Measuring The Border Affect:
How Schengen Impedes Heroin Confiscation

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In partial fulfillment of the requirements for the degree of
Economic Policy in Global Markets

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Budapest, Hungary
2015

Abstract

Panel data for heroin seizures reported in the *World Drug Report* are analyzed with the aim of answering the question: How does joining Schengen affect a country's ability to confiscate heroin? Both fixed and random effects models are estimated to measure the *Schengen effect* on the confiscation of heroin, by controlling for cross-country differences, and long run trends in seizures. The results show that Schengen countries seize less heroin then they otherwise would as a non-Schengen country.

Acknowledgements

I would like to express my gratitude towards my supervisor, Andrzej Baniak for the valuable feedback he provided during the writing of this thesis. I would also like to thank my first economics professor, Amjad Kahn, and Mike's mom.

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Introduction

Basic economic theory tells us that scarcity, or lack of supply has a positive effect on price. The same is true in the case of heroin markets. That is to say, a person is less likely to become a heroin user if that said drug were scarcer and thus more expensive. Many European governments have formulated a drug policy of prohibition in part, based on this principle. Prohibition allows a government to use seizures as an instrument to influence, and use price as a deterrent to consumption. Others have analyzed the relationship between seizures of illicit drugs, and their price (Caulkins, and Reuter 1998; Farrell, Mansur, and Tullis, 1996; Konrad 1994; Storti & De Grauwe, 2008). I however, aim to measure a policy of higher priority, which might hinder current supply side measures reducing heroin consumption. More specifically, this paper poses the question: How does joining Schengen affect a country's ability to confiscate heroin?

I hypothesize that the measured *Schengen effect* on seizures will be substantial, and negative. This conjecture is made based on the border mechanism, or *border effect,* eliminated by Schengen. A significant proportion of heroin in non-Schengen Europe is seized at a country border, as the trafficker is confronted with the task of bypassing customs agents and law enforcement. Schengen eliminates the existence of border checks, thus decreasing the amount of heroin seized. There is however, one challenge when confronting this question. That is, when a country joins Schengen is there a change in the amount of heroin being trafficked through that country?

It seems likely that when a country joins Schengen, trafficking behavior in that country might also change. For instance, if Romania were to join Schengen, a Bulgarian heroin trafficker would seem more likely to traffic heroin via Romania to Western Europe than through Serbia. While such a substitution effect is plausible, it may not always be the case. In a scenario where

several countries in the same region join Schengen simultaneously, there may not be a chance for a direct substitution effect. For instance, Slovenia and Hungary are both known to transit heroin that is trafficked through Croatia wide (*World Drug Report* 2010 (henceforth *WDR* 2010), p. 55). Normally, one might assume that because Slovenia joined Schengen, the amount of heroin moving through Croatia and onward through Slovenia might increase. However, since Hungary also joined at the same time we cannot say if there is any change. It is for these reasons why this paper makes the key assumption, that there is no change in heroin being trafficked though a country after it joins Schengen.

Background

Consumption of opiates has remained stable between 12.9 million and 21 million users world-wide (World Drug Report 2012 (henceforth WDR 2012), p. 26). In 2011, global potential opium production recovered after a significant decline in 2010, which was primarily attributed to a reduced crop yield caused by a disease of opium poppy plants in Afghanistan, the world's principal opium producer (WDR 2012. p. 26). Despite this supply shock to the global opiate market, consumption trends in Europe have remained stable, particularly in those countries with substantial opiate use (WDR 2012. p. 30). Such a trend sheds evidence on the elasticity of demand for opiates in Western Europe, the world's largest heroin market, which sources its heroin from opium grown primarily in Afghanistan (UNODC, 2010b, p. 41).

In their most recent report, the UNODC reported that in 2013 illicit opium poppy cultivation reached new highs, since 1998 when estimates became available, bringing an even greater challenge for law enforcement further down the supply chain (WDR 2014. p. 21).

The main route for opiates into Europe is trafficked along the main Balkan route, which

leads from Afghanistan to Western and Central Europe via South-Eastern Europe. Prior to shortages from Afghanistan in 2010, heroin and opium seizures continued to rise along this route (WDR 2012, p. 31).

Literature review

The literature available on heroin seems to generally share one pervasive opinion: As it stands currently drug prohibition and policy do work to some extent, but are largely inefficient. In general there is a consensus that there is a threshold for what defines efficient drug policy, and that this, as of now arbitrary threshold, is being crossed.

The paper entitled *What Price Data Can Tell Us About Drug Markets* by Jonathan P. Caulkins and Peter Reuter use well founded economic theory to approach the complicated market that is: illicit drugs. They establish a number of indicators for what they believe affect prices most and subsequently delve into measuring the extent to which each indicator plays a role in the price of illicit narcotics. Caulkins and Reuter use cocaine price data provided by The White House's, Office of National Drug Control Policy (ONDCP). They calculate, using the estimated value of the amount of cocaine seized, that the value equated to a significant portion of the total market value of cocaine at the time. "…[S]eizures of all forms by all levels of government account for about 8-11% of the retail price of cocaine" (Caulkins, and Reuter 1998, p. 6). They calculated that the cost of "Compensation for the risks of incarceration" accounted for roughly a quarter of all of the costs associated with drug markets (Caulkins, and Reuter 1998, p. 7). The paper briefly addresses the shortcomings of drug policies but describes them as essentially effective in their goals of increasing price.

In the article, *Drug Policy and Federalism* by Kai A. Konrad discusses how due to the imperfect nature of illicit drug markets, prices vary depending on geography. Due to these varying prices, he proposes that addicts will want to go where their drug of choice is least expensive. He states that there is over-enforcement in federal systems and that this enforcement should become more concentrated and discrete based on where it is needed most. As a policy prescription he concludes "…[P]ossible migration restrictions impose a burden on those (including addicts) who want to migrate for reasons other than avoiding law enforcement" (Konrad 1994, p. 11). These restrictions would, potentially, force addicts to buy more expensive drugs, hopefully ebbing demand in these regions.

In the article *Price Setting Behavior in the Heroin Market* Thomas Pietschmann argues that risk is the main limiting factor for drug trafficking (Pietschmann 2006, p. 106). The higher the risk, the more costly it can be to traffic drugs. And consequently, the added risk will cause the price of heroin to rise. He addresses the point that this is equivalent to a higher wage for drug traffickers, and that, because of this more potential drug traffickers could flood these markets demanding jobs; but that in the long term, enforcement of prohibition of heroin should maintain a relatively stable level of perceived risk, keeping average prices high.

In the essay *Cocaine and Heroin in Europe 1983-93: A Cross-national Comparison of Trafficking and Prices* written by, Graham Farrell, Kashfia Mansur and Melissa Tullis; data regarding seizures of heroin and its effect on prices of heroin are analyzed over the decade stemming from 1983 to 1993. The study found that as seizures of heroin rose over this time period, the price of heroin declined (Farrell, Mansur, and Tullis, 1996, p. 1). In their analysis of Germany they found that over the period of 1983-93 kilograms of heroin seized increased from 268 in 1980 to 1,095 in 1993; and over the same period the price of a kilogram of heroin in

Germany (in USD) decreased from 58,000 dollars in 1983 to 24,645 dollars in 1993 (Farrell, Mansur, and Tullis 1996, Table 3 and Table 7).

This trend occurred across the board in their analysis of Europe and brought them to the conclusion that "The balance of evidence suggests increasing enforcement will impact only marginally upon prices due to rapidly decreasing marginal returns" (Farrell, Mansur, and Tullis, p. 25). To put this more simply, this means that more enforcement is not always better, which has been the prevailing mantra of drug policy.

