Quasi-Experimental Analysis on the Effects of Adoption of a Value Added Tax

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Abstract:

Value added taxes have become an important source of government funding in past decades, but little empirical work has been done on their macroeconomic impacts. Because the decision to implement a VAT is endogenous, regression methods analyzing the impact of the policy choice will yield biased estimates. To solve this problem, I first model the VAT adoption decision for 192 countries using survival analysis. I then match adopters to non-adopters using propensity score matching. I find that VAT adoption is associated with an increase in growth and investment as well as and lower inflation and government spending as a share of GDP. (JEL-H2)

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I. INTRODUCTION

The Value Added Tax (VAT) has displaced many different trade, sales, and manufacturing taxes in the past half century, becoming a major source of government revenue for an increasing number of countries. The tax has all the desirable non-distortionary theoretical properties of a sales tax, its incidence ultimately falling on consumers, but it is much harder to avoid, as the tax is collected at all intermediate steps during the production of a finished retail good rather than at the final sale. As a result, it has proven incredibly popular as it can collect a great deal of revenue (Keen and Lockwood 2010; Toder and Rosenberg 2010). While this tax is theoretically less distorting than other taxes for the each dollar of revenue collected, there is limited analysis of its overall impact on the economy. Demonstrating that the tax also has few negative side effects would make it invaluable for developing countries looking for an effective tax instrument. Showing that a VAT is associated with increases in growth, trade, or investment would be solid evidence in favor of its adoption. Yet empirical studies of the VAT are rare. This paper seeks to fill a hole in the literature, analyzing the overall economic impact of a VAT, which will allow policy makers to make informed decisions of the costs and benefits of this tax.

Understanding the effects of the VAT is vital, as the importance of the VAT is unlikely to decline in the future. The IMF increasingly suggests countries raise more revenue through adopting a VAT or raise VAT rates (Bird 2010). Many countries, facing rising pressures on pension programs due to changing demographics, are funding social programs from general revenues rather than payroll taxes, which usually means shifting the overall tax composition of government revenues toward VATs (Bird and Smart 2012). A VAT is a requirement for EU membership, and other international organizations are considering their own mandatory VATs, with a unified set of rules and repayment systems at an international rather than national level,

such as the Gulf Cooperation Countries (Cnossen 1998; Kapur 2012). As more countries adopt VATs, international payment mechanisms for collecting VATs will become more efficient (Ebrill et al. 2001) and VATs will become more popular in the future.

As a country chooses when it will enact a VAT, simply estimating the effect of adoption on macroeconomic variables, such as trade or growth, through normal regression techniques, will result in biased estimates, as countries that choose to adopt a VAT may be fundamentally different from those who do not. In order to solve this selection problem, I use survival analysis to generate probabilities of VAT adoption to be used as propensity score for 192 countries from 1967-2012. I then employ matching methods to obtain the causal effects of VAT adoption among 112 countries, 90 of which do eventually adopt, from 1986-2007 in the year of adoption and for five years after.[1] This study finds that VAT adoption is associated with increases in investment, and less robustly increases in growth as well as decreases in inflation and government spending. There is no evidence of changes in trade once controlling for selection.[2]

This paper is organized as follows. Section II covers background on previous studies on the VAT and other tax policies. Section III describes the adoption equation using survival analysis, both its determinants and results. Section IV introduces the matching process. Section V describes the results of the matching as well as some robustness checks. Section VI concludes.

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[1] Depending on the variable considered, fewer than 90 treated countries may be compared due to data constraints, but never fewer than 76. See online appendices A-G for more details.

[2] Note that every countries' adoption event will be different, with VATs replacing some taxes in some or being a completely new tax in others. Regardless of the specifics, a VAT will represent a move to shift overall government revenue collection more towards a specific type of consumption tax. This does not imply that it becomes the only, or even dominant form, of taxation revenue. Hence, this paper only can estimate mean effects of adopting a VAT.

