		52,910	44,478	-16%	21.8	2.21	0.2
(10) Ecuador		138,661	163,531	18%	17.6		0.0
(11) Estonia		8,856	8,758	-1%	21.7		0.7
(12) Georgia		4,869	5,290	9%	27.9		20.0
(13) Greece		79,359	60,083	-24%	21.2		1.5
(14) Hong Kong		129,401	75,780	-41%	24.0		1.4
(15) Hungary		68,969	45,020	-35%	19.7		0.1
(16) India		560,110	559,805	0%	31.8	2.18	13.4
(17) Indonesia		97,247	65,071	-33%	22.9	2.40	88.0
(18) Iran		28,425	10,398	-63%	9.4		99.0
(19) Israel		357,644	307,101	-14%	24.9		18.9
(20) Jamaica		277,895	224,478	-19%	17.3		0.0
(21) Jordan		30,631	21,214	-31%	22.3	3.27	94.0
(22) Korea		811,951	845,272	4%	28.7	2.48	0.2
(23) Kyrgyz Republic		2,000	1,668	-17%	40.5		75.0
(24) Latvia		11,733	10,494	-11%	25.4		0.4
(25) Lithuania		13,747	15,811	15%	24.6		0.1
(26) Macau		1,439	937	-35%	11.2		0.0
(27) Malaysia		95,709	53,160	-44%	39.3		59.0
(28) Malta		9,112	5,790	-36%	10.3		1.1
(29) Mauritius		3,028	1,415	-53%	22.2		16.6
(30) Morocco		27,590	18,021	-35%	15.3		99.0
(31) Peru		229,307	224,542	-2%	17.1	1.96	0.0
(32) Philippines		281,463	273,439	-3%	20.3	1.68	7.0
(33) Poland		147,125	155,810	6%	17.1	1.99	0.1
(34) Romania		44,162	49,722	13%	24.6		1.0
(35) Russia		137,476	119,333	-13%	43.5	2.35	0.2
(36) Slovak Republic		20,933	24,629	18%	29.0	2.39	0.0
(37) South Africa		132,818	108,232	-19%	28.8	2.17	2.0
(38) Thailand		97,560	74,195	-24%	30.4		9.1
(39) Tunisia		12,885	4,181	-68%	16.2		98.0
(40) Turkey		131,056	106,051	-19%	23.3	3.04	99.8

Table 2: Summary Statistics of Non-Immigrant Entries in the Sample

	Log Entrants											
	Least Squares (Baseline)			Poisson Pseudo Maximum Likelihood			Gamma Regression					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post-9/11 * Visa Waiver Program	-0.116 (0.063)*	-0.105 (0.058)*	-0.074 (0.060)	0.012 (0.054)	-0.055 (0.069)	-0.131 (0.056)**	-0.177 (0.074)**	-0.054 (0.054)	-0.115 (0.061)*	-0.106 (0.057)*	-0.078 (0.058)	0.021 (0.046)
VWP	0.013 (0.048)	0.486 (0.473)	-0.004 (0.063)	0.718 (0.593)	-0.071 (0.065)	0.027 (0.049)	-0.051 (0.030)*	0.023 (0.077)	0.001 (0.056)	0.195 (0.228)	-0.006 (0.061)	0.486 (0.418)
Log GDP in Home Country	1.260 (0.305)***	0.446 (0.207)**	0.507 (0.344)	0.317 (0.151)**	2.220 (0.0391)***	1.038 (0.310)***	0.197 (0.590)	0.214 (0.362)	1.264 (0.305)***	0.401 (0.204)**	0.471 (0.350)***	0.302 (0.149)**
Log Real Exchange Rate	-0.580 (0.117)***	-0.505 (0.075)***	-0.443 (0.146)***	-0.353 (0.053)***	-0.455 (0.134)***	-0.549 (0.118)***	-0.214 (0.104)**	-0.406 (0.128)***	-0.586 (0.116)***	-0.521 (0.084)***	-0.438 (0.142)***	-0.359 (0.052)***
Separate Time Trends	N	N	Y	Y	N	N	Y	Y	N	N	Y	Y
Weighted	Y	N	Y	N	Y	N	Y	N	Y	N	Y	N
Fixed Effects	Time Country	Time Country	Time Country	Time Country	Time Country	Time Country	Time Country	Time Country	Time Country	Time Country	Time Country	Time Country
Excluded Countries	Canada Mexico Argentina	Canada Mexico Argentina	Canada Mexico Argentina	Canada Mexico Argentina	Canada Mexico Argentina	Canada Mexico Argentina	Canada Mexico Argentina	Canada Mexico Argentina	Canada Mexico Argentina	Canada Mexico Argentina	Canada Mexico Argentina	Canada Mexico Argentina
Countries	62	62	62	62	62	62	62	62	62	62	62	62
Observations	4898	4898	4898	4898	4898	4898	4898	4898	4898	4898	4898	4898

