INTERPOL's surveillance network in curbing transnational terrorism

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Abstract

This paper investigates the role that INTERPOL surveillance – the Mobile INTERPOL Network Database (MIND) and the Fixed INTERPOL Network Database (FIND) – played in the War on Terror since its inception in 2005. MIND/FIND surveillance allows countries to screen people and documents systematically at border crossings against INTERPOL databases on terrorists, fugitives, and stolen and lost travel documents. Such documents have been used in the past by terrorists to transit borders. By applying methods developed in the treatment-effects literature, this paper establishes that countries adopting MIND/FIND experienced fewer transnational terrorist attacks than had they not adopted MIND/FIND. Our estimates indicate that, on average, during 2008–2011, adopting and using MIND/FIND results in 1.23 fewer transnational terrorist incidents each year per 100 million people. Thus, a country like France with a population just above 64 million people in 2008 would have 0.79 fewer transnational terrorist incidents per year owing to its use of INTERPOL surveillance. For most treatment countries, this amounts to a sizeable proportional reduction of about 60 per cent.
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1 Introduction

The International Criminal Police Organization (INTERPOL) benefits member countries by coordinating their police efforts. Sandler, Arce, and Enders (2011) estimated that for every dollar invested in INTERPOL's counterterrorism activities, member countries receive $200 in average returns. This is a huge rate of return for public money. INTERPOL provides multiple services – e.g., police training, communication links, and coordinating the hunt for fugitives – to member countries. This paper focuses on one of these services – the control of transnational terrorism.

In 2005, INTERPOL introduced two surveillance networks, the Mobile INTERPOL Network Database (MIND) and the Fixed INTERPOL Network Database (FIND), which facilitate searches of people, motor vehicles, and documents at international transit or other points. The main difference between these networks is that FIND allows access to an online database, which is continously updated, whereas MIND provides access to an offline database, which is periodically downloaded in an updated form.

These technologies may be effective at curbing international crime and transnational terrorism, however, as of December of 2008 only 47 of the then 188 INTERPOL member countries had adopted these technologies. The associated crime-fighting transnational externalities derived from MIND/FIND were not fully internalized by member countries. In order to understand the reasons for these unexploited benefits, Enders and Sandler (2011) studied why some countries chose to join the MIND/FIND networks and others did not. They found that income per capita, population, and democratic freedoms were the main determinants of whether INTERPOL member countries installed MIND/FIND technologies. As of August 2012, more than a hundred members were connected to either the MIND or FIND networks or both. This increased membership came as INTERPOL pushed to educate its member countries about the benefits of MIND/FIND in fighting international crime and transnational terrorism. However, not all connected countries used the network.

The current paper differs from Enders and Sandler (2011), which investigated not only the determinants of MIND/FIND adoption, but also what could be done to encourage greater adoption. In the current paper, we ask whether the implementation and use of MIND/FIND technology reduced the amount of transnational terrorism in the implementing countries. A favorable answer to this question can provide strong positive inducements for other INTERPOL countries to adopt and use MIND/FIND. Countries must, however, remember that keeping transnational terrorists from moving about freely from country to country is a weakest-link public good problem, because terrorists will seek to transit the least-vigilant borders (Enders and Sandler 2012). If, in addition, MIND/FIND limits transnational terrorist attacks in adopting countries, then planned attacks are likely displaced to other countries, where similar technologies are not deployed (see Enders and Sandler 1993). Displacement effects are ameliorated when main airport hub countries are utilizing INTERPOL surveillance, so that terrorists must take circuitous routes and cannot enter their prime-target countries.

We apply causal inference methods developed in the treatment-effects literature (Angrist and Pischke 2009; Wooldridge 2010) in order to establish a causal relationship between the treatment, MIND/FIND adoption, and transnational terrorist incidents. Applying causal inference methods to assess treatment effects at the country level is a challenging task as some of the assumptions maintained in the treatment-effects literature might not hold at the aggregate level. However, we are not the first to investigate causal effects at an aggregate level: for instance, Lin and Ye (2007) assessed the effectiveness of the inflation-targeting policy; Gilligan and Sergenti (2008) looked at the effect of United Nations peacekeeping missions on building a sustainable peace after civil war; Nielsen et al. (2011) examined the effect of foreign aid on armed conflict; and Chang and Lee (2011) analyzed the trade-promoting effect of the World Trade Organization. Using the treatment-effects approach for causal inference, we find that MIND/FIND adopters, who used the technology, experienced fewer transnational terrorist attacks than non-users. Even though the reduction in incidents per adopter is small, the proportional reduction is large for the period 2008–2011, at about 60 per cent for most treatment countries.

The remainder of the paper contains six sections. Section 2 presents necessary preliminaries on INTERPOL and MIND/FIND, while Section 3 describes our data set. Section 4 indicates our methodology. Section 5 reports estimates of the treatment effects. Section 6 addresses the issue of covariate balance. Section 7 concludes with a discussion of our findings.

2 Preliminaries on INTERPOL and MIND/FIND

A relevant distinction for this study is between domestic and transnational terrorist attacks. Domestic terrorism is homegrown and home-directed with no international externalities for other countries. For domestic terrorism, the perpetrators, the victims, and the targets (e.g., the institution receiving terrorist demands) are all from the venue country, where the attack takes place. In contrast, transnational terrorism involves perpetrators, victims, and/or targets from two or more countries. If the terrorists stage an attack in another country, then the incident is transnational terrorism. When a terrorist attack in, say, England kills or injuries an American, the attack is a transnational terrorist event. As such, it generates transnational externalities for countries other than where the attack takes place. In some terrorist instances, INTERPOL's resources assist member countries' effort to capture transnational terrorists, given INTERPOL's international mission.

INTERPOL was established in 1923 as an independent international organization with the mission to promote international cooperation in fighting international crime. Currently, INTERPOL has 190 member countries, whose assigned membership fees mostly fund the organization's staff, infrastructure, and operations. The remainder of INTERPOL's funding comes from voluntary donations. INTERPOL links law enforcement agencies, the members' National Central Bureaus (NCBs), and INTERPOL General Secretariat (IPGS) in fighting transnational crime and terrorism. In particular, INTERPOL addresses six primary criminal concerns: corruption, drugs and organized crime, financial and high-technology crime, fugitives, trafficking in humans, and transnational terrorism (INTERPOL 2011). After the four skyjackings on September 11, 2001 (henceforth, 9/11), INTERPOL channelled up to 20-25% of its annual crime-fighting resources into coordinating international law enforcement efforts to address transnational terrorism (Sandler, Arce, and Enders 2011). INTERPOL provides its communication networks, its training facilities, its best practices, its data banks, and other assets to member countries to assist in their arrest of suspected terrorists. Many of these arrests occur as terrorists are identified when they attempt to transit countries' borders.

INTERPOL members and their law enforcement agents can communicate over INTERPOL's secure communication linkage, I-24/7, which is a restricted-access internet portal. When connected to I-24/7, members' law enforcement agents can share information and access INTERPOL databases and online resources. I-24/7 is also used by INTERPOL to issue arrest (red) notices and to broadcast country-initiated diffusions to alert member countries to detain suspected criminals and/or terrorists. Among many other things, INTERPOL databases contain information on suspected terrorists and stolen and lost travel documents (SLTD). Such documents have been used by terrorists and criminals to transit international borders – this was true of some 9/11 hijackers (Sandler, Arce, and Enders 2011).

The ability of member countries to apprehend criminals and terrorists at their borders was greatly enhanced at the end of 2005 when INTERPOL offered MIND and/or FIND to interested members. MIND/FIND provides an efficient systematic means for checking people, motor vehicles, and travel documents against INTERPOL's global databases. With MIND/FIND, countries can check all passports and motor vehicles at border crossings and other points. In a matter of seconds, scanned passports or vehicle documents are checked by MIND/FIND against national and INTERPOL data banks. In the absence of MIND/FIND, these searches would be prompted by suspicious behavior, which has a strong random component. Moreover, the border official would have to leave his/her duty post and key in the passport or other document numbers at the I-24/7 portal. Such action is subject to error.

Countries may rely on MIND, FIND, or both, depending on their infrastructure. One key difference between MIND and FIND involves the freshness of accessed infomation. FIND allows real-time online access to INTERPOL databases, while MIND contains a copy of these databases. This offline copy is updated periodically, usually within 48 hours (Enders and Sandler 2011). Thus, FIND provides somewhat more up-to-date data; however, this advantage is likely to dissipate over time as MIND is updated more regularly. Countries can still make arrests without having MIND/FIND when a person's behavior raises suspicions, an alert has been issued, or a person turns him- or herself in. Not all countries linked to MIND/FIND utilize the technology for searches. This is particularly true of countries whose MIND/FIND linkage was externally funded – e.g., some Caribbean and African countries. So possessing MIND/FIND is no guarantee that it will be applied to border and/or other searches (Enders and Sandler 2011). In March 2014, this was made abundantly clear when two passengers boarded Malaysian Airlines flight 370 with stolen passports and were not screened by Malaysia, a FIND country since June 2007.