In Farrell, Mansur and Tullis's essay they state that diminishing marginal returns to drug enforcement provide evidence that, at this point, there exists a certain amount of over-enforcement. They argue that seizing heroin and prosecuting traffickers has little effect on heroin prices due to the ease attributed to replacing low-skilled traffickers and the low costs associated with replacing the heroin seized (ie. the suppliers can just grow more)… (Farrell, Mansur, and Tullis 1996, p. 27).

One point that they leave out, but that is brought up by Caulkins and Reuter is that "…[I]t is possible for overall price to decline even as enforcement stringency increases if declines in the other cost-components more than offset the increase in enforcement's component" (Caulkins, Reuter 1998, p. 14). As an example, import costs could decline as traffickers become more highly skilled. Thus, all of drug prohibition enforcements efforts can be outweighed by changes on the supply side of heroin markets. As Pietschmann states, risk is the largest limiting factor to heroin trafficking and these measure should increase risk. The challenge, according to the literature, is to strike a balance in this policy that allows it to achieve its goals most effectively and efficiently by establishing what the threshold regarding efficient drug policies should equate to.

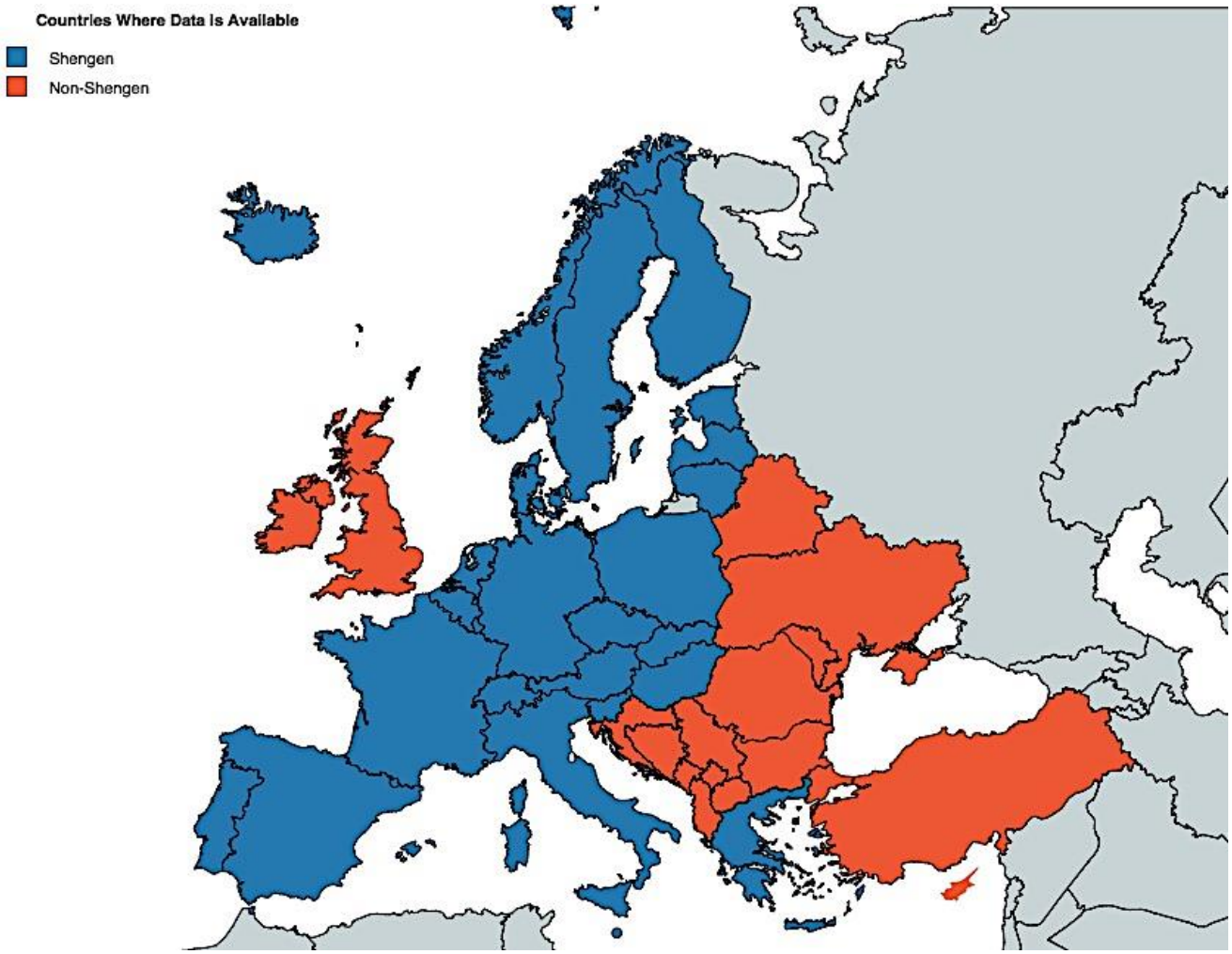
While generally there is an academic consensus that current practices of enforcing heroin prohibition are inefficient. And while some of these studies do not provide policy recommendations so much as findings, many of those studies that do provide recommendations, often conclude that there should be liberalization of current prohibition policies. These conclusions may be based on well-founded scientific methods, and principles, major liberalization of drug policy does not seem to be a political feasibility for many policy makers, particularly with a drug like heroin. With this in mind, this study aims to increase the efficiency in which law enforcement handles current policy, rather than advocating for broad legalization.

Data Exploration

Panel data for heroin seizures reported in the *World Drug Report* are collected from 43 European countries over 23 years (1990 – 2012). The sample consists of countries that joined Schengen sometime during the time frame observed, and countries that have never been part of Schengen. These data are collected by the United Nation's Office on Drugs and Crime (UNODC) through Annual Questionnaire Reports (ARQs) where data on wholesale and retail prices, annual seizures, and the purity of each particular drug is collected.

Despite its source, these data come with several weaknesses. First off, the dataset is sparse for some European countries. This is especially true in regard to some small Balkan countries. Second, data from ARQs are unlikely to come from the same respondents or from the same part of each country every year (Chandra, Barkell, and Steffen, 2011, p. 2). And third, in instances in which ARQ data are not available the UNODC uses data from other sources, including Europol, and already existing government records (Chandra, Barkell, and Steffen, 2011, p. 2).