* Significant at 10%, ** Significant at 5%, *** Significant at 1%

Table 3: Primary Regression Specifications

Notes: Standard errors are robust to general heteroskedasticity and are clustered by country to account for serial correlation. PPML estimates are implemented in Stata using the command “poisson” and specifying the independent variable in levels (we still write “log entrants” above because the estimates should still be interpreted as elasticities). Gamma regressions are estimated in Stata using the command “glm” and also specifying the independent variable in levels, with the options including link(log) and family(gamma).

VWP		Non-VWP		Log Entrants			
		(1)	(2)	(3)	(4)		
Portugal	Czech Republic	-0.160	0.036	-0.274	-0.070		
Slovenia	Greece	(0.092)	(0.129)	(0.098)**	(0.197)		
Spain	Mauritius						
New Zealand	Korea	0.088	0.881	0.148	1.336		
Italy	Malta	(0.065)	(0.860)	(0.091)	(0.924)		
	Cyprus						
	Israel						
	Log GDP in Home Country	0.779	2.451	0.347	2.632		
		(0.349)**	(0.960)**	(0.273)	(0.887)**		
	Log Real Exchange Rate	-0.914	-0.614	-1.096	-0.443		
		(0.233)***	(0.405)	(0.164)***	(0.524)		
	Separate Time Trends	N	N	Y	Y		
	Weighted	Y	N	Y	N		
	Fixed Effects	Time	Time	Time	Time		
		Country	Country	Country	Country		
	Countries	12	12	12	12		
	Observations	964	964	964	964		

* Significant at 10%, ** Significant at 5%, *** Significant at 1%

Table 4: Results Conditional on Country GDP

Notes: Standard errors are robust to general heteroskedasticity and are clustered by country to account for serial correlation.

Panel A: Propensity Score Determination (Probit)

Propensity Score Probit	Coefficient	Std. Error
Population 1995	5.92E-04	1.14E-04
Trade 1995	1.06E-08	8.41E-10
PPP-relative GDP 1995	5.22E-02	5.70E-04
Ln_Distance	-2.21E-01	2.02E-02

Panel B: Regression Results from Propensity-Score Restricted Dataset

VWP	Non-VWP		Log Entrants			
			(1)	(2)	(3)	(4)
Australia	Botswana					
Austria	Chile	Post-9/11 * Visa	-0.023	0.017	-0.107	0.072
Belgium	Croatia	Waiver Program	(0.090)	(0.060)	(0.082)	(0.057)
Denmark	Cyprus					
Finland	Czech Republic	VWP	-0.021	-0.020	-0.035	0.059
France	Estonia		(0.048)	(0.054)	(0.065)	(0.075)
Germany	Georgia					
Iceland	Greece	Log GDP in Home Country	1.595	1.043	1.025	0.608
Ireland	Hong Kong		(0.451)***	(0.351)***	(0.332)***	(0.332)*
Italy	Israel					
Netherlands	Korea	Log Real Exchange Rate	-0.775	-0.670	-0.534	-0.202
New Zealand	Kyrgyz Republic		(0.118)***	(0.150)***	(0.226)**	(0.184)
Norway	Latvia					
Portugal	Lithuania	Separate Time Trends	N	N	Y	Y
Singapore	Macau					
Spain	Malta	Weighted	Y	N	Y	N
Sweden	Mauritius					
United Kingdom	Russia	Fixed Effects	Time	Time	Time	Time
	Slovak Republic		Country	Country	Country	Country
		Countries	37	37	37	37
		Observations	2962	2962	2962	2962

* Significant at 10%, ** Significant at 5%, *** Significant at 1%

Table 5: Regression Results Conditional on Propensity Score

Notes: Standard errors are robust to general heteroskedasticity and are clustered by country to account for serial correlation.