II. BACKGROUND

Starting with France in 1954, and reaching its first developing country in 1960 when it was enacted by the Cote D'Ivoire, 166 countries have at some point implemented a VAT as of January 2013, and once passed, nearly all countries retain it. Figure 1 shows aggregate VAT adoption over time and Figure 2 shows the adoptions in each year. After slow initial adoption, a number of countries, primarily in Europe and South America, adopt in the mid 1970s. Countries then adopt slowly until the late 1980s, after which adoption rises and then remains high until the early 2000s, with a very large spike following the fall of the Soviet Union. By the mid 2000s adoptions taper off to only a few a year. The general shift towards a consumption type tax that necessarily accompanies the passing of a VAT should have observable effects if different types of taxes have different levels of efficiency or distort economic activities differentially.

Many papers have demonstrated, empirically or theoretically, that consumption taxes like the VAT are superior to income taxes with regard to distortions. Acosta and Yoo (2012) and Arnold (2011) show that countries grew faster when more of their revenue came from consumption taxes. These papers employed panel regression methods, so selection effects may confound these findings as countries legislate their own tax codes. Krusell et al (1996) create a theoretical model that demonstrates consumption taxes are less distortionary than income taxes, but are less able to achieve redistributive ends. Milesi-Ferretti et al. (1998) finds similar results, with consumption taxes distorting the choice between leisure and consumption less than income taxes for each dollar of revenue raised. It would appear there is some evidence that consumption taxes cause less distortion than income taxes for each dollar raised, but these findings are not specific to VATs and the econometric methods employed may not estimate true causal effects of a given tax policy.

However, there has been less work done to analyze the distortions from VAT adoptions. Keen and Lockwood (2010) study government revenue collected as a result of VATs. Using regression analysis of a system of equations, they found that the higher government revenue was as a share of GDP, the less likely countries were to adopt VATs. VATs generally increased government revenue as a share of GDP, with Sub-Saharan African countries seeing the least benefit. While many authors expect VATs to increase revenues collected, and possibly government spending, due to the relative ease of tax collection, its effects on government consumption and revenues are not clear (Keen 2007; Toder and Rosenberg 2010). Effects on other important macroeconomic variables such as investment, growth, trade and inflation are practically unstudied empirically. While empirical work is scarce, there is a great deal of theoretical work done on VATs that can guide this study.

III. SURVIVAL ANALYSIS

III.A Modeling VAT adoption

Understanding the causes and processes of VAT adoption is necessary to address endogeneity due to selection into a VAT. Previously, other authors have modeled the decision to have a VAT as a probit (Keen and Lockwood 2010) or a Markov switching model based on a Cox hazard regression (Cizek et al 2012). Despite their popularity in determining the probability of adopting a policy (Dehejia and Wahba 2002 among others), a panel probit model may not be the best fit for the policy being described in this paper due to VAT's persistence as when a

country adopts a VAT, it almost never removes this policy, with only 5 countries ever repealing out of 166 countries adopting (Grandcolas 2005).[3]

A Cox proportional hazard model is employed here instead to model the decision of adopting a VAT. This approach has a number of strengths when applied in this case. First, it fits the decision making accurately: countries tend to adopt a VAT once and never repeal it, so one could model survival time in the pre-VAT state. Second, the hazard model can be used to produce propensity scores for matching. It will predict linear hazards at every period, and two countries with the same predicted linear hazard would be at the same risk of adoption in that period. Thus, among two countries with the same propensity scores, if one adopts and the other does not, one can consider this assignment to the treatment group as random conditional on observable variables. Third, this selection equation also allows one to estimate the average effects of VAT adoption immediately following the event as well as several years before and after, as adoption is a single event rather than a repeated decision.

Cox proportional hazard models assume all observation units are at risk of failure, in this case adoption of a VAT, at a rate determined by some baseline hazard as well as a vector of time variant and invariant covariates. Only variation from years in which a failure event takes place is used, and once a country fails it contributes no further information to the estimation of the hazard model. More detailed explanation of a Cox proportional hazard model can be found in Cox (1972), with particular attention to the interpretation of covariates and results in Fisher and Lin (1999) and its use in experimental design in Lu (2005).