3 The data

Based on the treatment-effects literature jargon, INTERPOL member countries are the unit of analysis, the treatment is the connection to and moderate use of the MIND/FIND network, and the outcome variable is the number of transnational terrorism incidents.

IPGS provided us with the exact date of MIND/FIND connections, which ran from December 13, 2005 when Switzerland linked to the MIND network to July 19, 2012 when the Ivory Coast linked to the MIND network. Some countries joined MIND, others joined FIND, and some joined both. Despite minor differences between MIND and FIND, indicated earlier, we treat them as equivalent in this study. Figure 1 plots the number of countries connected to the MIND/FIND network from 2004 to 2011. During 2005, Switzerland and Liechtenstein were the first countries to join the network. By the end of 2006, Belgium, Lithuania, Spain, St. Kitts and Nevis, and Turkey had joined the network. The number of countries connected to the MIND/FIND network rose to 24 in 2007, 47 in 2008, 71 in 2009, 94 in 2010, and 102 in 2011.

In addition to the exact date of connection, IPGS also provided the number of searches by each member country for the years 2008 to 2011. The total number of MIND/FIND searches by member countries showed that some connected countries did not actually use the network and also that some formally unconnected countries made searches through their I-24/7 portal when prompted by suspicious behavior or international events. Figure 1 also plots the number of countries that carried out 1,000 or more annual searches using the MIND/FIND network. Such a mild threshold makes a difference in terms of the number of countries that actually used MIND/FIND and therefore can be considered as treated. Table 1 reports the number of country-year cases and average number of searches, classified according to whether countries were formally connected to MIND/FIND and whether the number of searches was above or below the 1,000 threshold. Table 1 shows that in 128 country-year cases, countries were formally connected to MIND/FIND but performed less than 1,000 searches on the network (on average 79 searches per year). These figures indicate that those countries although formally connected to MIND/FIND, did not use the network systematically. In addition, our sample includes seven country-year cases where countries, not formally connected to the network, performed more than 1,000 searches on the network through their I-24/7 portal (on average 897,974.9 searches). Although these countries were not MIND/FIND countries, their volume of searches is sufficiently high to suggest a more than casual use of the network. In summary, Table 1 shows that the 1,000 searches threshold identifies a more systematic use of the network than the connection status. Henceforth, we consider a country as treated when it actually used the MIND/FIND or I-24/7 network to perform a minimum of a thousand searches per year.

The time domain of our analysis runs from 2005 to 2011. It includes not only the 2008–2011 period for which the number of searches is available but also the 2005–2007 period with only information on MIND/FIND connection status but no information on the number of searches. A number of countries formally connected at some point during the 2005–2007 period have no searches (or searches below the 1,000 threshold) in 2008. It seemed reasonable to consider those cases as untreated.[1]

The outcome variable is the number of transnational terrorist incidents ending in a country, which is available from International Terrorism: Attributes of Terrorist Events (ITERATE) (Mickolus et al. 2012). ITERATE uses the news media to identify transnational terrorist incidents and their country venue. Although transnational terrorism is a concern for many countries, the MIND/FIND network is not primarily intended to curb transnational terrorism; rather, it is meant to reduce international crime (e.g., human trafficking). The number of transnational terrorist incidents is a count variable; its support is the set of non-negative integers. We account for this characteristic of the outcome variable using count regression methods. Countries vary considerably in size, thus having different exposures to transnational terrorism. Accordingly, we measure the outcome variable as the Transnational Terrorist Incidence Rate (TTIR), based on population as a proxy for country size and, therefore, exposure.

In addition to the treatment and outcome variables, other covariates of interest for our analysis are those that are determinants of the treatment status and the outcome variable. Enders and Sandler (2011) found that income per capita, population, and democratic freedoms were the main determinants of whether INTERPOL member countries installed MIND/FIND technologies. We gather Real GDP per capita and population data from the World Bank and we obtain a measure of democratic freedoms, *Polity*, from POLITY IV PROJECT (Marshall, Jaggers, and Gurr 2011).

We dropped from the analysis countries with incomplete data, resulting in a balanced panel of 141 countries as listed in Table 2. The temporal framework of the analysis takes the five years from 2000 to 2004 as the pre-treatment or pre-sample period, and uses years 2005 to 2011 as the sample or estimation period. The effect of MIND/FIND adoption and use on TTIRs is evaluated for the 2008–2011 period.

4 The methods

Let Y_{it} be the number of transnational terrorist incidents that takes place in country i during year t, which is a non-negative integer or count variable. The treatment status variable, D_{it}, equals one when country i uses MIND/FIND or the I-24/7 portal for a thousand or more searches in year t and equals zero otherwise. To assess the treatment effects, we use Rubin causal model (Rubin 1974, Sekhon

[1] These countries are Cambodia, Indonesia, Lao PDR, Lithuania, Malasya, South Africa, Thailand, Turkey, and Vietnam.

2007). Let Y_{it}^{1} be the *potential* number of transnational terrorist incidents under treatment, and let Y_{it}^{0} be the *potential* number of transnational terrorist incidents under no treatment. The observed number of transnational terrorist incidents, Y_{it}, is related to the potential numbers of transnational terrorist incidents according to $Y_{it} = Y_{it}^{0} + D_{it}\left(Y_{it}^{1} - Y_{it}^{0}\right)$.

We assume that the conditional expectation of the potential number of transnational terrorist incidents is exponential

$$E(Y_{it}^{j} \mid X_{it}, \eta_i, \lambda_t) = \exp\left(Z_{it}'\beta_j + \eta_i + \lambda_t + \ln(n_{it})\right), \tag{1}$$

for $j = 0,1$, where X_{it} is a vector of covariates, Z_{it} is a vector whose elements are a constant term and powers and interactions of the covariates, η_i is a time-invariant country-specific unobserved effect, λ_t is a unit-invariant period-specific unobserved effect, and n_{it} is the population of the country. The latter accounts for differential exposures across countries. Therefore, the conditional mean of the observed number of transnational terrorist incidents is

$$E(Y_{it} \mid X_{it}, D_{it}, \eta_i, \lambda_t) = \exp\left(Z_{it}'\beta_0 + D_{it}Z_{it}'\gamma + \eta_i + \lambda_t + \ln(n_{it})\right), \tag{2}$$

where $\gamma = \beta_1 - \beta_0$. Thus, the expected TTIR is $E(Y_{it} \mid X_{it}, D_{it}, \eta_i, \lambda_t)/n_{it}$, which is an exponential function of a vector of covariates.

A desirable feature of the specification in (2) is that it allows for different functional forms for treated and control units by including interactions with the treatment indicator. In addition, equation (2) permits a flexible parametric functional form by including powers and interactions of the covariates.

Although the MIND/FIND network was not devised as a counterterrorism tool, it can be argued that MIND/FIND adoption is not entirely exogenous for the analysis of transnational terrorism. Countries that have a record of transnational terrorist incidents in the past could to a greater degree decide to adopt MIND/FIND technology. Anecdotally, one of the first countries to adopt MIND/FIND, Spain, joined the network on 1 October 2006, not long after the 11 March 2004 train attacks in Madrid. Accounting for country-specific unobserved effects, η_i, is important because there might be unobserved factors that affect both the decision to join MIND/FIND and transnational terrorism incidents, therefore generating a problem of endogenous treatment. For example, one such factor could be the stance against transnational terrorism, which could affect both the adoption of MIND/FIND technology and the level of transnational terrorism. Thus, it is necessary to allow for dependence between the unobserved factors, η_i, and the treatment status, D_i.

We control for time-invariant unobserved heterogeneity, η_i, using various panel estimators: the Random Effects Poisson Quasi Maximum Likelihood estimator (REP-QML) and the Fixed Effects Poisson Maximum Likelihood estimator (FEP-QML) introduced by Hausman, Hall, and Griliches (1984), as well as the Pre-Sample Mean Generalized Method of Moments (PSM-GMM) estimator, suggested by Blundell, Griffith, and Windmeijer (2002).