	Log Entrants											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post-9/11 * Visa Waiver Program	-0.116 (0.065)*	-0.102 (0.058)*	-0.048 (0.066)	0.019 (0.053)	-0.218 (0.060)***	-0.212 (0.056)***	-0.185 (0.072)**	-0.056 (0.060)	-0.133 (0.062)**	-0.146 (0.059)**	-0.097 (0.058)*	-0.017 (0.054)
Post-9/11 * Share of Business Travelers	0.000 (0.356)	0.221 (0.436)	0.299 (0.349)	0.520 (0.268)*								
Post-9/11 * View of U.S.					-0.426 (0.122)***	-0.259 (0.069)***	-0.221 (0.115)*	-0.195 (0.060)***				
Post-9/11 * Share of Muslims in the Population									-0.248 (0.119)**	-0.241 (0.094)**	-0.296 (0.099)**	-0.188 (0.050)***
VWP	0.013 (0.048)	0.480 (0.477)	0.006 (0.066)	0.725 (0.593)					0.016 (0.046)	0.490 (0.472)	-0.004 (0.061)	0.718 (0.594)
Log GDP in Home Country	1.260 (0.285)***	0.438 (0.207)**	0.527 (0.350)	0.317 (0.149)**	1.338 (0.375)***	0.505 (0.237)**	0.283 (0.541)	0.102 (0.261)	1.276 (0.308)***	0.459 (0.207)**	0.457 (0.345)	0.296 (0.155)*
Log Real Exchange Rate	-0.580 (0.126)***	-0.499 (0.080)***	-0.414 (0.169)**	-0.331 (0.057)***	-0.588 (0.155)***	-0.474 (0.103)***	-0.491 (0.212)**	-0.480 (0.125)***	-0.585 (0.114)***	-0.478 (0.090)***	-0.441 (0.143)**	-0.318 (0.056)***
Separate Time Trends	N	N	Y	Y	N	N	Y	Y	N	N	Y	Y
Weighted	Y	N	Y	N	Y	N	Y	N	Y	N	Y	N
Fixed Effects	Time Country	Time Country	Time Country	Time Country	Time Country	Time Country	Time Country	Time Country	Time Country	Time Country	Time Country	Time Country
Excluded Countries	Canada Mexico Argentina	Canada Mexico Argentina	Canada Mexico Argentina	Canada Mexico Argentina	Canada Mexico Argentina	Canada Mexico Argentina	Canada Mexico Argentina	Canada Mexico Argentina	Canada Mexico Argentina	Canada Mexico Argentina	Canada Mexico Argentina	Canada Mexico Argentina
Countries	62	62	59	59	18	18	18	18	62	62	62	62
Observations	4898	4898	4223	4223	1474	1474	1474	1474	4898	4898	4898	4898