[3] Vietnam in the 1970s, Grenada in 1986, Ghana in 1995, Malta in 1998, and Belize in 1999 all attempted to pass a VAT and later repealed it. Vietnam, Grenada, and Ghana would later re-adopt a VAT in 1999, 2010, and 1998 respectively, and these re-introduction events that are not repealed are included in this paper. Belize and Malta are included with their original adoption dates. This is in line with what was done by Keen and Lockwood (2010). Omitting these observations does not qualitatively change results.

III.B Determinants of VAT Adoption

The theoretical literature on VATs and taxation comparisons provides a rich pool of covariates that could affect a countries' VAT adoption decision. Among these, international pressure plays possibly the most important role. Keen and Lockwood (2010) propose that countries under IMF lending programs are more likely to adopt a VAT to pay off their debts, both out of necessity for more revenue in general and due to the encouragement they receive from the IMF to adopt a VAT in particular (Bird 2010). This can be seen in some recent IMF reports on the fiscal conditions of many nations; if a country does not yet have a VAT, the IMF often suggests it should adopt one.[4] I thus include a dummy equal to 1 if a country is under an IMF lending program, either GRA or PRG facility, for any number of months in a given year. Keen and Lockwood (2010) use a similar dummy variable approach. Summary statistics on Table 1 show that roughly half of the country-years from 1965-2010 are under some sort of IMF lending program.[5]

Another feature of VAT adoption is its general diffusion through proximity. Keen and Lockwood (2010) find evidence that they tend to spread in regional booms and Cizek et al(2012) find significant evidence that adoption may be influenced by a number of spatial factors. Experience with a neighboring country's adoption may better inform a country on the costs of adopting a VAT, or fears of losing manufacturing to a neighbor with a more efficient tax code, could increase the likelihood of a country adopting it. I represent this proximity by creating an index variable that is the equal to share of bordering countries with a VAT, taking a value

[4] See IMF Country Report No. 12/54 on Palau, IMF Country Report No. 08/186 on the Marshall islands, IMF Country Report No. 10/191 on Comoros, and the Kingdom of the Netherlands-Aruba: Concluding Statement of the 2010 Article IV Consultation Mission, among others, as well as various newspaper editorials.

[5] IMF lending data is not available before 1984, so this table is only looking at lending data from this point onward.

between 0 and 1. Thus, if a given country borders 4 countries and 2 of them have a VAT, the value of this index is .5 for that country for that year. Table 1 shows that on average 15% of a countries neighbor's have a VAT, but this measure is heavily skewed right.

Other elements of national economic composition, specifically agriculture and natural resource production, are also likely major determinants of choosing a VAT. Edminston and Fox (2006) found that farmer cooperatives were disproportionately affected by a VAT, and Cnossen (1998) claims that due to the less formal nature of agriculture, enforcing a complex taxation system is likely to lead to compliance problems. Agricultural production is included as a share of GDP, and it is expected to have a negative effect on the propensity to adopt a VAT. According to Table 1, on average 23.45% of a countries' GDP comes from agriculture, but the standard deviation is quite high at 17.51%, and so this variable provides a fair amount of variation. Additionally, a country with rich natural resources is less likely to seek tax revenue from other sources (Keen and Lockwood 2010). Keen and Lockwood (2010) use a static measure of subsoil natural resource wealth in 2000 to capture this effect, while instead here I choose to use a series on natural resource rents as a share of GDP from the World Development Indicators. This should accomplish the same objective as the subsoil resource stock in showing the importance of natural resources to a country, but also allow values to change over time, as it is a share of GDP in that year rather than a static stock of natural resources. Resource rents as a share of GDP average 10%, but with a standard deviation of 15.93% the distribution should be quite skewed right.

It is possible geopolitical factors affect the choice to have a VAT. Since authorities need to be able to observe imports in particular, and economic activities in general, in order to tax them, it is possible that large, landlocked countries will have a much harder time collecting a VAT compared to small, island countries due to the number of possible entry locations (Keen

2010). As a result, I have included log of physical size in square kilometers, and whether the country is an island (whether it has no neighboring countries with land borders) or landlocked as potential determinants of VAT adoption. Approximately 20% of country-year pairs are islands and 20% are landlocked according to Table 1. On the other hand, as the definition of a small island country is subject to interpretation, I have also included a dummy variable for membership in a political organization, The Alliance of Small Island States, allowing countries to self-select as small islands. While this organization was only founded in 1990, I treat membership as time-invariant, as countries would likely have considered themselves small islands states prior to joining this organization. I also include regional dummies for North America, South America, Europe, Africa, and Asia, with Pacific being the omitted variable.