Figure 4.1: Countries Considered



One additional issue is that not all heroin is created equally. That is to say that due to the illicit nature of operating in heroin markets, price, quality, and the amount of heroin varies across countries. In their paper Caulkins and Reuter (1998, p.8) inferred that "The most striking observation about illicit drug prices is how widely they vary across market levels, between

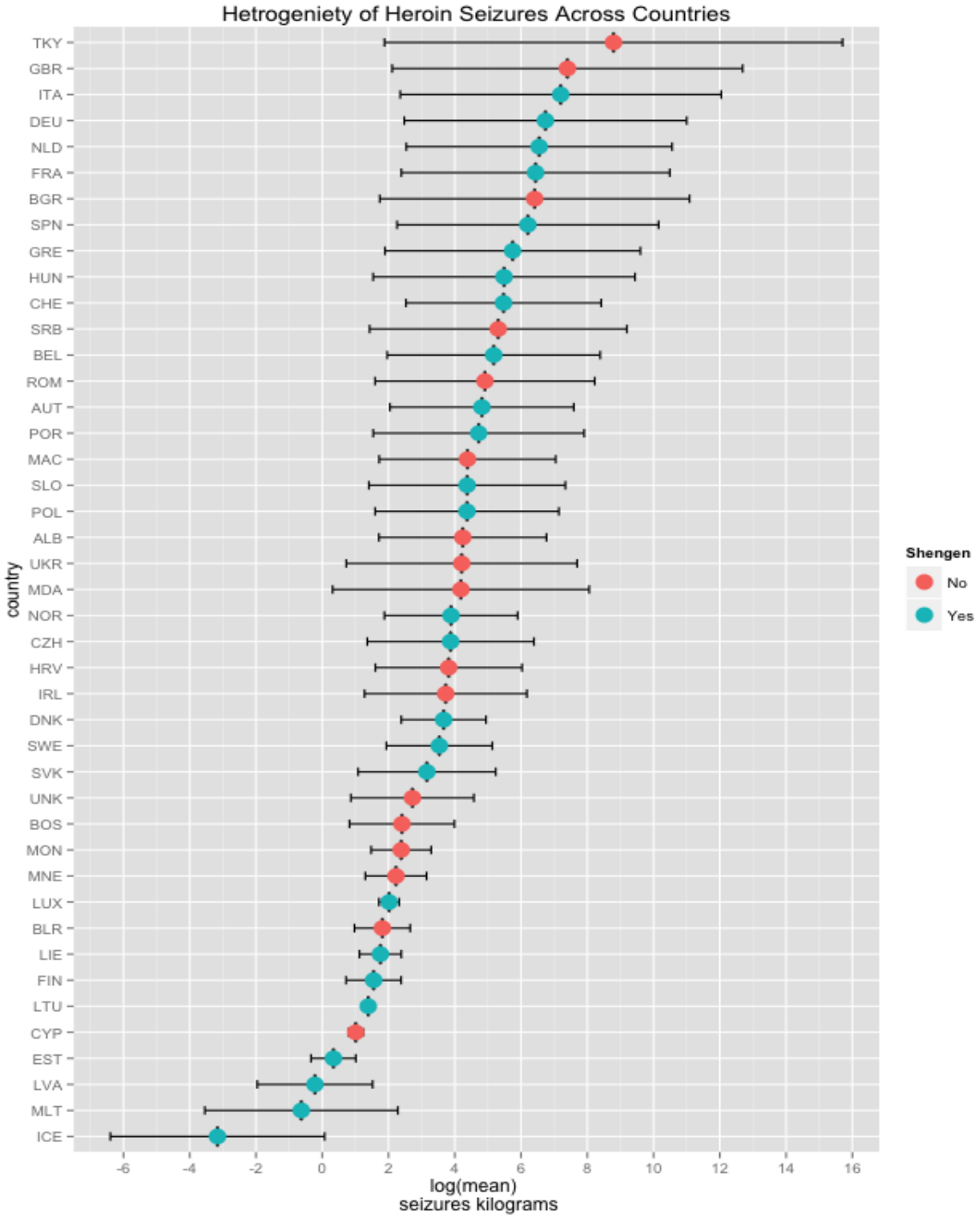
locations, over time and from transaction to transaction". This again, is due to rigid market structures of illicit drug markets.

When heroin travels form its source to consumption it moves through a distributional hierarchy, which is characterized by those market levels. As heroin makes its way across those market levels transaction sizes, decrease and per unit prices increase (Caulkins and Reuter 1998,p.8). One might also expect both quality and potency of the product to become less homogenous across different retail markets through the same mechanism. In other studies it has been show that these too are determinates in retail drug prices.

Figure 4.2 shows the country level distribution of all observed countries, and whether or not they are currently Schengen members. Each point is the mean amount of seizures observed for each country, while bar gives us an idea of the within country seizure variation by displaying one standard deviation from each country's mean. Turning to the x-axis we can see a mall group of countries whose value for average log(seizures) is less than zero, which means these countries seize less than 1 kilogram of heroin per year on average. On the other end of the axis there is a slightly larger group of countries whose value for average log(seizures) is greater than 6. This means these countries tend to seize more than 400 kilograms of heroin per year.

Naturally, those countries like Turkey, The United Kingdom, Italy, and Germany have more raw variation as compared to many other countries, but this is because they seize much more heroin on average. Moreover, there seems to be a clear relationship between the average amount of heroin seized and the standard deviation estimate. That is, until we reach the bottom of the graph. Explanation of the relationship between average heroin seized and within country variation seems to not only dissipate, but reverse around the point where the average annual country level log (seizure) is around zero, or 1 kilogram per year.

Figure 4.2



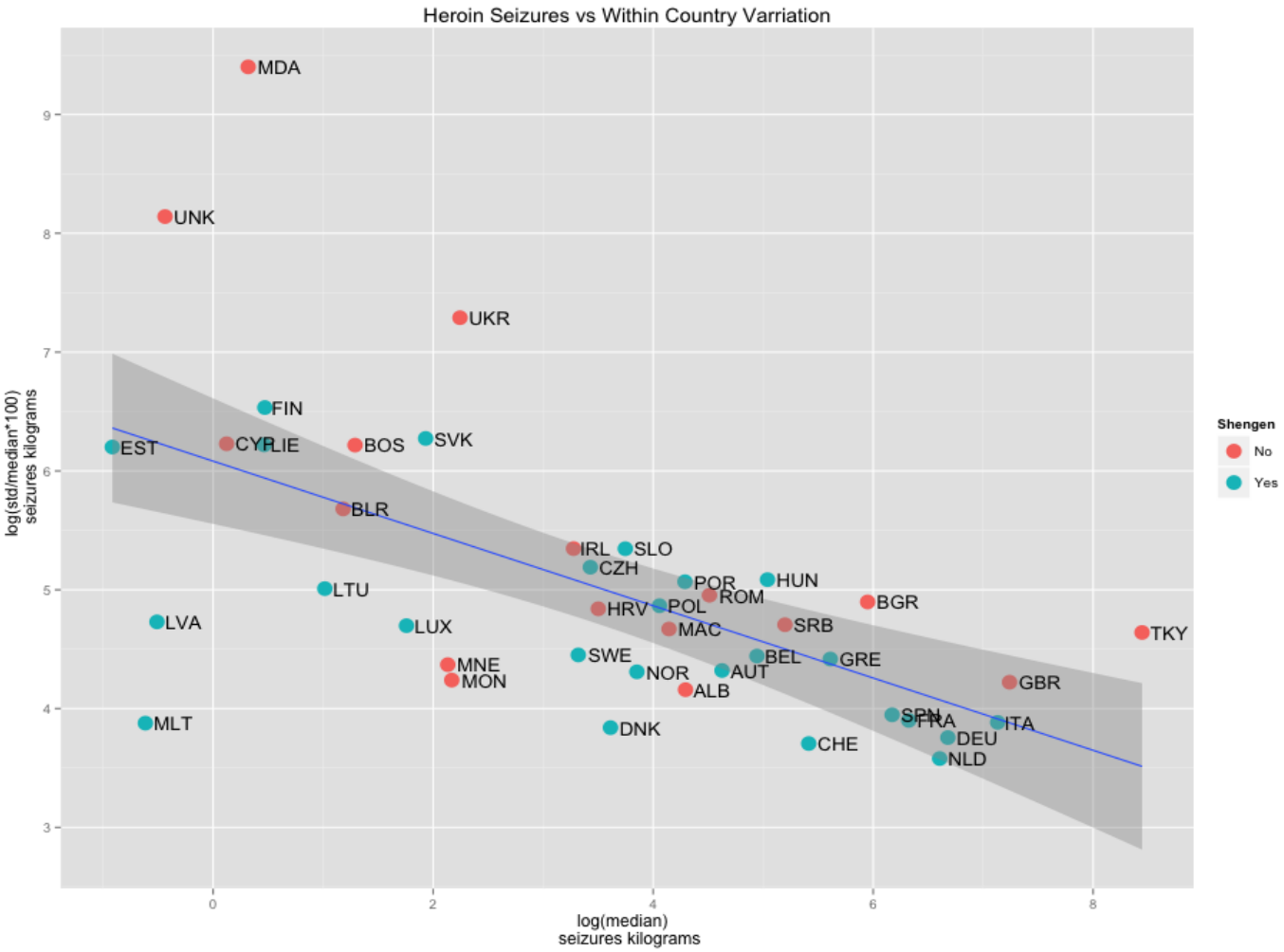
The reason why we can observe this is several-fold. First, is that these countries tend to be small, and there is just not much volume coming through. Second, is missing values. In the