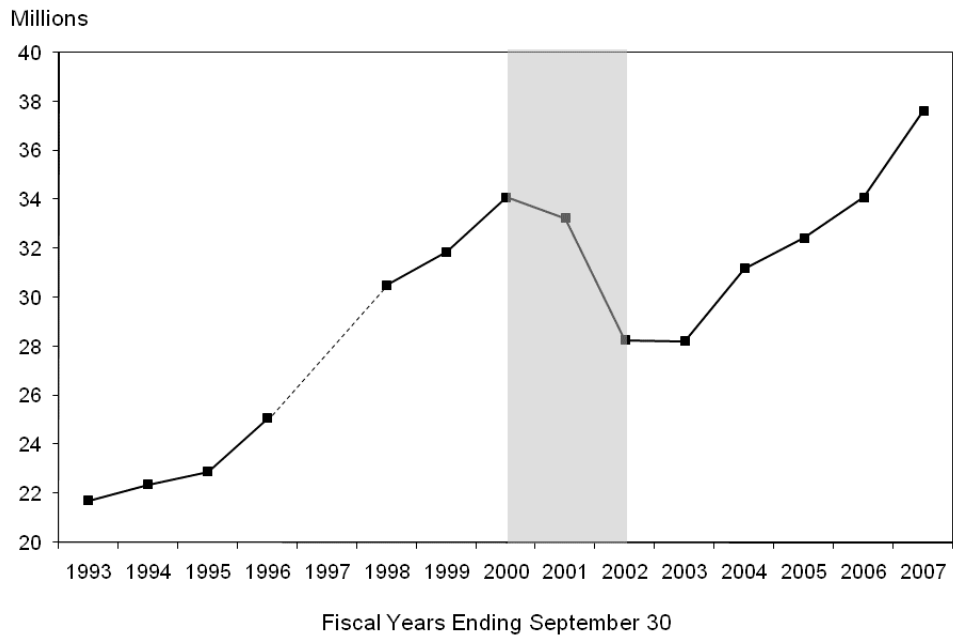
* Significant at 10%, ** Significant at 5%, *** Significant at 1%

Table 6: Regression Results Conditional on Business Travelers and View of U.S.

Notes: Standard errors are robust to general heteroskedasticity and are clustered by country to account for serial correlation.

Figures

Panel A: Annual Non-Immigrant Entries (Published)



Panel B: Monthly Non-Immigrant Entries (Our Dataset)