The REP-QML estimator assumes independence between unobserved heterogeneity and regressors. Therefore, it might lead to inconsistent estimates if MIND/FIND adoption is triggered by unobserved factors that also influence the incidence of transnational terrorism. The FEP-QML estimator, however, allows for dependence between the unobserved factors and the MIND/FIND adoption indicator as well as the other regressors. Furthermore, although originally derived using strong distributional assumptions, the FEP-QML can be obtained under weaker assumptions: exponential mean and strictly exogenous regressors (Wooldridge 1999). While the FEP-QML estimator allows for arbitrary dependence among the unobserved factors and the treatment indicator, it restricts the analysis to units with positive number of counts (transnational terrorist events) during the sample period. As a result of this restriction, the sample is significantly reduced. This sample selection might be reasonable, as the there is no much point in analyzing the effect of MIND/FIND use on countries that do not experience transnational terrorism. Unfortunately, it can also be argued that the sample selection imposed by the FEP-QML estimator cannot account for the effect of MIND/FIND use on countries that had a small number of transnational terrorist events, say one or two events, prior to the treatment period, but then had no transnational terrorist events during the sample period and therefore are dropped from the estimation sample. If this reduction in transnational terrorist events is due to MIND/FIND use, we would then be dropping countries from the sample, for which the proportional reduction in transnational terrorist incidents is a hundred per cent.

The PSM-GMM estimator assumes that the country-specific effects can be accounted for by the pre-sample mean of the dependent variable (i.e., the number of transnational terrorist incidents during a pre-sample period). Let $Y_i^* = \frac{1}{T^*}\sum_{r=1}^{T^*} Y_{i,1-r}$ be the average number of transnational terrorist incidents ending in country i during the T^* periods before the beginning of the sample period. The PSM-GMM estimator assumes that

$$E(Y_{it} \mid X_{it}, D_{it}, Y_i^*, \lambda_t) = \exp\left(Z_{it}'\beta_0 + D_{it}Z_{it}'\gamma + \alpha \ln Y_i^* + \lambda_t + \ln(n_i)\right),$$

where α is a parameter to be estimated. By relying on the pre-sample mean of transnational terrorist events as a proxy for the country effects, the PSM-GMM estimator is not affected by the endogenous treatment bias, as the country effects are an additional regressor that can be correlated with the other regressors.

As the PSM-GMM estimator does not require the assumption of strict exogeneity of regressors and allows for predetermined ones, this estimator can also be applied to a lagged dependent variable model of the form suggested by Crépon and Duguet (1997):

$$E(Y_{it} \mid Y_{i,t-1}, X_{it}, D_{it}, Y_i^*, \lambda_t) = h(Y_{i,t-1}; \delta) \exp\left(Z_{it}'\beta_0 + D_{it}Z_{it}'\gamma + \alpha \ln Y_i^* + \lambda_t + \ln(n_i)\right),$$

where $h(Y_{i,t-1}; \delta) = \exp\{\delta d_{i,t-1}\}$ with $d_{i,t-1}$ being a dummy variable that equals one when the lagged number of transnational terrorism incidents is positive, $Y_{i,t-1} > 0$, and equals zero otherwise.

As the pre-sample mean of transnational terrorist incidents enters the exponential mean in logarithms, the PSM-GMM method restricts the sample to the set of countries with a positive number of transnational terrorist incidents during the pre-sample period. As in the case of the FEP-QML estimator, imposing this restriction reduces the sample significantly. However, the PSM-GMM analysis focuses on countries with some transnational terrorist incidents during the pre-sample (i.e., pre-treatment) period, while the number of transnational terrorist incidents during the sample period is unrestricted. It seems to us that this sample restriction constrains the analysis to a more natural sample than in the FEP-QML case.[2]

Our estimation methods rely on the assumption of random sampling under which the number of transnational terrorist incidents are independent across countries. In particular this assumption rules out the case where MIND/FIND adoption in one country has an effect on the number of transnational terrorist incidents in another country. In the treatment-effects literature, the absence of interference among units is labeled the Stable Unit Treatment Value Assumption (SUTVA). This assumption is likely violated in our analysis of the effect of MIND/FIND on transnational terrorism. There are at

[2] As an example, suppose you are interested in analyzing the effect of some retroviral pharmaceutical on viral load. It seems pretty obvious that, in this case, you certainly want to use a sample of previously virus-infected individuals for your analysis. Similarly, the PSM-GMM estimator focuses on a sample of countries that experienced some transnational terrorist incident prior to the treatment period.

least two reasons why the no-interference assumption may not hold in opposing directions. First, the War on Terror is considered to be a weakest-link public good problem (Enders and Sandler 2012); that is, world security depends on the level of security in the least-secure country. Therefore, it could be argued that MIND/FIND adoption by a country might deflect some transnational terrorist incidents to countries, where this technology is not used, thus reducing the TTIR in treated countries and increasing the TTIR in untreated countries. This violation of the no-interference assumption is not as problematic as one might think. Interference among units occurs when treatment applied to some units affects untreated units as well. Typically, when interference occurs, the treatment affects the treated and the control units in the same direction, but usually in different magnitudes.[3] In this situation, if we use as a comparison group whose units are affected by the treatment, we may erroneously conclude that there is no causal effect, or that the effect is smaller than it actually is. However, if the weakest-link effect exists, treatment in one country causes more transnational terrorist incidents in untreated ones, so that treatment affects treated and control units in the opposite direction. As a result, interference biases the treatment effect upwards, but it cannot be the case that we conclude there is no causal effect when there is one. Therefore, under the assumption that MIND/FIND adoption in treated countries increases the number of transnational terrorist events in untreated ones, part of the estimated treatment effect should be attributed to the effect of the treatment on the control group rather than on the treated.

Second, another violation of the no-interference assumption might occur if MIND/FIND adoption generates peer effects, whereby countries benefit from tighter border controls elsewhere. Here, the effect of the treatment applied to a country affects the outcome of other countries, either treated or not, in the opposite direction from the weakest-link effect. For MIND/FIND, the peer effect may be associated, in part, with the hub-spoke system of air travel, where passengers must travel through major hubs (e.g., Heathrow in London, Charles de Gaulle in Paris, or Frankfurt Airport in Germany) to get to their final destination. If major hub airports are in MIND/FIND treatment countries, then peer effects will be more prevalent. One can envision that much less than 100 per cent MIND/FIND treatment may effectively protect all nations, not unlike the concept of herd immunity for contagious diseases. As the number of MIND/FIND countries grows in our later sample years, peer effects are more of a concern. Testing for the presence of peer effects is difficult, see Angrist and Pischke (2009, pp. 192-197). To illustrate this difficulty, suppose that peer effects are proportional to the average number of searches across countries, so that the greater the overall use of MIND/FIND, the greater the benefit to any individual country. Also suppose that, to identify this form of peer effects we include the average number of searches as an additional regressor in our models. This strategy would not help to identify the peer effect if there are other country-invariant shocks that affect transnational terrorism and are correlated with the average number of searches. However, despite this difficulty, we can still account for the presence of peer effects and other time-varying country-invariant shocks by including time dummies in our regressions. In particular we parameterize the time-effect $\lambda_t = \sum_{s=1}^{T} \delta_s d_{st}$ where d_{st} is a dummy variable that takes the value one when $s = t$ and zero otherwise. By so doing, we are not able to identify the extent of peer effects but we are able to free our estimates from biases accruing from the existence of such effects.

In computing treatment effects, we follow Lee and Kobayashi (2001), who proposed the conditional Proportional Average Treatment Effect (PATE)

[3] As an example, Rosenbaum (2007) argues that "vaccinating one child may prevent her from contracting a viral infection and spreading it to her unvaccinated brother." That is, vaccination, the treatment, reduces the chances of a viral infection for the treated child and the untreated brother, although in the latter case probably to a lesser extent.

$$PATE(X_{it}) = \frac{E(Y_{it}^1 - Y_{it}^0 \mid X_{it}, \eta_i, \lambda_t)}{E(Y_{it}^0 \mid X_{it}, \eta_i, \lambda_t)} = \exp\left(Z_{it}'\gamma\right) - 1.$$

This is a particularly appropriate measure of the treatment effects for exponential conditional mean models. It does not depend on unobserved heterogeneity or time effects as they cancel out in computing the ratio. The conditional PATE depends on the values of the covariates, so it requires evaluating the covariates at a particular point. To integrate out the covariates, we compute, for a given year, the geometric mean to obtain the unconditional PATE,

$$\left\{\prod_{i=1}^{N} \exp\left(Z_{it}'\gamma\right)\right\}^{1/N} - 1 = \exp\left(\overline{Z}_t'\gamma\right) - 1,$$

where $\overline{Z}_t = \frac{1}{N}\sum_{i=1}^{N} Z_{it}$. Thus, the unconditional PATE is equal to the conditional PATE with (transformed) covariates evaluated at their sample means. Similarly, we can compute the geometric mean for the treated units to obtain the PATE on the treated (PATET),

$$\prod_{i=1}^{N} \left\{\exp\left(Z_i'\gamma\right)\right\}^{\frac{D_{it}}{N_{1t}}} - 1 = \exp\left(\overline{Z}_{1t}'\gamma\right) - 1,$$

where $N_{1t} = \sum_{i=1}^{N} D_{it}$ is the number of treated units and $\overline{Z}_{1t} = \frac{1}{N_{1t}}\sum_{i=1}^{N} D_{it} Z_{it}$ the average value of Z for the treated.