23-year span of observation Turkey, The United Kingdom, Italy, and Germany have data for all 23 years, as compared to Iceland with a mere 5 years of data. Such infrequency of data makes for an increased likelihood of high within-country variance.

Those countries typically seizing less than one kilogram of heroin per year high are prone to high variance should they meet with merely one large bust in the given time frame. Meaning that one outlying value can greatly skew the data's depiction. Which brings us to the third reason why these countries might experience less consistency in heroin confiscation, the country level distribution of amount of heroin seized per seizure. This is just another way to say that those countries confiscating less than a kilogram per year are not confiscating from larger wholesalers, as they are at the end of the distributional hierarchy. And in the case that a country makes a bigger bust, the average amount of heroin seized becomes less consistent. For these reasons, regardless of the model estimated. For these reasons it is clear that Iceland should not be included in this analysis as it has an unacceptably high level of variance, missing values, not to mention what can only be characterized as irrelevance in the global heroin trade.

While it seems clear that counties Iceland should not be included in the analysis it is important to dig a little deeper. Analyzing these data by a slightly different measure of variation gives a bit of perspective. In figure 4.3 you can see the same x-axis as in figure 1.1, log-average of annual seizures by country, however this is plotted against a measure of within country variation of seizures. More specifically, it is the log of each country's standard deviation normalized by it's median, or $log(\sigma/\tilde{x} * 100)$, or the log value of each country's relative standard deviation.

Figure 4.3

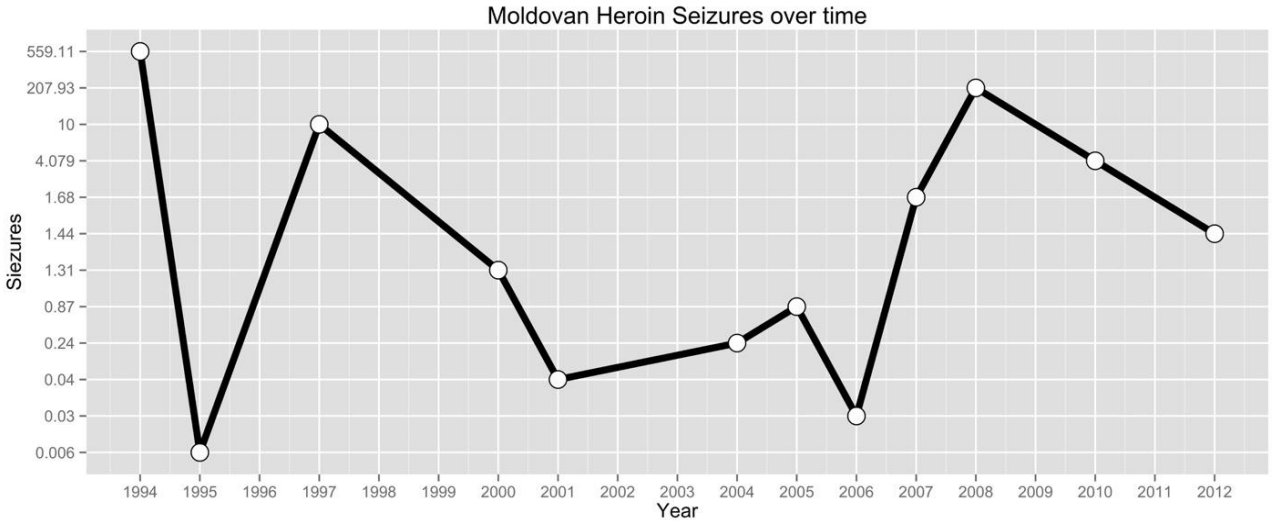


Here we can notice several important things. First, there is more dispersion around y for lower values on the x-axis. Thus giving more evidence to the notion that, small countries, or in particular countries who seize less heroin, are not as consistent at seizing heroin than those countries that tend to seize more heroin.

Second, it is important to notice the high level of measured seizure consistency across time for Moldova. Figure 4.4 is a time series of annual Moldovan heroin seizures from the years 1990 – 2012. Due to the high amount of variation seizure values are taken as factors in the graph.

This is clearly visible in the scale of the y-axis. What is more, eleven of the twenty-three possible observations are missing. Accounting for the data that is available we can see very large seizures for the years of 1994 and 2008, 559 and 208 kilograms respectively, while all other observations are ten kilograms or below. Because Moldavian heroin seizures are so volatile, and even notwithstanding the two exceptionally high years Moldova has reported having only seized a median of 1.31 kilograms of heroin over 23 years. For these reasons, it seems clear that Moldova should not be considered in this analysis.

Figure 4.4



Aside from those very small, and volatile heroin seizing countries, there are some other country subsets that should be considered. A good way to think about which country sets to consider is to distinguish the difference between the *border effect* and the *Schengen effect.* The Schengen effect is simply the expected difference in average hoin confiscation a country would experience after becoming a Schengen member. The border effect however, is the same expected difference, but only given that they existed in a state where there were checkpoints at every border, to no border checkpoints. While it is clear that these effects are similar, not every country that experiences a Schengen effect experiences a full border effect. Even if this paper is only

ambitious enough to measure the average Schengen effect, it is important to at least distinguish those country groups that tend to experience a qualitatively different experience.

The clearest of the country groups deserving consideration are those 'buffer countries'. In this case 'buffer countries' refers to those that share a border with non-member countries post Schengen succession. For instance, Hungary has been a Schengen member since 2007, but it still is required to check all persons crossing the external borders both inbound and outbound to Croatia, Serbia, Romania, and Ukraine. This means that those members bordering non-Schengen countries might expect a smaller decrease in post Schengen accession heroin seizures.

This means that in comparing two countries that joined Schengen concurrently, lets take Belgium and Germany for example. One might expect the true Schengen effect to differ, as Germany borders with Switzerland, Austria, Czech Republic, and Poland; all of which were non-Schengen countries at the time, while Belgium on the other hand borders only with countries that ascended to Schengen in the concurrent year. This example should also go on to illustrate the difference between the border and Schengen effects, because while a country might join Schengen, if it shares a land border with only non-Schengen countries, there should be close to no change in the expected border effect.