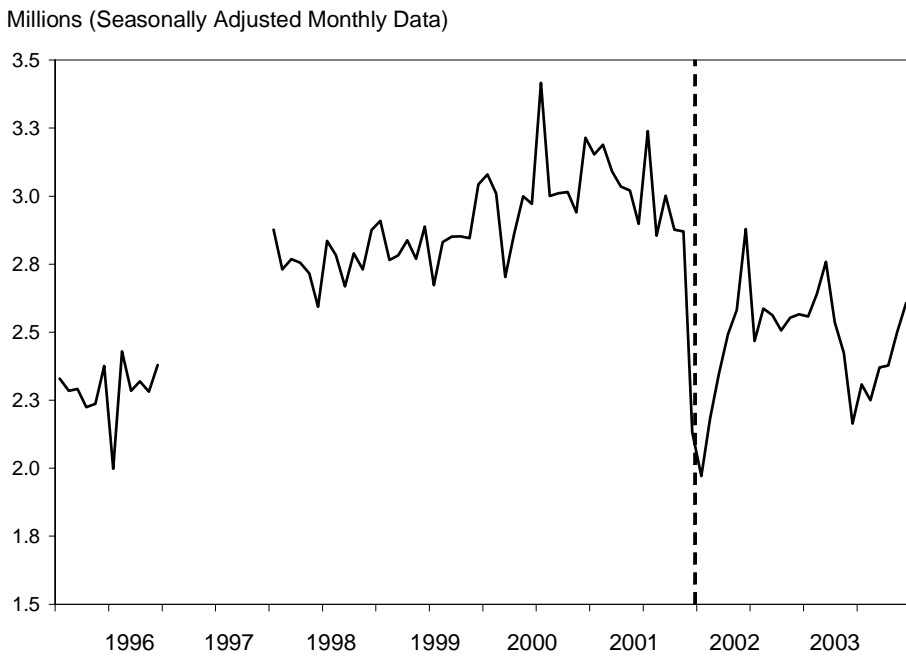


Figure 1: Non-Immigrant Entries

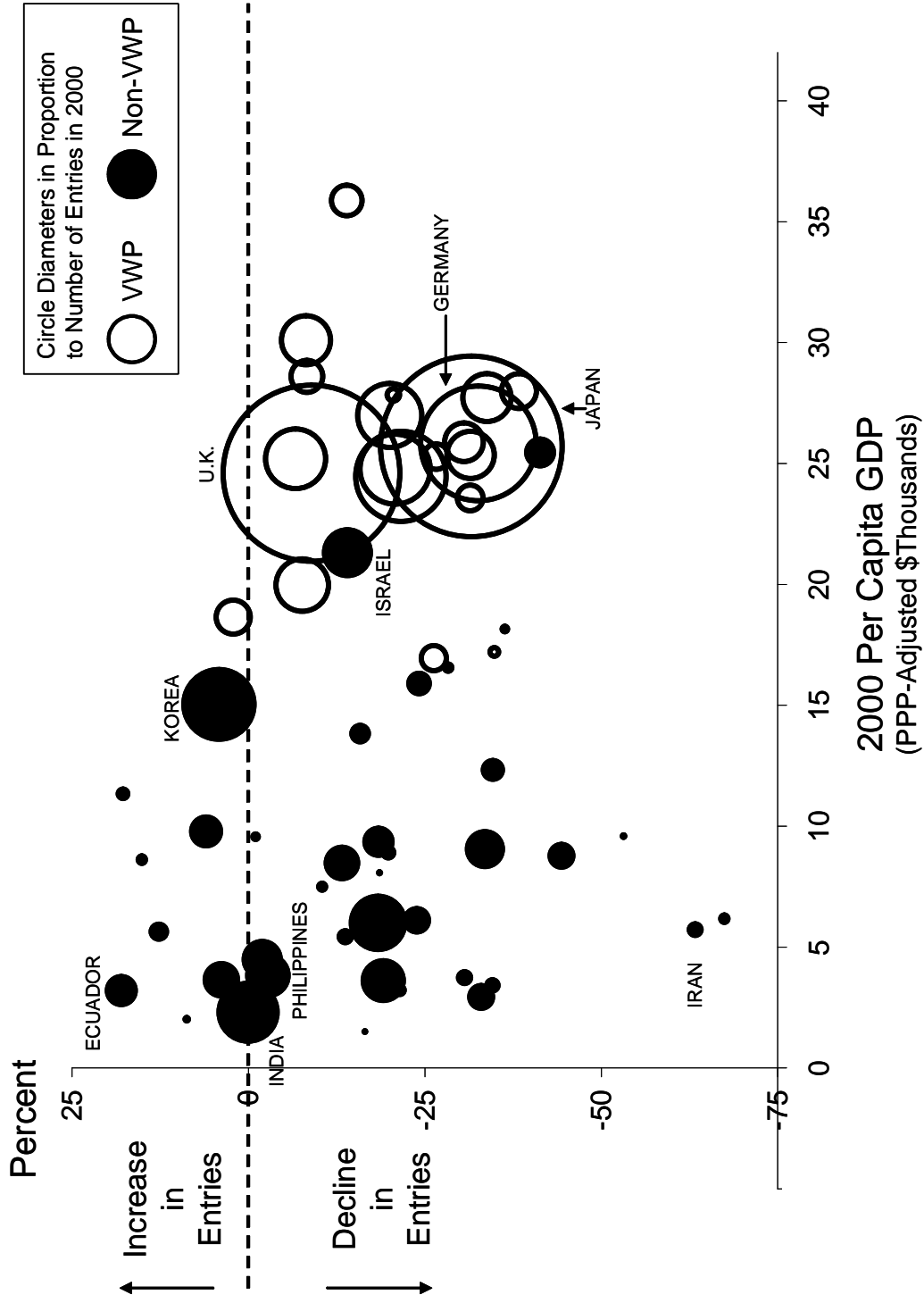
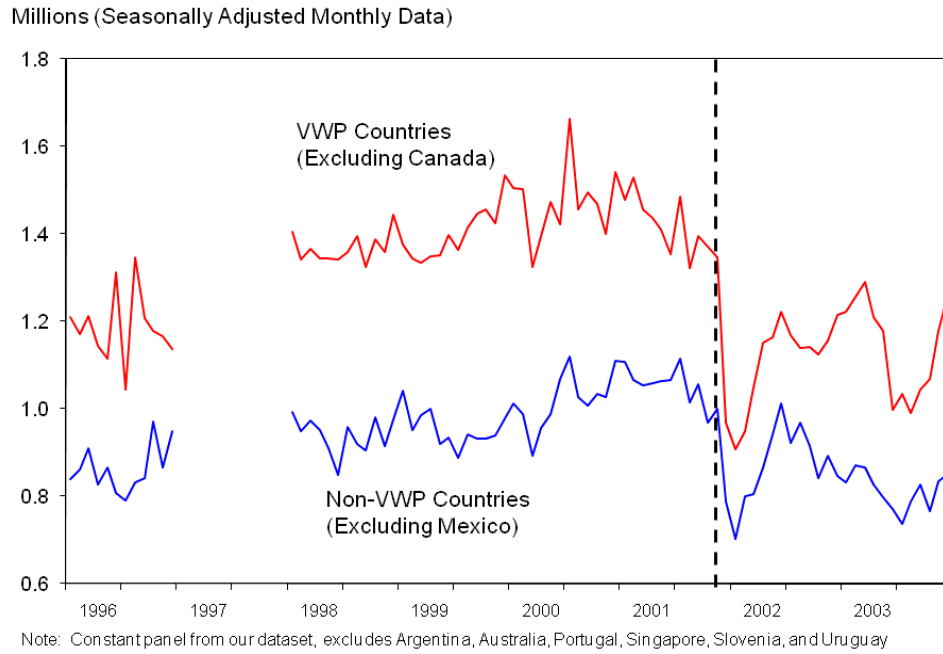