The arithmetic average of the unconditional PATE would, by Jensen's inequality, return a bigger value; hence, by using the geometric average instead of the arithmetic mean, we are on the conservative side. We also note that the PATE is bounded below by -1, because the number of transnational terrorist incidents cannot be negative. Thus, when later reporting PATE estimates, we use the asymmetric confidence interval suggested by Lee and Kobayashi (2001).

5 The results

Columns (1)-(4) of Table 3 report panel regressions estimates for the period 2005–2011 using REP-QML, FEP-QML, and PSM-GMM for a static model, and PSM-GMM for the lagged dependent variable model respectively. These panel estimators account for unobserved heterogeneity in different ways. The REP-QML estimates use data for 141 countries for a total of 987 observations, and account for unobserved heterogeneity, but do not allow for dependence between the unobserved heterogeneity and the treatment indicator. The FEP-QML estimator wipes out the country-specific unobserved effects by conditioning on the sum of transnational terrorism incidents for each country over the sample period, so that it allows for arbitrary dependence between unobserved factors and the treatment indicator. The FEP-QML estimates are conditional on having at least one transnational terrorist incident during the sample period, which results in a sample of 399 observations from 57 countries. The PSM-GMM estimates use pre-sample information (the pre-sample mean of the number of transnational terrorist incidents) to construct a proxy for the country fixed effects. Because the country-specific pre-sample mean enters the exponential mean in logarithms, the analysis is restricted to all countries that have at least one transnational terrorist incident during the pre-sample period (2000–2004) for a total of 539 observations from 77 countries.

Table 3 reports the exponential regression estimates using flexible parametric functional forms. The estimates reported correspond to functional forms obtained after a careful specification search procedure. Details about the procedure and intermediate steps are reported in an online appendix. In essence, the specification search procedure is as follows. We start with an initial specification including the covariates, (log) GDP per capita, (log) population, and the polity score, together with their interactions with the treatment indicator. By including interactions with the treatment indicator, we allow for different regression functions for the treated and the untreated. The final specification reported in Table 3 is obtained after several rounds of regressions. At each round, a set of exponential regressions is estimated, each of these regressions includes all terms included in the previous round plus a pair of nonlinear terms: a squared covariate or interaction of two covariates and its interaction with the treatment indicator. Among all regressions in a round, we select the most statistically significant pair of non-linear term and interaction with the treatment indicator, which is then included in the next round of regressions. When no more pairs of terms turn out to be significant, we run a final round of regressions, in each of which an insignificant term is eliminated. The resulting regressions are reported in Table 3 where all regressors are statistically significant at the ten per cent or lower significance level. Retaining only significant terms is important for the estimation of the treatment effects, which could otherwise be affected by large but insignificant parameter estimates.

Table 4 reports PATET estimates across the 2008–2011 period along with lower and upper confidence interval bounds. For instance, the PATET for 2008 using the REP-QML estimator is -0.2635, a 26.35 per cent reduction in the number of transnational terrorist incidents, which is a statistically significant estimate because the associated confidence interval does not include zero. Similarly, the estimated PATETs for subsequent years indicate a significant 25 or 26 per cent reduction in TTIRs. The PATET estimates from the FEP-QML estimator range from a low of -0.2795 in 2009 to a high of -0.4530 in 2008. Despite the fact that the FEP-QML estimates account for arbitrary dependence between unobserved heterogeneity and the treatment indicator while the REP-QML does not, the estimated PATETs are not that different. As there are no big differences in proportional treatment effects among the REP-QML and FEP-QML estimates, one is tempted to conclude that the selection bias might not be that important. Notice, however, that the FEP-QML estimates are conditional on having at least one transnational terrorist incident during the sample period, while the REP-QML estimates refer to a sample of countries that, in addition to the 57 countries used in the FEP-QML estimation, includes 84 additional countries with no transnational terrorism incidents during the sample period. In other words, the proportional treatment effects obtained from the REP-QML and FEP-QML estimates are of similar magnitude, but the REP-QML estimates refer to a sample with a much lower number of transnational terrorism events. Therefore, the proportional treatment effects obtained from the FEP-QML estimates suggest a deeper reduction in TTIRs as a result of MIND/FIND use.

As argued above, it might be more reasonable to base our inference on a sample of countries with some pre-treatment transnational terrorism incidents, which is exactly what the PSM-GMM method does. The PSM-GMM PATET estimates using the static and dynamic specifications are all significant. Using the static specification, we find that the estimated PATET ranges from -0.1854 in 2011 to -0.4098 in 2008. Based on the dynamic specification, the PATET ranges from -0.2835 in 2009 to -0.3315 in 2011. Overall, we find negative and significant PATET estimates. This findings indicate a significant proportional reduction of transnational terrorism as a result of using the MIND/FIND network for searches.

Proportional treatment effects are to be interpreted relative to the number of transnational terrorist events. To provide an idea of the magnitude of the effects, we compute expected TTIRs under treatment and under no treatment for the treated. Computing the expected TTIRs is not possible for the

REP-QML and FEP-QML estimators, which do not provide estimates of the country effects, while the PSM-GMM estimators do allow us to estimate expected TTIRs. For the static specification, we compute the expected TTIR in year t under treatment and under no treatment for the treated as the geometric average of the corresponding exponential mean function, which is equal to the exponential function evaluated at the arithmetic average of the covariates across treated countries. Table 5 reports the expected TTIRs under treatment, under no treatment, and their difference, which is an estimate of the treatment effect. These quantities vary depending on whether we use estimates from the static or dynamic specifications. According to the estimates from the static model as of 2008, treated countries experienced a reduction of 0.8208 transnational terrorist incidents per 100 million people. This figure fell in subsequent years to 0.5267, 0.4029, and 0.3468. According to the estimates from the dynamic model, the effect of MIND/FIND use on TTIRs is initially smaller, 0.5069 fewer transnational terrorism incidents per 100 million people in 2008, fell to 0.4366 in 2009, but then rose to 0.5279 in 2010 and 0.5604 in 2011.

We note that our estimates of the TTIRs and treatment effects are conservative because we use the geometric average of the exponential mean function instead of the arithmetic mean, which would, by Jensen's inequality, result in larger estimates. Table 6 reports estimates of the expected TTIRs for the treated obtained as the arithmetic average of the exponential mean function. As expected, estimated TTIRs are a lot larger than when using the geometric average. Differences between the TTIRs under treatment and under no treatment suggest treated countries experienced from 0.9860 to 2.4435 fewer transnational terrorist incident than under no treatment.

6 Adjusting for covariate imbalance

An important concern involves covariate balance. When the effect of a treatment is estimated using a randomized experiment, the distributions of the variables that affect the outcome are balanced across treatment and control groups, at least ex ante. However, in our application, countries are not randomly assigned to these two groups, which results in an uneven distribution of covariates across treatment and control groups. To address this problem, we perform a covariate rebalancing pre-analysis, constructing control groups that match as closely as possible the characteristics of the treatment group. Covariate rebalancing would be easy if the analysis was cross-sectional or, alternatively, if the group of treated countries was the same for every period after the initial treatment period. In our panel setup, the set of treated countries changes across periods posing an extra difficulty for covariate rebalancing. We choose to analyze covariate balance when the treatment group is defined as the set of countries that adopted MIND/FIND technology and used it as of the end of 2008, the midpoint of our sample period.