The second groups to consider are island countries. When an island country joins Schengen the process of entering said country does not change in the same way as with a non-island country. In plain terms, one cannot drive to Cyprus, Iceland, Ireland, or Malta like they can Germany, France, or Hungary. Due to this key difference, the island countries of Malta is dropped in addition to Moldova, and island country Iceland. Despite being island countries Cyprus and Ireland are not dropped from the sample as they have never been part of Schengen, and can still help control for trends in heroin seizures across time.

Model

Both fixed and random effects models are estimated using a panel over 23 years (1990 - 2012) for two samples; first our reduced sample of forty and full sample of forty-three European countries. The models aim to explore the relationship between predictor and outcome variables within each country that has joined Schengen between the years of (1990 – 2012). In our case the outcome or dependent variable is the amount of heroin seized in kilograms. The distribution country level seizures is skewed strongly to the right. For this reason the log value of annual heroin seizures is taken (see Appendix figure 9.1 & 9.2). The independent variable is an Schengen dummy for each year a country is a member. Time dummies are also used to control for long run seizure trends in Europe. In both fixed and random effects models robust standard errors are used, in the fixed effects estimations standard errors are clustered.

A fixed/random effects models are appropriate here as we are interested in analyzing the impact of Schengen, which varies across individuals or in this case, countries over time. The first model estimated is a fixed effects model, which can be written as:

$$Y_{it} = \alpha_i + \beta X_{it} + u_{it}$$

Where α_i is the unknown intercept for each country, Y_{it} is the dependent variable, X_{it} is the independent variable, β is the coefficient for that independent variable, and u_{it} is the error term. For each variable i refers to the individual or country, and t refers to time, or in our case the year. This model makes the basic assumption that the constant term for each country differs.

For many of the same reasons why these countries might have different constant terms, each country has its own individual characteristics that may or may not influence the predictor variables (for example, geography). When using fixed effects we assume that something within

the individual may impact or bias the predictor or outcome variables and we need to control for this – hence, the rationale behind the assumption of the correlation between country's error term and predictor variables. Fixed-effects estimation works to remove the effects of those time-invariant characteristics, so we can assess the net effect of the predictors on the outcome variable. In essence fixed effect models control for all time-invariant differences between the individuals, so the estimated coefficients of the fixed-effects models cannot be biased because of omitted time-invariant characteristics (Kohler, Ulrich, Frauke, Kreuter, p.245).

Another important assumption of the fixed-effects model is that those time-invariant characteristics are unique to the individual country and should not be correlated with other individual characteristics. Each country is different therefore the country's error term and constant, which captures individual characteristics, should not be correlated with the others. If they are correlated, then fixed effects estimation is not suitable and a random effects model should be considered instead.

The main assumption underlying random effects is that unlike the fixed effects model, the variation across individual countries is said to be random and uncorrelated with the predictor or independent variables included in the model: "…the crucial distinction between fixed and random effects is whether the unobserved individual effect embodies elements that are correlated with the repressors in the model, not whether these effects are stochastic or not" (Greene, 2008, p.183). Random effects essentially allows for time-invariant variables to play a role as explanatory variables. The random effects model used can be written as:

$$Y_{it} = \alpha_i + \beta X_{it} + u_{it}$$

Where α_i is comprised of $(\alpha + v_i)$ the unknown intercept for each country plus some independent random variable, Y_{it} is the dependent variable, X_{it} is the independent variable, β is the

coefficient for that independent variable, and u_{it} is the error term. As with fixed-effects each variable i refers to each country, and t refers to each year.

Random-effects does have one potential drawback: It is possible that regression error, u_{it} is correlated with explanatory variable. The basic idea is that there may be some unobserved quality with effect on both Schengen accession and seizing heroin. This makes it very important to specify those individual characteristics that may influence the predictor variables when estimating a random effects model. This unobserved quality could be geography, police spending, or other endogenous advantages.

The problem with this is that some variables may be difficult to measure, and must be omitted. This could cause regression error and the explanatory variable to be correlated. Hence why Iceland, and Malta – island countries, which joined Schengen, are excluded from the model all together. Again, while we could specify another predictor variable for island countries. However, we would merely be comparing the pre and post seizure average of two countries that together over the 23-year time of observation reported having seized 12.4416 kilograms of heroin 8 observations while in Schengen, and 20 prior to succession. Given these data, it seems too probable that specifying this characteristic in the modeling will give results that do no accurately characterize the true Schengen effect for island countries. Therefore, these countries are dropped.

Naturally when choosing which strategy is best for a given dataset once should consider if time-invariant characteristics are unique to individual countries and whether or not they correlated with other individual characteristics. Correlation between the error-term and specified independent variables should be considered, in addition to the possibility of omitted variables. In essence, if you have some reason to believe that differences across individuals have influence on

your dependent variable then use random effects. However, if your error term is correlated with the explanatory variables, random effects are not appropriate.

While such a though process is always vital when deliberating on panel data, a test developed by Jerry A. Hausman in 1978 can help determine whether fixed or random effects is the appropriate model to employ. The null hypothesis the Hausman test is that the fixed effects and random effects estimators do not differ substantially. The test statistic developed by Hausman has an asymptotic $\chi 2$ distribution. If the null hypothesis is rejected, or if the error terms are uncorrelated, the conclusion is that random effects is not appropriate and that we may be better off using a fixed effects model (Hausman, 1978). But again, it is important to go through the thought process characterized above, regardless of the results of the underlying test.

Sample

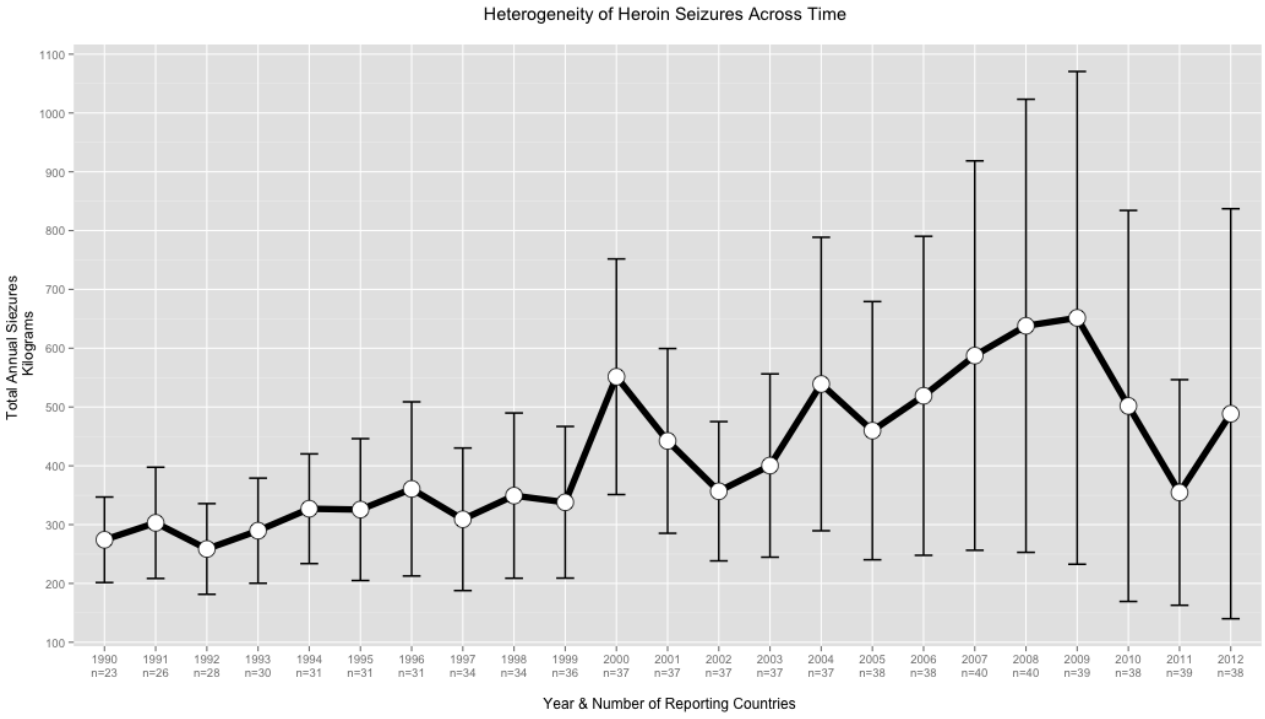
The sample consists of countries that joined Schengen sometime during the time frame observed, and European countries that have never been part of Schengen. Moldova is dropped because of infrequent, generally low, and inconsistent data. Similarly Iceland, and Malta are omitted for having some of the negative qualities of the previous group, but in addition are small island countries that have joined Schengen. Non-Schengen island countries, and countries which might have missing values, but are not in Schengen are still included in the sample.