Figure 2: Change in Entries by Country, FY2000 to FY2003

Notes: Per Capita GDP in 2000 (PPP) taken from IMF World Economic Outlook database. Excludes Argentina, Canada, and Mexico, because they are not included in our baseline empirical results, as well as Macau, for which there is no per capita PPP-adjusted GDP.

Panel A: Levels



Panel B: Natural Log Levels

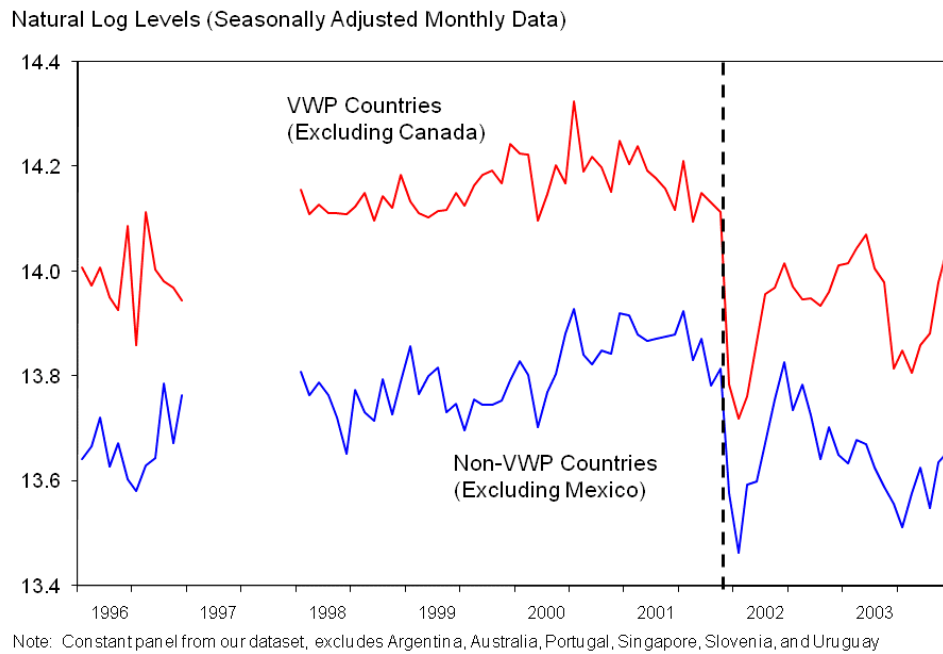
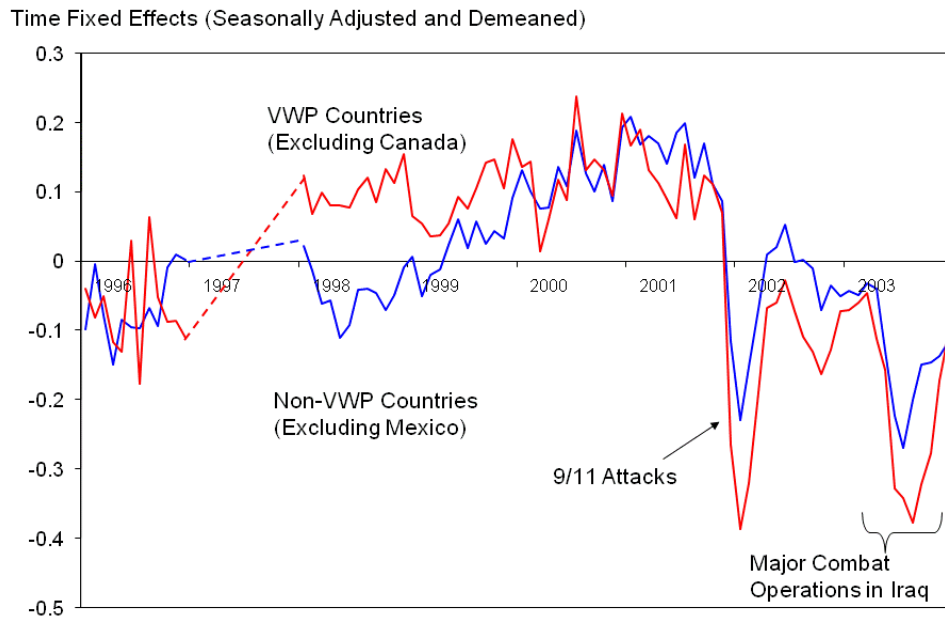