To form balanced treatment and control groups, we apply the method of matching on the propensity score (Rosenbaum and Rubin 1983). The propensity score function, $p(X) = P(D = 1 \mid X)$, is the probability of adopting the MIND/FIND technology, conditional on the vector of covariates X. Enders and Sandler (2011) showed that the probability of observing that a country adopts the MIND/FIND technology depended on income per capita, population, and democratic freedoms. Based on these treatment-status determinants, Table 7 reports estimates of the propensity score using a logit specification. As in Enders and Sandler (2011), we model the probability of receiving treatment as of the end of 2008. However, because we restrict the analysis to countries with available data on transnational terrorism, the sample is smaller than theirs. Column (1) uses contemporaneous values of the covariates, as in Enders and Sandler (2011), to predict the probability of receiving treatment when treatment is defined according to connectivity. Analogous to Enders and Sandler (2011), we use the connection status as the treatment indicator and include a Polity dummy as a regressor instead of the Polity score itself. The Polity dummy equals one when the Polity score is seven or larger. The results are qualitatively equivalent to those obtained by Enders and Sandler (2011), with income per capita, population, and a Polity dummy having a positive and statistically significant effect on the probability of receiving treatment (i.e., adopting MIND/FIND). Column (2) differs from Column (1) since treatment status is defined according to the criterion of performing at least 1,000 annual searches. This definitional change of treatment has an effect on the significance of the covariates; income per capita remains significant and the Polity dummy is marginally significant, but population loses its significance. The actual Polity score, however, turns out to be significant in Column (3). The binary choice models, estimated by Enders and Sandler (2011), were not meant to be used for inference on the treatment effects of MIND/FIND adoption. However, in the treatment-effects literature, it is typically recommended that covariate values should correspond to the pre-treatment period. In addition, the set of covariates should include pre-treatment values of the outcome to account for unobserved pre-treatment heterogeneity. In this regard, Column (4) reports logit estimates when covariates are dated as of 2004, the last year before MIND/FIND began functioning. Income per capita, population, and the Polity score have qualitatively identical effects on the probability of receiving treatment. Column (5) also includes the average number of transnational terrorist incidents during the 2000-2004 period as an additional regressor, which turns out to be insignificant. Taking Column (5) as our benchmark specification, we looked for significant nonlinear terms (squares and interactions) of the covariates. To arrive at the specification reported in Column (6), we run a specification search procedure similar to the one used for the conditional exponential mean function. The procedure consists of several rounds of logistic regressions. In each round, we run a set of logistic regressions, each of which includes all the covariates in Column (5) plus an additional nonlinear term (squares or interactions). Among all the logistic regressions in a round, we select the one that includes the most statistically significant additional nonlinear term, which is then included in all logistic regressions in the next round. The procedure ends when no additional significant terms are found. In a first round of regressions, the squared value of the pre-sample average number of transnational terrorist incidents turned out to be significant and corresponded to the highest increase in the log-likelihood function. This logistic regression is reported in Column (6). We then run a second round of logistic regressions, each of which included all the covariates in Column (6) plus an additional nonlinear term. The specification selected in the second round is reported in Column (7), which includes the squared Polity score as an additional regressor, which turns out to be significant and results in the highest increase in the log-likelihood value. A further round of logistic regressions of adding nonlinear terms to the specification reported in Column (7) does not find any significant term nor noteworthy increase in the log-likelihood function. This final specification of the propensity score is next used to match treated countries with untreated ones.

Table 8 reports statistics on the distribution of pre-treatment values of covariates, the propensity score, and the log odds ratio across treatment groups for the two samples, the full 141-country sample and the 77-country sample with positive pre-sample average number of transnational terrorist events. The 57-country sample with a positive number of transnational terrorist events during the sample period, used for the FEP-QML estimation, is sufficiently small to preclude pre-analysis covariate rebalancing. In each of these samples, the treated group and the group where all controls are included exhibit differences in means and standard deviation (SD), especially for the Polity score and the log odds ratio. In addition to mean and standard deviation statistics, Table 8 also reports additional statistics that look at more subtle differences in covariate distributions across treatment groups. The overlap rate is the proportion of observations in the treatment group that fall within the 0.025 and 0.975 quantiles of the empirical distribution of the control group, thus 95 per cent of the observations in the treatment group have Log GDP per capita values that fall within the $(0.025, 0.975)$ quantile interval of the empirical distribution of Log GDP per capita among countries in the control group. A high overlap rate for the treatment group indicates that it is reasonably easy to find countries in the control group with covariate values similar to those in the treatment group. When all controls are considered, overlap rates are fairly high except for the propensity score and the log odds ratio, $\log(p(X)/(1-p(X))$. The standarized mean difference (SMD) is defined as $(\bar{x}_1 - \bar{x}_0)/\sqrt{(\widehat{\sigma}_1^2 + \widehat{\sigma}_0^2)/2}$, where $\bar{x}_g$ and $\widehat{\sigma}_g$, $g = 0, 1$ are the sample mean and the standard deviation of covariate x for the two groups. The latter measures the extent to which the two groups differ in mean values. Values above 0.25 should warn of important differences in means, which is the case for most covariates. Finally, the log ratio of standard deviations (LRSD) is defined as $\log\left(\frac{\widehat{\sigma}_1}{\widehat{\sigma}_0}\right)$, with big absolute values indicating differences in dispersion across groups. With some exceptions, there are large differences in the dispersion of covariates across treatment groups. In sum, covariate distributions across treatment groups show a high degree of imbalance.

Poor covariate balance is typical of observational studies and our analysis of the effect of MIND/FIND use on transnational terrorism is no exception. In order to cope with this problem, we follow standard procedures for covariate balancing, (e.g., Rosenbaum 2010; Imbens and Rubin (forthcoming)). The idea behind these methods is to use matching techniques to ensure balance in covariate distributions before the actual estimation of the treatment effects is carried out. Accordingly, we match each treated unit (country using MIND/FIND) with an untreated unit from the control group. Each treated country is matched with an untreated country, using the propensity score and the log odds ratio. First, we order units according to the probability of receiving treatment in decreasing propensity score order. Second, for the first treated unit (the one with highest estimated propensity score), we find the unit in the control group with the closest log odds ratio. Then, we do the same for the second treated unit, and continue in this fashion until we find a match for each unit in the treatment group. Matches are based on the log odds ratio rather than the propensity score because, as argued by Imbens and Rubin (forthcoming), the log odds ratio is linear in the covariates, whereas the propensity score is not. In other words, differences in the propensity scores between treatment and control units are not equally important across all values of the propensity score. For instance, differences in propensity scores of 0.01 and 0.02 are more important than differences in propensity scores of 0.49 and 0.50, while this is not the case with the log odds ratio.

The third set of columns in Table 8 reports overlap rates, SMD, and LRSD for the matched samples. Although overlap rates decrease in some cases, they increase for the propensity score and log odds ratio, which are the variables that had the lowest overlap rates with the full set of controls. Standarized mean differences and the log ratio of standard deviations are much smaller, with only a few cases above the 0.25 yardstick. Figure 2 plots the distribution of the log odds ratio for the two samples using all controls and the matched set of controls. Covariate rebalancing does make a noticeable difference, as the matched samples exhibit a more similar distribution across treatments than those that use all controls. Although matching makes treatment and control groups much more similar, the small sample size precludes better pre-analysis covariate rebalancing. In any case, covariate rebalancing makes subsequent evidence more credible for the matched controls than using all controls. It should be noted, however, that the matching is done as of the midpoint of the estimation period, year 2008. Therefore, covariate balance before or after the midpoint is surely poorer.

Once equipped with the matched samples we repeat the analysis estimating exponential mean functions using the REP-QML and the PSM-GMM methods. We first run specification search as explained previously. Details on this procedure and intermediate steps are given in the online appendix.

Table 9 reports the final specifications estimates. When using the REP-QML estimator, the specification search ends disregarding all covariates, powers, and interactions, while retaining the treatment indicator, which exhibits a negative and significant estimate. The final specification reported for the PSM-GMM estimator includes the treatment indicator together with other covariates, powers, and interactions. The specification search for the lagged dependent variable estimator ended up disregarding as insignificant the lagged dependent variable term and validating precisely the same specification found with the static model. It is not surprising that after rebalancing the specification search procedure delivers estimated conditional means with less significant terms. In fact, had the assignment to treatment been randomized, a simple mean comparison across treatment groups would suffice as there would be no need to account for covariates.

Table 10 reports the associated proportional treatment effects for the treated. According to the REP-QML estimates, the PATET is a significant -0.4474 and constant across years. PATET estimates using the PSM-GMM estimation method range from -0.6084 in 2009 to -0.6272 in 2008. After covariate rebalancing, PATET estimates roughly double those previously reported. Accordingly, we find a much higher reduction in the number of transnational terrorist incidents. Table 11 reports TTIRs for the treated and their change attributable to treatment. As a consequence of MIND/FIND or INTERPOL surveillance use, the thirteen treated countries experienced 0.56 fewer transnational terrorist incidents per 100 million people in 2008 and as much as 1.85 fewer incidents per 100 million people in 2010. Over the 2008–2011 period, these countries had 1.23 fewer transnational terrorist incidents per 100 million people each year on average.