It is not problematic to leave these countries in the sample as they have never been part of Schengen, and only help determine the average European trend in seizures (European Commission, 2015). The exclusion of Iceland and Malta however, may bias the results; as the Schengen affect of these countries are not measured. Again this is relevant because their border

crossing process does not change in the same way as with non-island countries when they join Schengen.

That is not to say that adjusting for time trends is not important. Figure 1.4 shows heterogeneity of heroin seizures in our sample across time, by visualizing the mean, standard deviation, and number of the reporting countries in each sample year. One important feature is that the number of reporting countries increases as time wears on. This means that if a country has missing values, they are more likely to occur in the first half of the time observations then the second half. This is pertinent because the average heroin confiscation may differ between those countries that are missing observations, and those who are missing none at all, thus skewing the graph. Despite this fact, there seems to be clear evidence time trends in heroin confiscation should be controlled for.

Figure 6.1



Notwithstanding these realties, it seems unlikely that the exclusion of these countries would change the results in a meaningful way. In consideration of the other two weaknesses mentioned, might give additional noise to the LHS variable. While the additional noise may reflect some measurement error on the LHS, making the model estimators less precise, it will not bias those estimators.

To measure whether a country was an Schengen country in a given year, the date of implementation is considered. For those countries who joined in March of any given year, that year was considered a Schengen year. As for those countries who joined late in year (October – December) the following year was considered their first in Schengen. While this does result in some measurement error in the independent varriable, this is a fairer treatment than what could be gained from utilizing other methods of estimation. While such error may bias the resulting coefficients, it seems unlikely that it will change the interpretation of those estimates in a meaningful way, especially since the long run average change is considered.

Figure 6.2

Dates of Schengen Implementation In Sample							
Mar 1995	Oct 1997	Dec 1997	Mar 2000	Mar 2001	Dec 2007	Dec 2008	Dec 2011
Belgium France Netherlands Portugal Spain Luxembourg Germany	Italy	Austria	Greece	Denmark Finland Sweden Norway	Czech Rep. Estonia Hungary Poland Slovakia Slovenia	Switzerland	Liechtenstein

Results

Both the random and fixed effects models yield significant results at least the 5 percent level. So, by following the random effects model we can see that controlling for European trends in heroin seizures over time, and country specific differences, the long-term average seizure rate for a Schengen country is 47 percent lower that of a non-Schengen country. Following the fixed effects model, this difference is slightly larger at 56 percent. To put that in perspective, if Romania were to join the Schengen area one might expect its ability to seize heroin to decrease by around 56 percent.

Table 7.1 – Random & Fixed Effects: Sub-Sample – With Time Dummies

	Dependent variable: log seizures	
	(Random)	(Fixed)
Schengen	-0.469**	-0.564*
	(0.168)	(0.274)
Constant	2.56**	2.96**
	(0.415)	(0.271)
n	798	798

*p<0.05, **p<0.02

As a robustness check the same regression is ran including dropped countries Moldova, and island countries Iceland, and Malta. When including these countries similarly significant results can be observed. With fixed effects controlling for European trends in seizures over time, and country specific differences, the long-term average heroin seizures for an Schengen country is 52 percent lower that of a non-Schengen country. If we were to consider the random effects model we could say that the long-term average heroin seizures for a Schengen country is expected to be 46 percent lower than that of a non-Schengen country.

Table 7.2 – Random & Fixed Effects: Full Sample – With Time Dummies

	Dependent variable: log seizures	
	(Random)	(Fixed)
Schengen	-0.460**	-0.522*
	(0.170)	(0.261)
Constant	2.25**	2.66**
	(0.448)	(0.299)
n	838	838

*p<0.05, **p<0.02

Returning to the original hypothesis that the measured *Schengen effect* on heroin seizures will be substantial and negative is clearly true. Regardless of which of the four estimates is conceded closest to the true effect, the main result is the same, Schengen impedes heroin confiscation in Europe. Moreover, it does help our case that all of our coefficients are negative and significant, but which is the 'best estimate'? Throughout this study there has not been any clear indication to believe that differences across individuals have influence on the dependent variable, thus giving support for fixed effects. What is more, there seems to be clear, mostly geographical factors like being an island or buffer country that might cause the error term to be correlated with the independent variable.

Table 7.3 – Hausman Tests

	Null hypothesis: GLS estimates are consistent	
	(full-sample)	(sub-sample)
Test statistic: $\chi 2(23)$	48.27	49.29
P-value	0.00154	0.00114

Turning to the output form the Hausman test in figure 7.3 we can see that in both samples the random effects appear to be correlated with the independent variable, thus according to both the test results, and econometric theory fixed effects estimates should be more accurate.

Of the two estimates it is probably better to go with our reduced sample with three few countries, for the reasons explained extensively in previous chapters. So from these results we can see that controlling for European trends in heroin seizures over time, and country specific differences, the long-term average seizure rate for a Schengen country is 56 percent lower that of a non-Schengen country. Keeping in mind the assumption that there is no change in the amount of heroin trafficked through a country when the join Schengen, when there is reason to believe that there is either an increase or no significant change when a country joins Schengen bolsters the results even more. Reason being that if there is an increase in heroin flowing through a country after joining Schengen then the coefficients estimated in this paper are actually biased toward zero, underestimating the true effect.

It is also important to keep in mind that this is merely an average effect. If we subtract and add the standard error from the coefficient we can see an interval between 29 and 83 percent. Much of the variation is likely to be caused by the omitted buffer country factor, whereby the Schengen effect captures only part of a given countries' potential border effect. In simple terms, despite joining Schengen many countries are still legally obliged to do border checks with their non-Schengen neighbors. Therefore, if for example Bulgaria were to join Schengen we might expect their ability to seize heroin to change less than lets say Belgium, when it joined in 1995.

Discussion

Even when we assume that there is no change in heroin trafficked through a country after it joins Schengen, we observe a clear *'border effect'*. When the model was estimated using different samples: large countries, and small countries: large markets, and small markets: transit countries, and destination countries, the results were practically the same. Results at this level of robustness leaves little doubt about their interpretation – The Schengen effect on heroin seizures is large and negative.

Policy makers are contracted by the people to make decisions, and decisions have tradeoffs. In this case the tradeoff is keep Schengen at the cost of weaker instruments to combat heroin consumption, or reinstate the old system at the cost of the many benefits of Schengen. It is clear that the previous is a greater policy priority. Given that Schengen is here to say, perhaps member countries should reconsider their supply side policies, reallocating efforts toward demand side policies.