Figure 3: Monthly Non-Immigrant Entries, by Participation in the Visa Waiver Program

Panel A: Separate Time Fixed Effects, Baseline Case (Weighted)



Panel B: Separate Time Fixed Effects (Unweighted)

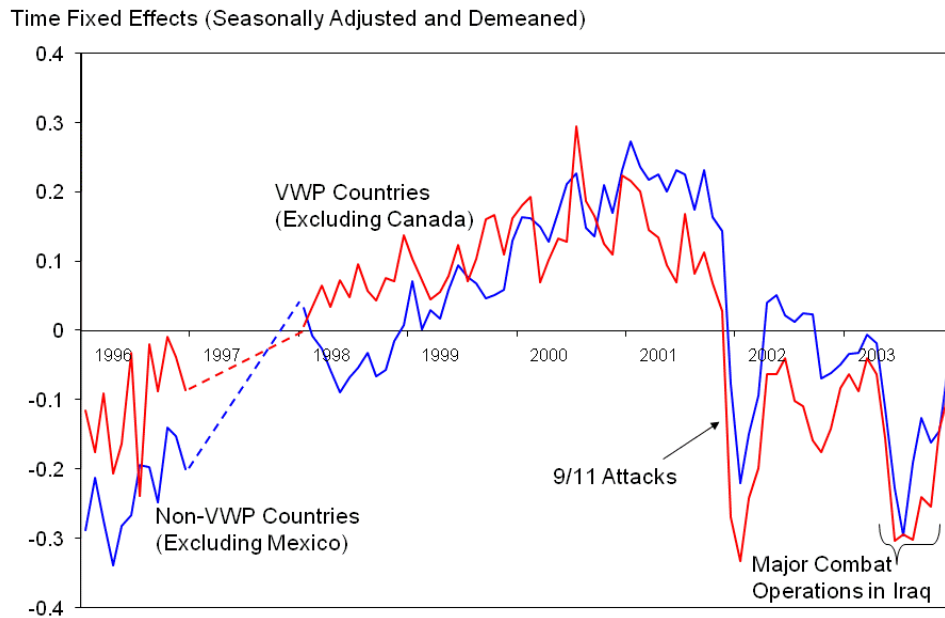


Figure 4: Separate Time Fixed Effects