According to our findings, among those countries that experienced some transnational terrorist incidents during 2000–2004, the thirteen MIND/FIND-using countries experienced a significant reduction in transnational terrorist incidents during 2008–2011. One is tempted to conclude that greater MIND/FIND use would reduce transnational terrorist incidents in newly adopting countries. Making such statement, however, requires a further step. We have estimated the proportional treatment effects that would have been experienced by untreated countries in our matched sample, had they used MIND/FIND for border control. Table 12 shows that the PATE for the untreated countries in our matched sample runs from a low -0.2819 in 2009 to a high -0.3744 in 2008. Therefore, we can claim that, at least for the untreated countries in our matched sample, MIND/FIND use would lead to fewer transnational terrorist incidents, but not as many as for the treated group, which is further evidence that treatment matters in our study of INTERPOL surveillance.

7 Discussion

Our analysis indicates that INTERPOL countries that adopted MIND/FIND and also applied it to screen people and documents at border crossings and other key points suffered fewer transnational terrorist incidents than the control group, which either did not install MIND/FIND or else installed it but did not utilize it. Our estimates indicate that a country with 64 million people, like France in 2008, would on average experience 0.79 fewer transnational terrorist incidents each year as a result of using MIND/FIND.[4] This terrorism reduction might not seem like a lot, but it represents for most countries a sizeable proportional reduction in transnational terrorist incidents of about 60 per cent. Globally, this can translate into quite a reduction in attacks.

Although each transnational terrorist incident kills one person and injuries two on average, the

[4]Back-of-the-envelope calculation using the average reduction in transnational terrorist incidents during the 2008–2011 period gives: expected reduction in TTIR$\times$hundreds of millions population $=-1.23 * (64.37/100) = -0.79$.

reduction of these incidents through MIND/FIND means the potential capture of terrorists and the disruption of terrorist groups. Additionally, on occasion a terrorist spectacular, such as 9/11 or the Madrid commuter train bombings in 2004, may be stopped where the payoff is huge. However, we would argue that the payback is large even for small incidents, since curbing such incidents reduces media coverage of attacks, limits terrorist recruitment, and curtails society's anxiety. Moreover, a fall in transnational terrorist incidents worldwide allows countries to reduce somewhat homeland security spending. Non-adopting countries will come to realize that terrorists will transfer attacks to their soil, which should eventually foster more universal adoption of MIND/FIND.

The hub-spoke system of international air travel means that less than universal adoption of MIND/FIND may be sufficient to greatly curb transnational terrorist attacks, in which borders are transgressed by the perpetrator. Unfortunately, MIND/FIND cannot eliminate transnational terrorist incidents where a perpetrator attacks foreign assets (i.e., people or property) for political purposes on his/her home soil. Moreover, MIND/FIND cannot stop the transit of would-be terrorists, who are not in INTERPOL or national data banks as suspected terrorists. Thus, MIND/FIND can ameliorate transnational terrorism, as shown here, but it cannot eliminate it. A potential downside of MIND/FIND may be an increase in domestic terrorism, so that the authorities must be vigilant for this transference as MIND/FIND use expands. For domestic terrorist attacks, countries possess the proper incentives to take proactive measures because any resulting benefits are fully captured by the acting country – there are no transnational externalities (Enders and Sandler 2012). Thus, MIND/FIND-induced domestic transference of attacks does not pose too much of a concern.

Finally, we want to put the associated INTERPOL costs into perspective. The entire operating budget of INTERPOL was 60 million euros in 2011 (INTERPOL 2011). Based on past percentages calculated by Sandler, Arce, and Enders (2011) for 2006 and 2007, about 23 per cent of INTERPOL's budget goes to coordinating the fight against terrorism. This is a high-end estimate because these authors wanted to err on the high side to give more credence to their benefit-cost computations. For example, they included the entire costs of I-24/7 in INTERPOL's efforts to address terrorism. If we use their percentage, then less than 13.8 million euros were spent by INTERPOL in 2011 on assisting its member countries' counterterrorism activities. Of course, member countries using MIND/FIND have initial setup costs before their border officials can start using the technology and databases for searches. Nevertheless, the associated costs are minuscule compared to the tens of billions that the United States alone spends on homeland security. In 2012, the US Department of Homeland Security budget was about $60 billion, of which about 66 per cent or $40 billion went to defensive measures against terrorism (Enders and Sandler 2012). Our analysis shows that the small INTERPOL costs have huge paybacks in thwarting transnational terrorism as borders are made more secure.

Data Appendix

- Transnational terrorist events. Yearly number of terrorist events ending in a particular country or territory. Source: ITERATE data base (Mickolus et al. 2012). Please not that the number of transnational terrorist events ending in Cabinda were added to Angolan attacks, those ending in Canary Islands were added to Spanish attacks, those ending in Corsica were added to French attacks, those ending in Scotland and Northern Ireland were added to the United Kingdom attacks, and those ending in Dubai were added to United Arab Emirates attacks.

- Gross Domestic Product per capita. GDP per capita, PPP, constant 2005 international $. Source: World Bank.

- Population. Total population. Source: World Bank.

- Polity score. Polity2 variable. Source: POLITY IV PROJECT (Marshall, Jaggers, and Gurr 2011)..

- MIND/FIND data. Connection dates and number of searches. Source: INTERPOL General Secretariat.

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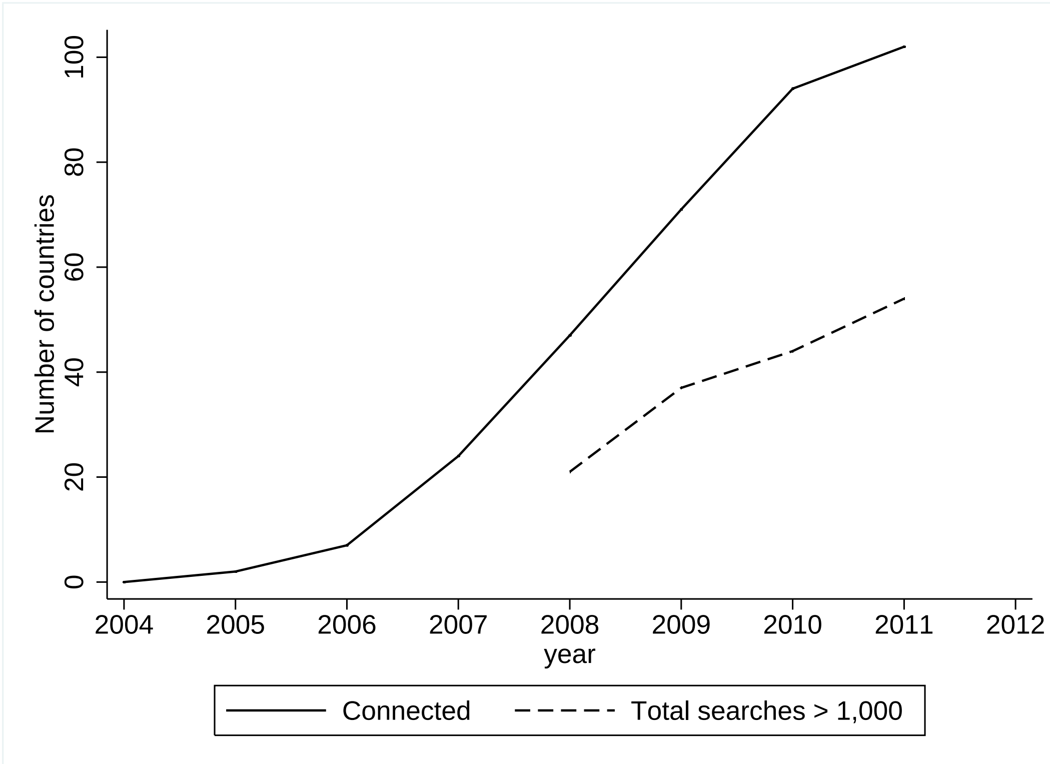

Figure 1: Number of countries connected to MIND/FIND network and number of countries with total searches above 1,000.