Appendix

Figure 9.1

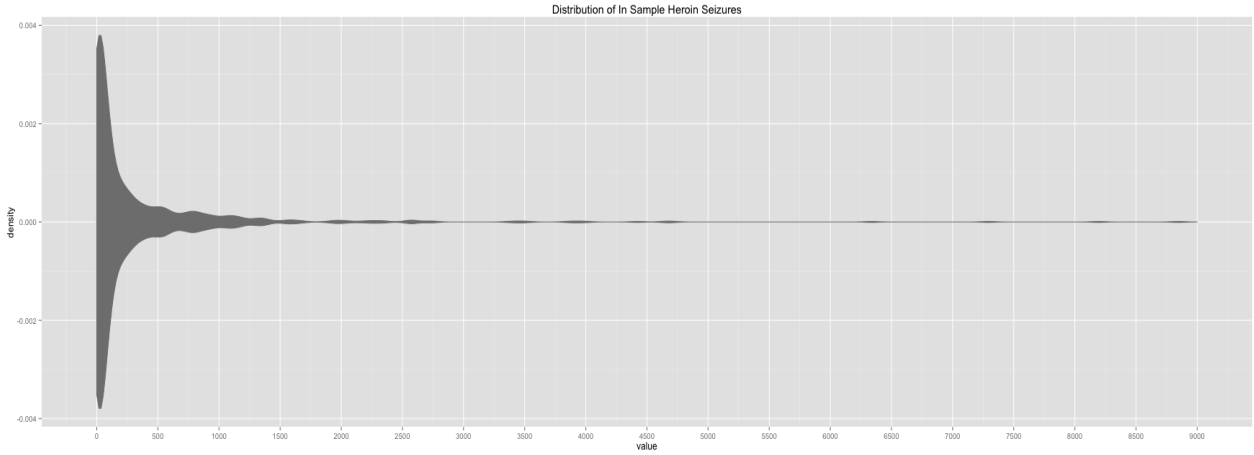
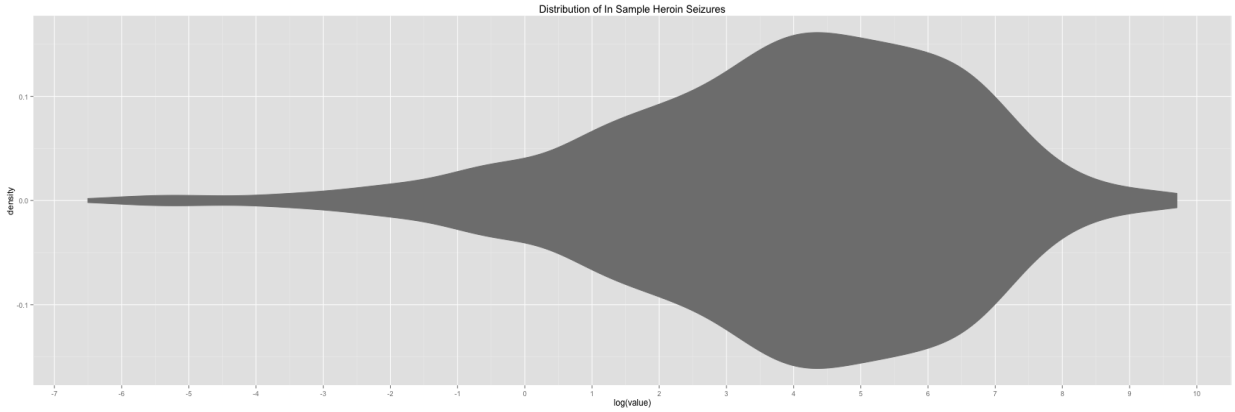