In addition to time invariant geographic factors, time invariant historical factors may also influence a nation's tax design. Keen (2010) suggests that the British legal system and French legal systems may both, for various reasons, encourage adoption of sales taxes generally and the VAT in particular. Some elements of the VAT in former French colonies bear some resemblance to French tax law, including advanced collection schemes where some sectors are taxed higher than other. It is also possible that the link is not directly from France, but rather just from shared colonial history, with countries with similar colonial pasts cooperating on fiscal policies.[6] British common law countries are more likely to set up independent tax authorities or large tax offices that would make VAT collection more effective with respect to revenue collected, which could make them more willing to set up a VAT in the first place (Keen 2010). To control for this, I include dummy variables for membership or observer status in both the British Commonwealth

[6] This reasoning may suggest other colonial dummies may be possible determinants of policy adoption, but no other country colonized nearly as many countries as France or Britain, so adding other colonial dummies will not capture much more variation.

and the Francophonie.[7] I treat membership as time invariant, so if a country was ever a member of either organization it is treated as a member at all periods for this adoption equation. While membership in these organizations may change over time, often for political reasons, the reason for joining is ultimately a historical one, and thus being a member at any point would indicate some historical influence from the British or French legal systems.[8] According to Table 1, approximately 30% of country-years are from Francophonie countries and roughly the same amount are from commonwealth countries. Aside from historical origin, the structure of the government may matter as well, specifically whether the country is governed as a federation or not. Federations are less likely to adopt VATs, due to a decentralized tax collecting apparatus (Keen and Lockwood 2010; Ebrill et al 2001; Treisman 2002). However, only 8% of countries are federations.

While not strictly time invariant, membership in the Warsaw pact or being a Former Soviet Republic may also affect VAT adoption. There are a large number of VAT adoptions among such countries in the 1990s after the fall of the Soviet Union. It is thus possible such countries would have adopted a VAT much earlier if they had been allowed to do so by the Soviet Union. Thus I include a time invariant dummy for membership in the Warsaw pact or status as a former Soviet republic.[9] These combined make up only 14% of total country years.

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[7] Using legal origins gives similar results, as they are highly collinear with membership. However, as legal origin is not a declared membership like membership in the Commonwealth or Francophonie and subject to interpretation.

[8] While membership in the British Commonwealth usually relies on colonial history, the Francophonie is much less restrictive in its membership, allowing countries to join due to more general historical reasons. Several countries in Eastern Europe are members of the Francophonie, for example, despite never being colonized by France.

[9] Former Yugoslavian republics are treated as Warsaw pact countries despite not being members of the Warsaw pact due to sharing similar political cold war history and experiencing turmoil in the 1990's. Due to data demands, these observations often drop out due to missing information for covariates.

Several other general demographic variables, employed by Keen and Lockwood (2010) in their VAT selection equation, are also included here, including percentage of population that are children below 14 and percentage of population that is above 65 (dependency ratios), log of population level, population growth, log of GDP, and log of GDP per capita. The overall size of the economy and the average wealth of its citizens may affect the decision to adopt a VAT; a very poor country or very small country may not find it worthwhile to construct a large tax collection apparatus that one needs with a VAT. Similarly, the age structure of a country may affect the tax system, as the need for revenue and the ability to collect it changes with demographics (Bird and Smart 2012).

Additionally, the macroeconomic variables likely to be affected by a VAT, investment, growth, trade, inflation, and government consumption, should also be considered as determinants of adoption. Policy makers arguably will take into account the current state of variables most likely to be affected by a VAT; those seeing a low rate of investment or growth may be more likely to adopt a VAT to replace other, more distortionary taxes in hopes of raising investment or growth. Investment, in the form of gross capital formation as a percent of GDP, and growth, as per capita GDP growth, are both included as determinants of the adoption equation.