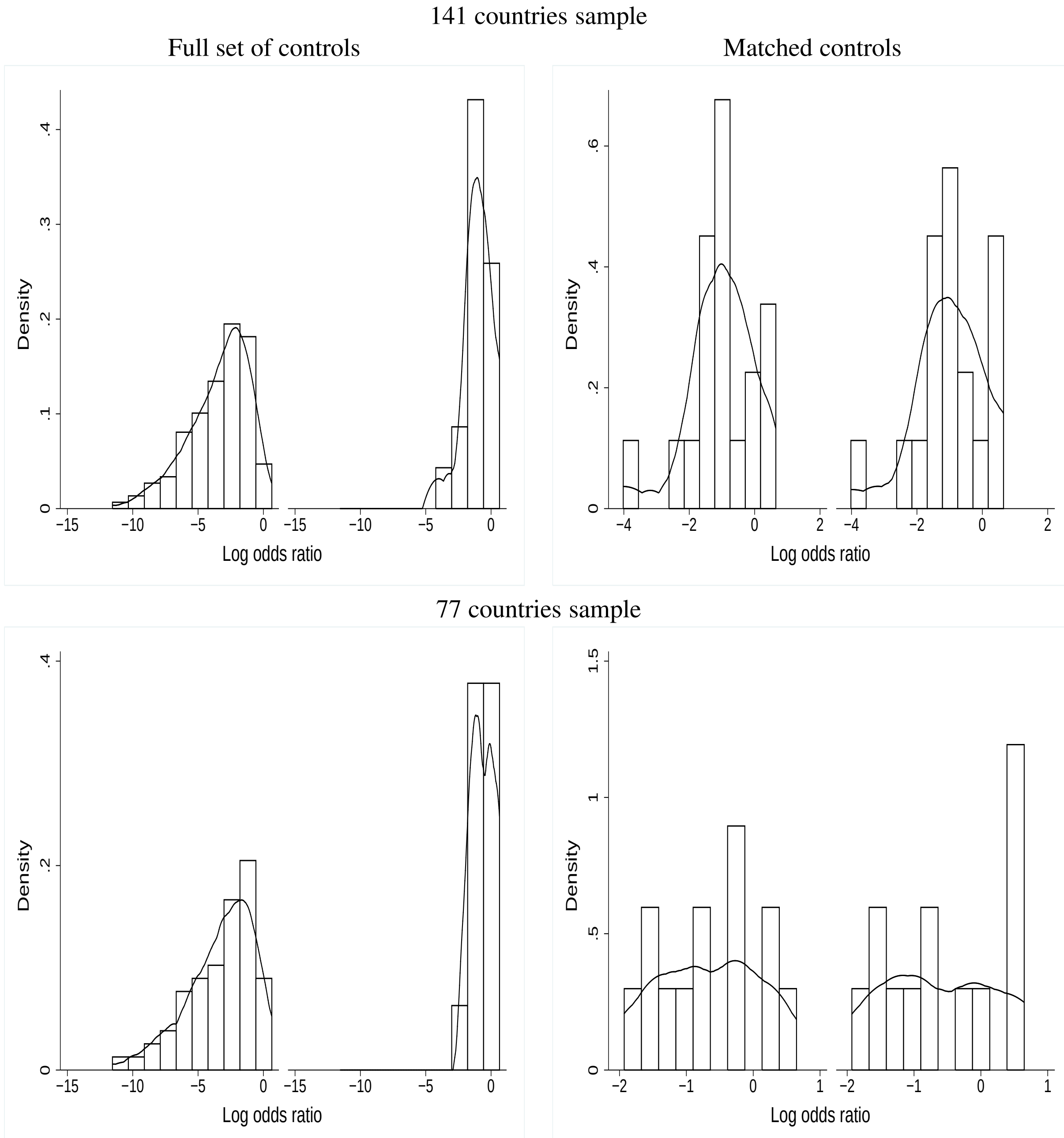

Figure 2: Log odds ratio histogram and density with all controls and matched controls. Each graph plots the distribution of the log odds ratio for the treated (right panel) and the control group (left panel).

Table 1: Number of searches and MIND/FIND connection status

	Connected		Not connected	
	Average Searches	Country-year cases	Average Searches	Country-year cases
Number of searches $\geq$ 1,000	10,924,717.0	136	897,974.9	7
Number of searches < 1,000	79.0	128	2.9	321

Table 2: List of Countries

Albania, Algeria, Angola, Armenia, Australia, Austria, Azerbaijan, Bahrain, Bangladesh, Belarus, Belgium, Benin, Bhutan, Bolivia, Botswana, Brazil, Bulgaria, Burkina, Faso, Burundi, Cambodia, Cameroon, Canada, Cape Verde, Central African Republic, Chad, Chile, China, Colombia, Comoros, Republic of the Congo, Costa Rica, Cote d'Ivoire, Croatia, Cyprus, Czech Republic, Denmark, Ecuador, Egypt, El Salvador, Equatorial Guinea, Eritrea, Estonia, Ethiopia, Fiji, Finland, France, Gabon, Gambia, Georgia, Germany, Ghana, Greece, Guatemala, Guinea, Guinea-Bissau, Guyana, Haiti, Honduras, Hungary, India, Indonesia, Ireland, Israel, Italy, Japan, Jordan, Kazakhstan, Kenya, Republic of Korea, Kuwait, Kyrgyz Republic, Lao PDR, Latvia, Lesotho, Liberia, Lithuania, Macedonia FYR, Madagascar, Malawi, Malaysia, Mali, Mauritania, Mauritius, Mexico, Moldova, Mongolia, Morocco, Mozambique, Namibia, Nepal, Netherlands, New Zealand, Nicaragua, Niger, Nigeria, Norway, Oman, Pakistan, Panama, Papua New Guinea, Paraguay, Peru, Philippines, Poland, Portugal, Qatar, Romania, Russian Federation, Rwanda, Saudi Arabia, Senegal, Sierra Leone, Singapore, Slovak Republic, Slovenia, Solomon Islands, South Africa, Spain, Sri Lanka, Sudan, Suriname, Swaziland, Sweden, Switzerland, Tajikistan, Tanzania, Thailand, Togo, Trinidad and Tobago, Turkey, Uganda, Ukraine, United Arab Emirates, United Kingdom, United States, Uruguay, Uzbekistan, Venezuela RB, Vietnam, Republic of Yemen, Zambia.

Table 3: Model estimates using different estimation methods

	(1)	(2)	(3)	(4)
	REP-QML	FEP-QML	PSM-GMM	PSM-GMM
Dummy Lagged Trans. Terror Incidents				1.2245***
				(0.2379)
Log GDP pc	4.3899***	-3.6665***	5.4184***	4.3420***
	(1.6647)	(1.1062)	(1.3084)	(1.1808)
Log Population	6.7313***		2.1918***	1.5208**
	(2.6083)		(0.6817)	(0.6046)
Polity score	-0.8355***	1.2149***	-0.3629**	-0.2275*
	(0.2512)	(0.4182)	(0.1618)	(0.1374)
Log Population squared	-0.1429**			
	(0.0684)			
Polity score squared			-0.0221***	-0.0174***
			(0.0059)	(0.0052)
Log GDP pc $\times$ Log Pop.	-0.2548**		-0.3199***	-0.2562***
	(0.0992)		(0.0774)	(0.0700)
Log GDP pc $\times$ Polity score		-0.1535***	0.0465**	0.0297*
		(0.0531)	(0.0193)	(0.0159)
Log Pop. $\times$ Polity score	0.0473***			
	(0.0144)			
Treatment	31.5238**	50.2436**	94.9887***	85.3380***
	(13.2842)	(23.9211)	(20.8831)	(24.6531)
Treat. $\times$ Log GDP pc	-7.0165**	-10.7331**	-14.2888***	-9.0549***
	(2.9412)	(5.0947)	(2.1939)	(2.6161)
Treat. $\times$ Log Population			-3.7867***	-4.8737***
			(1.4407)	(1.4648)
Treat. $\times$ Polity score			0.4384*	
			(0.2484)	
Treat. $\times$ Log GDP pc squared	0.3806**	0.5611**	0.3967***	
	(0.1611)	(0.2696)	(0.1039)	
Treat. $\times$ Polity score squared			0.0190**	0.0194***
			(0.0080)	(0.0068)
Treat. $\times$ Log GDP pc $\times$ Log Pop.			0.4185***	0.5079***
			(0.1500)	(0.1550)
Treat. $\times$ Log GDP pc $\times$ Polity score			-0.0518*	
			(0.0279)	
Pre-Sample Mean Trans. Terror Incidents			0.4741***	0.3208***
			(0.0969)	(0.1069)
Constant	-77.7209***		-40.4420***	-29.7967***
	(26.4761)		(11.5677)	(10.2754)
Number of observations	987	399	539	539
Number of countries	141	57	77	77

Standard errors in parentheses, *** p-value<0.01, ** p-value<0.05, * p-value<0.1
All regressions include year dummies.