Figure 9.2



```
Model 1: Fixed-effects, using 838 observations
Included 43 cross-sectional units
Time-series length: minimum 5, maximum 23
Dependent variable: l_value
Robust (HAC) standard errors

             coefficient   std. error   t-ratio   p-value
  ---------------------------------------------------------
  const        2.66628      0.299525     8.902    3.88e-18 ***
  shengen     -0.522682     0.261821    -1.996    0.0462   **
  dt_2         0.361558     0.200987     1.799    0.0724   *
  dt_3         0.364360     0.275284     1.324    0.1860
  dt_4         0.620409     0.320665     1.935    0.0534   *
  dt_5         1.04411      0.385047     2.712    0.0068   ***
  dt_6         0.539754     0.481626     1.121    0.2628
  dt_7         0.702743     0.509344     1.380    0.1681
  dt_8         0.580489     0.435567     1.333    0.1830
  dt_9         0.918635     0.426863     2.152    0.0317   **
  dt_10        0.989255     0.344506     2.872    0.0042   ***
  dt_11        1.39944      0.406345     3.444    0.0006   ***
  dt_12        1.13205      0.445229     2.543    0.0112   **
  dt_13        1.16558      0.456388     2.554    0.0108   **
  dt_14        1.19145      0.397451     2.998    0.0028   ***
  dt_15        1.33561      0.342695     3.897    0.0001   ***
  dt_16        1.37734      0.441362     3.121    0.0019   ***
  dt_17        1.40364      0.400607     3.504    0.0005   ***
  dt_18        1.43563      0.447209     3.210    0.0014   ***
  dt_19        1.66615      0.474377     3.512    0.0005   ***
  dt_20        1.47576      0.517296     2.853    0.0044   ***
  dt_21        0.875719     0.535458     1.635    0.1024
  dt_22        0.826600     0.465108     1.777    0.0759   *
  dt_23        1.13327      0.442786     2.559    0.0107   **

Mean dependent var   3.536684   S.D. dependent var   2.895889
Sum squared resid    1356.717   S.E. of regression   1.325672
LSDV R-squared       0.806714   Within R-squared     0.073236
LSDV F(65, 772)      49.57055   P-value(F)           8.2e-232
Log-likelihood      -1390.947   Akaike criterion     2913.893
Schwarz criterion    3226.141   Hannan-Quinn         3033.581
rho                  0.268852   Durbin-Watson        1.332156

Joint test on named regressors -
  Test statistic: F(23, 772) = 2.65242
  with p-value = P(F(23, 772) > 2.65242) = 4.50105e-05

Test for differing group intercepts -
  Null hypothesis: The groups have a common intercept
  Test statistic: F(42, 772) = 74.0758
  with p-value = P(F(42, 772) > 74.0758) = 7.1112e-240

Wald test for joint significance of time dummies
  Asymptotic test statistic: Chi-square(22) = 89.0469
  with p-value = 4.97649e-10
```

```
Model 2: Random-effects (GLS), using 838 observations
Included 43 cross-sectional units
Time-series length: minimum 5, maximum 23
Dependent variable: l_value

             coefficient   std. error   t-ratio   p-value
  ---------------------------------------------------------
  const        2.24683      0.448283     5.012    6.61e-07 ***
  shengen     -0.460425     0.170352    -2.703    0.0070   ***
  dt_2         0.354784     0.378108     0.9383   0.3484
  dt_3         0.358152     0.372247     0.9621   0.3363
  dt_4         0.609321     0.367151     1.660    0.0974   *
  dt_5         1.01078      0.360913     2.801    0.0052   ***
  dt_6         0.509888     0.366010     1.393    0.1640
  dt_7         0.673558     0.368580     1.827    0.0680   *
  dt_8         0.544101     0.360499     1.509    0.1316
  dt_9         0.880958     0.362885     2.428    0.0154   **
  dt_10        0.935249     0.357613     2.615    0.0091   ***
  dt_11        1.35623      0.357847     3.790    0.0002   ***
  dt_12        1.07974      0.360331     2.997    0.0028   ***
  dt_13        1.11697      0.362044     3.085    0.0021   ***
  dt_14        1.12786      0.360725     3.127    0.0018   ***
  dt_15        1.28414      0.360169     3.565    0.0004   ***
  dt_16        1.32440      0.358677     3.692    0.0002   ***
  dt_17        1.33588      0.358051     3.731    0.0002   ***
  dt_18        1.37853      0.356445     3.867    0.0001   ***
  dt_19        1.59423      0.367753     4.335    1.64e-05 ***
  dt_20        1.40275      0.373073     3.760    0.0002   ***
  dt_21        0.803076     0.372289     2.157    0.0313   **
  dt_22        0.756316     0.372779     2.029    0.0428   **
  dt_23        1.04706      0.372083     2.814    0.0050   ***

Mean dependent var   3.536684   S.D. dependent var   2.895889
Sum squared resid    7402.971   S.E. of regression   3.013868
Log-likelihood      -2101.912   Akaike criterion     4251.823
Schwarz criterion    4365.368   Hannan-Quinn         4295.346

'Within' variance = 1.75741
'Between' variance = 5.22162
corr(y,yhat)^2 = 0.00214269

Wald test for joint significance of time dummies
  Asymptotic test statistic: Chi-square(22) = 55.3405
  with p-value = 0.000106231

Breusch-Pagan test -
  Null hypothesis: Variance of the unit-specific error = 0
  Asymptotic test statistic: Chi-square(1) = 4171.56
  with p-value = 0

Hausman test -
  Null hypothesis: GLS estimates are consistent
  Asymptotic test statistic: Chi-square(23) = 48.2662
  with p-value = 0.00154673
```

```
Model 3: Fixed-effects, using 798 observations
Included 40 cross-sectional units
Time-series length: minimum 6, maximum 23
Dependent variable: l_value
Robust (HAC) standard errors

             coefficient   std. error   t-ratio   p-value
  ---------------------------------------------------------
  const        2.83566      0.307148     9.232    2.78e-19 ***
  shengen     -0.564254     0.274800    -2.053    0.0404   **
  dt_2         0.383442     0.207558     1.847    0.0651   *
  dt_3         0.386688     0.285213     1.356    0.1756
  dt_4         0.762936     0.302555     2.522    0.0119   **
  dt_5         0.944882     0.352991     2.677    0.0076   ***
  dt_6         0.792945     0.468628     1.692    0.0911   *
  dt_7         0.756421     0.529461     1.429    0.1535
  dt_8         0.561003     0.449995     1.247    0.2129
  dt_9         0.989610     0.442211     2.238    0.0255   **
  dt_10        1.10627      0.354063     3.124    0.0019   ***
  dt_11        1.47456      0.421007     3.502    0.0005   ***
  dt_12        1.30384      0.453198     2.877    0.0041   ***
  dt_13        1.27247      0.469525     2.710    0.0069   ***
  dt_14        1.28995      0.411160     3.137    0.0018   ***
  dt_15        1.46332      0.350631     4.173    3.36e-05 ***
  dt_16        1.48392      0.458712     3.235    0.0013   ***
  dt_17        1.58646      0.403294     3.934    9.16e-05 ***
  dt_18        1.53618      0.464316     3.308    0.0010   ***
  dt_19        1.64744      0.479347     3.437    0.0006   ***
  dt_20        1.56958      0.533210     2.944    0.0033   ***
  dt_21        0.955573     0.553079     1.728    0.0845   *
  dt_22        0.881036     0.484954     1.817    0.0697   *
  dt_23        1.12098      0.456436     2.456    0.0143   **

Mean dependent var   3.766697   S.D. dependent var   2.714025
Sum squared resid    1193.715   S.E. of regression   1.274403
LSDV R-squared       0.796664   Within R-squared     0.085430
LSDV F(62, 735)      46.44681   P-value(F)           7.7e-213
Log-likelihood      -1292.997   Akaike criterion     2711.994
Schwarz criterion    3006.967   Hannan-Quinn         2825.323
rho                  0.281709   Durbin-Watson        1.317276

Joint test on named regressors -
  Test statistic: F(23, 735) = 2.98504
  with p-value = P(F(23, 735) > 2.98504) = 4.07651e-06

Test for differing group intercepts -
  Null hypothesis: The groups have a common intercept
  Test statistic: F(39, 735) = 70.9127
  with p-value = P(F(39, 735) > 70.9127) = 1.67773e-220

Wald test for joint significance of time dummies
  Asymptotic test statistic: Chi-square(22) = 92.4494
  with p-value = 1.30805e-10
```

```
Model 4: Random-effects (GLS), using 798 observations
Included 40 cross-sectional units
Time-series length: minimum 6, maximum 23
Dependent variable: l_value
```

	coefficient	std. error	t-ratio	p-value	
const	2.56309	0.414930	6.177	1.05e-09	***
shengen	-0.469660	0.167531	-2.803	0.0052	***
dt_2	0.373260	0.371874	1.004	0.3158	
dt_3	0.376784	0.365921	1.030	0.3035	
dt_4	0.746049	0.360771	2.068	0.0390	**
dt_5	0.924080	0.358478	2.578	0.0101	**
dt_6	0.752552	0.361821	2.080	0.0379	**
dt_7	0.711759	0.362385	1.964	0.0499	**
dt_8	0.510402	0.355999	1.434	0.1521	
dt_9	0.933058	0.356680	2.616	0.0091	***
dt_10	1.04557	0.353220	2.960	0.0032	***
dt_11	1.41386	0.353220	4.003	6.86e-05	***
dt_12	1.22852	0.355882	3.452	0.0006	***
dt_13	1.19883	0.356035	3.367	0.0008	***
dt_14	1.21583	0.355723	3.418	0.0007	***
dt_15	1.38920	0.355723	3.905	0.0001	***
dt_16	1.40761	0.354160	3.975	7.71e-05	***
dt_17	1.50820	0.354458	4.255	2.35e-05	***
dt_18	1.45328	0.351805	4.131	4.01e-05	***
dt_19	1.54326	0.362430	4.258	2.32e-05	***
dt_20	1.46135	0.366056	3.992	7.17e-05	***
dt_21	0.850282	0.367123	2.316	0.0208	**
dt_22	0.776338	0.365756	2.123	0.0341	**
dt_23	1.01486	0.368062	2.757	0.0060	***

Mean dependent var	3.766697	S.D. dependent var	2.714025
Sum squared resid	6145.706	S.E. of regression	2.816015
Log-likelihood	-1946.832	Akaike criterion	3941.663
Schwarz criterion	4054.034	Hannan-Quinn	3984.836

```
'Within' variance = 1.6241
'Between' variance = 3.81232
corr(y,yhat)^2 = 0.001562

Wald test for joint significance of time dummies
  Asymptotic test statistic: Chi-square(22) = 60.9198
  with p-value = 1.63198e-05

Breusch-Pagan test -
  Null hypothesis: Variance of the unit-specific error = 0
  Asymptotic test statistic: Chi-square(1) = 4121.78
  with p-value = 0

Hausman test -
  Null hypothesis: GLS estimates are consistent
  Asymptotic test statistic: Chi-square(23) = 49.2941
  with p-value = 0.00113922
```

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