Trade in particular is likely to be a major determinant of VAT adoption as well as significantly influenced by VATs. As exports are zero rated for taxation purposes and imports usually full taxed under a VAT (Ebrill et al 2001) countries must consider both the revenue and general economic changes that could arise from such a tax change given their trade patterns (Keen 2008). Hines and Summers (2009) also find that more open countries are more likely to rely on consumption type taxes, such as the VAT. Finally, Swank (2002) shows evidence that increased trade and international competition will affect government taxation behavior. Trade

here is represented by exports plus imports as a share of GDP, commonly referred to as openness in the literature.

As VATs have rebate mechanisms in place, they should not lead to increased prices in the same way that other taxes on manufacturing and production do (Ebrill et al 2001). Thus, if a country believes it faces higher than desired inflation, changing its tax system to prevent price cascading is one way that it could affect the price level in the short run. Government spending too is a likely determinant of adopting more taxes, though the sign is of indeterminate direction (Keen and Lockwood 2010). It is possible that individuals in countries with high government consumption as a share of GDP would have high preferences for government services and thus be more inclined to adopt VATs to fund more spending. Alternatively, they may also consider themselves to be at an ideal level of expenditure, and have no desire to raise tax burdens to increase spending further.

Finally, it is important to note the time dimension of adoption. Adoption is defined as the national rollout of the tax system. There is some lag between the decision to pursue a VAT and the collection of the taxes, both from implementing it politically and creating a tax collection infrastructure. There may also be pilot programs in certain regions before national rollout of the tax, so the effects of a VAT may appear before the tax is national. Thus, I lag all independent variables by two years to account for this, in line with the 18 month span from VAT being introduced in the legislature to collections starting (Ebrill et al 2001). Therefore, if a VAT is adopted in 1988, the determination to do so will be based on values of right hand side variables from 1986.

III.C Results of the VAT Adoption Equation

Table 2 presents the coefficients (linear hazards) of the Cox hazard regression for selecting a VAT. Five specifications are considered here. The first specification (1) contains all covariates discussed above, and as a result has the fewest number of observations, spanning from 1986 to 2012 with 112 countries, 90 of them adopting a VAT during the observed period. The second specification (2) is the same as the first, but omits government consumption and inflation. The third (3) specification also omits trade, investment, and per capita GDP growth, as these variables are outcomes we will test to see the effects of VAT's adoption, and conditioning the selection on one of the outcome variables may weaken observed effects when one later performs matching.[10] The fourth specification (4) omits many time-varying covariates. This is done in order to study a much longer window where such data is missing and as result observe many more countries, spanning from 1967 to 2012 with 178 total countries and 150 adoptions. The final specification (5) omits all time-varying covariates and looks at only time invariant factors of adoption, but gains few other observations over specification (4), with 192 countries and 158 adoptions. All countries are considered at risk of adopting a VAT starting in 1960, the year the first developing country adopts a VAT.[11]

Specifications (1)-(3) show no statistically significant differences in marginal effects of covariates on survival. IMF lending has the predicted positive sign, so being under an IMF program is associated with a greater risk of implementing a VAT. Being a member of the Francophonie also sees countries tend to adopt VATs more quickly. Alternatively, having a high

[10] I would like to thank an anonymous referee for this suggestion.
[11] There is an adoption event in every year from 1984-2012, so specifications 1-3 do not drop any years due to lack of adoption events as would happen with Cox hazard regressions. Specifications 4 and 5 will have some years drop as some years before 1984 have no adoption events.

share of natural resource rents as a share of GDP is correlated with slower VAT adoption, and countries that are governed as federations also tend to adopt VATs at slower rates. These effects are as predicted in the previous theoretical literature or match the prior empirical findings. No other included variables have a statistically significant effect on adoption rates, aside from continental-level controls.