Table 4: Proportional Average Treatment Effects on the Treated

Year	2008	2009	2010	2011
Random Effects Poisson (REP-QML)				
PATET	-0.2635	-0.2536	-0.2635	-0.2595
CI lower bound	-0.2738	-0.2642	-0.2739	-0.2702
CI upper bound	-0.2531	-0.2429	-0.2530	-0.2488
Fixed Effects Poisson (FEP-QML)				
PATET	-0.4530	-0.2795	-0.3108	-0.3633
CI lower bound	-0.4669	-0.3001	-0.3299	-0.3806
CI upper bound	-0.4389	-0.2586	-0.2914	-0.3457
Pre-Sample Mean GMM - Static				
PATET	-0.4098	-0.2815	-0.1970	-0.1854
CI lower bound	-0.4244	-0.2984	-0.2161	-0.2056
CI upper bound	-0.3950	-0.2644	-0.1778	-0.1649
Pre-Sample Mean GMM - Lagged dependent variable				
PATET	-0.2982	-0.2835	-0.3160	-0.3315
CI lower bound	-0.3167	-0.3012	-0.3333	-0.3496
CI upper bound	-0.2795	-0.2656	-0.2984	-0.3132

Table 5: Expected Transnational Terrorism Incident Rates (TTIRs) per 100 million people. Geometric average estimates.

Year	2008	2009	2010	2011
Pre-Sample Mean GMM - Static				
Expected TTIR under treatment	1.1821	1.3443	1.6417	1.5241
Expected TTIR under no treatment	2.0029	1.8710	2.0446	1.8709
Difference in TTIRs	-0.8208	-0.5267	-0.4029	-0.3468
Pre-Sample Mean GMM - Lagged dependent variable				
Expected TTIR under treatment	1.1928	1.1034	1.1428	1.1298
Expected TTIR under no treatment	1.6997	1.5400	1.6707	1.6902
Difference in TTIRs	-0.5069	-0.4366	-0.5279	-0.5604

Table 6: Expected Transnational Terrorism Incident Rates (TTIRs) per 100 million people. Arithmetic average estimates.

Year	2008	2009	2010	2011
Pre-Sample Mean GMM - Static				
Expected TTIR under treatment	1.4555	2.1277	2.6255	2.5207
Expected TTIR under no treatment	3.8990	3.6139	4.1102	4.0656
Difference in TTIRs	-2.4435	-1.4862	-1.4848	-1.5450
Pre-Sample Mean GMM - Lagged dependent variable				
Expected TTIR under treatment	1.4474	1.5818	2.0434	2.4104
Expected TTIR under no treatment	2.7404	2.9512	3.9158	3.3964
Difference in TTIRs	-1.2930	-1.3693	-1.8724	-0.9860

Table 7: Propensity score specification search

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Contemporaneous (2008) regressors			Pre-treatment (2004) regressors			
Log GDP pc	0.6123***	0.6723**	0.6818**	0.6548**	0.6430**	0.5900**	1.0175**
	(0.2022)	(0.2871)	(0.3016)	(0.2884)	(0.2914)	(0.2803)	(0.4641)
Log Population	0.5549***	0.1462	0.1377	0.1245	0.0917	-0.1128	-0.1588
	(0.1586)	(0.1557)	(0.1608)	(0.1605)	(0.1634)	(0.1643)	(0.1920)
Polity dummy	1.0113*	1.1643*					
	(0.5214)	(0.6892)					
Polity Score			0.1144**	0.1195**	0.1224**	0.1427**	0.3289***
			(0.0521)	(0.0537)	(0.0548)	(0.0598)	(0.1020)
Mean Trans. Terror					0.0719	1.2011***	1.1618**
					(0.1210)	(0.4651)	(0.4729)
Mean Trans. Terror squared						-0.1966***	-0.1835***
						(0.0746)	(0.0696)
Polity Score squared							-0.0342*
							(0.0184)
Constant	-16.1641***	-11.2096***	-11.0540***	-10.5214***	-9.9736***	-6.7706*	-8.9657**
	(3.3408)	(3.9538)	(3.9322)	(3.7700)	(3.8262)	(3.4929)	(3.8564)
Log-Likelihood	-64.0823	-46.8111	-46.1815	-45.7023	-45.5986	-43.12443	-41.6248
Observations	141	141	141	141	141	141	141

Table 8: Covariate overlap

141 Countries sample used for REP-QML estimation												
	Treatment		All controls					Matched controls				
Group size	19		122					19				
	Mean	SD	Mean	SD	Overlap	SMD	LRSD	Mean	SD	Overlap	SMD	LRSD
Log GDP pc	9.58	1.10	8.41	1.30	0.95	0.80	0.98	9.60	0.99	0.84	-0.02	0.11
Log Population	16.43	1.48	16.06	1.59	0.95	0.24	-0.07	16.06	1.29	0.84	0.27	0.13
Polity score	8.11	3.16	3.26	6.52	1.00	0.94	-0.72	7.98	3.57	1.00	0.05	-0.12
PSM Trans. Terror.	1.21	1.44	0.79	1.68	1.00	0.27	-0.15	0.87	1.10	0.84	0.26	0.27
Propensity score	0.32	0.19	0.11	0.13	0.79	1.29	0.39	0.30	0.17	0.84	0.11	0.16
Log odds ratio	-0.95	1.14	-3.40	2.34	0.79	1.33	-0.72	-1.03	1.04	0.84	0.08	0.09
77 Countries sample used for PSM-GMM estimation												
	Treatment		All controls					Matched controls				
Group size	13		64					13				
	Mean	SD	Mean	SD	Overlap	SMD	LRSD	Mean	SD	Overlap	SMD	LRSD
Log GDP pc	9.75	0.93	8.49	1.40	1.00	1.06	-0.41	9.39	1.21	0.77	0.33	-0.26
Log Population	16.98	1.27	16.67	1.63	1.00	0.21	-0.25	16.53	1.27	0.92	0.35	-0.00
Polity score	7.85	3.69	2.64	6.65	1.00	0.97	-0.59	8.08	2.22	0.85	-0.08	0.51
PSM Trans. Terror.	1.77	1.43	1.50	2.08	1.00	0.15	-0.38	1.49	1.20	0.77	0.21	0.17
Propensity score	0.38	0.20	0.13	0.16	0.69	1.41	0.20	0.35	0.16	0.77	0.16	0.17
Log odds ratio	-0.56	0.90	-3.29	2.59	0.69	1.41	-1.06	-0.68	0.78	0.77	0.15	0.14

Table 9: Model estimates with matched sample

	(1)	(2)
	REP-QML	PSM-GMM
Log GDP pc		6.9138***
		(1.8940)
Log Population		2.8024***
		(0.9805)
Log GDP pc × Log Population		-0.3643***
		(0.1070)
Treatment	-0.5932*	86.9512*
	(0.3045)	(48.1636)
Treatment × Log GDP pc		-9.3684*
		(4.9607)
Treatment × Log Population		-4.7815*
		(2.6573)
Treatment × Log GDP pc × Log Population		0.5116*
		(0.2731)
Pre-Sample Mean Transnational Terrorist Events		1.2513***
		(0.3112)
Constant	-3.8225***	-59.6367***
	(0.3504)	(17.5769)
Number of observations	266	182
Number of countries (treated+untreated)	38=19+19	26=13+13

Standard errors in parentheses, *** p-value<0.01, ** p-value<0.05, * p-value<0.1

All regressions include year dummies.

Table 10: Proportional Treatment Effects for the Treated (Matched Sample)

Year	2008	2009	2010	2011
Random Effects Poisson (REP-QML)				
PATET		-0.4474		
CI lower bound		-0.4673		
CI upper bound		-0.4272		
Pre-Sample Mean GMM				
PATET	-0.6272	-0.6084	-0.6240	-0.6126
CI lower bound	-0.6479	-0.6285	-0.6438	-0.6341
CI upper bound	-0.6060	-0.5877	-0.6036	-0.5905

Table 11: Expected TTIRs after matching per 100 million people. Geometric average estimates.

Year	2008	2009	2010	2011
Pre-Sample Mean GMM - Static				
Expected TTIR under treatment	0.3329	0.7691	1.1172	0.8248
Expected TTIR under no treatment	0.8931	1.9639	2.9709	2.1294
Change in expected TTIR	-0.5602	-1.1947	-1.8537	-1.3045

Table 12: Proportional Treatment Effects for the Untreated (Matched Sample)

Year	2008	2009	2010	2011
Pre-Sample Mean GMM				
PATET	-0.3744	-0.2819	-0.3507	-0.2969
CI lower bound	-0.4154	-0.3409	-0.3981	-0.3473
CI upper bound	-0.3319	-0.2205	-0.3015	-0.2447