Specification (4) cannot exactly be compared to (1)-(3) as it contains different covariates, covers a longer period, and has more countries. Being a member of the Francophonie or a Federation have the same effect here as they did in (1), but being a former Warsaw Pact country is associated with a lower likelihood of VAT adoption. This is sensible given the general failure to adopt VATs among these countries until the 1990s. Countries that are larger (as indicated by log population) or have older populations get VATs more rapidly. Specification (5) finds that, upon omitting all time varying variables, larger countries adopt VATs faster and smaller island countries adopt them slower, along the same effects of being a former Warsaw Pact, Francophonie member or Federal country as seen in specification (4).

IV. MATCHING

The fitted values from the Cox hazard regressions in section III.C can be used as propensity scores. Each country-year pair at risk of adopting a VAT will produce a fitted value *Xb*, the linear hazard of that observation, which describes its current failure rate. Using a simple difference algorithm like that used in Lu (2005), countries with similar hazard rates will have similar expected times of failure, and so any difference in failure time between two country-year pairs Xb_1 and Xb_2 where $|Xb_1 - Xb_2| \leq \varepsilon$ for very small positive ε can be treated as random. A variety of matching methods can be used to compare treatments, but single nearest neighbor

matching with replacement is the easiest to discuss and implement. In this method, a country [1] implementing a VAT is matched to a single other country [2] not implementing a VAT based on the choice of country-year pair that minimizes $|Xb_1\text{-}Xb_2|$. This method is used most often in this paper, but other matching methods, kernel matching and radius matching, are also employed to demonstrate robustness of results. The properties of matching estimators are covered in Rosenbaum and Rubin (1983,1985) as well as with a special focus on kernel estimation in Heckman et al (1998).

Figure 3 provides a visual example of the matching employed here over a general panel of N countries over T years. As time increases from 0 to T, many countries adopt VAT's and region (1) consists of the treatment group country-years for this matching study. After adopting, countries drop out of estimation for the survival equation, so the country-years in region (2) after adoption are not contributing to the survival equation and not used for matching. As this paper wants to look at effects over time for 5 years after adoption, it cannot use as a control a country 5 years or less before they adopt a VAT, region (3) on Figure 3. These country-year pairs still contribute information to the survival equation. Countries that do not have a VAT and will not adopt one for 5 years compose the control region (4) and will be used both as controls in the matching and contribute variation to the survival equation. Thus, country-years from the treatment region (1) are compared to similar country-years in the control region (4) to see the effects of adoption.

In order to verify this method has eliminated differences in observables between treatment and controls that would produce bias, one can examine the balance of observable covariates between the matches. One obviously cannot control for unobserved factors in matching. But if matched controls systematically differ from the treated observations on

observables, then it will not be clear if the observed difference in outcomes between treatment and control arises from the treatment itself or the underlying differences between the treatment and control groups. Lu (2005) discusses the importance of showing that there exists an equal distribution of all covariates as a result of this problem. Table 3 is the result of matching all treatment (adoption) country-years to a single nearest neighbor control, then comparing the difference in means for all covariates used in specification (1) of the selection equation from Table 2. Note that there will be more observations in this check of covariate balance than actually are matched to study given macroeconomic outcomes, as matching looks at a much longer time window on the outcomes of interest and thus is more sensitive to missing data.[12]

Of the 29 covariates[13] used to determine adoption, one variable shows a statistically significant difference in means between control and treatment at least at a 10% level. Warsaw Pact country-years are statistically significantly more common in the treatment group than in the matched controls. While one may expect a few variables to be significant out of 29 due to random chance, this single difference should not be dismissed out of hand. One could omit all former Warsaw Pact countries as a later robustness check to see if they are driving matching results.

The average propensity score of adopters and matched controls are not statistically different from one another, with a t statistic of .14. Thus the matched control group and adoption group have, on average, the same probability of adopting the policy accounting for the total effect of all observables. However, the average propensity score of adopters compared to all

[12] If one checks covariate matches of only those that will be matched for each of the macroeconomic outcomes, the results are qualitatively the same, but this approach requires many more tables, one for each macroeconomic variable (Growth, investment, etc). studied and if one considers matching through multiple methods the number of tables needed grows exponentially.
[13] The number of covariates is 31 if one includes former soviet republics, all of which drop out due to lack of data availability, and the pacific dummy, omitted for perfect collinearity.

unmatched observations is significantly higher at a 5% level, showing that the adopters, and the controls similar to them, are statistically significantly different from the overall population.[14] These two features are needed for the matching performed to be valid. Another difference of note, while not actually a dependent variable, is the average year difference between controls and treatment. The treatment years are on average 4 years later than control years. This is not a surprising result, as there are many more controls that can be drawn from earlier years due to the fact countries exit after adopting in a survival framework. This would be worrisome if earlier years were systematically different from later years; however, as we have already compared 29 variables and found only one of covariates that systematically differ at least at a 10% level (roughly what one would expect for 29 variables), it is unlikely that this difference in years would bias differences in means that is not be captured by these variables.[15]

V. RESULTS

The selection equation used to generate propensity scores for matching is specification (1) of Table 2, having the fewest observations but containing the most covariates and the highest pseudo R^2 of all specifications attempted.[16] The macroeconomic outcomes of interest here are investment, growth, trade, inflation and government consumption as shares of GDP.

While matching is performed on a single country-year propensity score, the effects on the outcomes of interest of the countries matched are compared several years before and after the event of adoption. This in many ways resembles the method employed by Abadie and

[14] Removing 7 very extreme propensity scores among the untreated as outliers raises the t statistic to approximately 9, while it is only 2.22 now.
[15] I cannot dismiss the possibility that years do matter for the outcomes, but it would have to work through a channel not captured by the current observables.
[16] Evaluating through SIC and AIC is indeterminate between which specification, 1 or 2, is superior, but specification 1 allows for analysis of the effects of more variables.

Gardeazabal (2003) in that it matches two countries (or more generally in their method, one country to a synthetic composite country that can be composed of one or more countries) then follows their respective paths over time, both forward and backward, to identify the impact of a policy change. The countries should be identical before the policy change and different after if the policy had a causal effect. In the case of this paper, the decision to adopt occurs at time t=-2 and adoption occurs at time t=0. I will consider looking back as far as time t=-7, 5 years before the decision to adopt occurs, and as far forward as time t=5, 5 years after adoption. Furthermore, I will also consider the change in outcomes of interest from 7 years before adoption to 2 years before adoption, when the decision to adopt a VAT is made, as well as the change from 2 years before adoption to 5 years after adoption, to capture the impact over time of VAT adoption.[17]

As one seeks to observe the average treatment on the treated (ATT), control and treatment countries should not differ in the years before a country decides to adopt a VAT. The control and treatment groups should have the same means of the variable of interest before the VAT is implemented, and the between-group difference in changes between 7 years before adoption to 2 years before should be zero. Thus, while both control and treatment group may see changes over time in the variable of interest, these changes should be the same. However, if the implementation of a VAT has an effect, then there should be a difference between changes in the treated group and the control group over time, 2 years before adoption to 5 years after, that is not zero. Thus while the variable of interest may change for both the control and treatment group, the changes should be different between treatment and control groups to assert a causal effect of the

[17] Because actors may expect a VAT, they may change their behavior before it is actually being collected, so looking at the effect from decision to adopt forward may provide a better picture of the overall change rather than starting 2 years later, when it is implemented. Pilot programs that implement a VAT in certain regions a few years before national level implementation may mean a country sees some smaller effect prior to nationwide adoption.

policy. Also, if the average difference in means between matched controls and treatment is nonzero in a given year post-adoption, this is further evidence of an effect.

Results from matching, compared treated to matched controls, are summarized by their t statistics on Table 4 for the five variables of interest over the thirteen year periods being tested, seven years before adoption to five years after adoption. Most importantly, the average differences in changes over time before adoption as well as after adoption are listed at the bottom of the table. Results comparing treated to unmatched controls, the whole data set without accounting for selection, are also presented for comparison purposes.

The overall differences in changes over time after adoption are statistically significantly different from zero for investment, being positive at a 10% level, and inflation and government spending, being negative at a 10% level. Growth is statistically significantly higher in the year of adoption, along with three and four years after adoption, and inflation is lower four and five years after adoption. These results would be consistent with VATs raising growth and investment as a share of GDP at the same time they lower inflation and government spending. Combining these generally positive effects with previous author's results that VAT can collect a great of revenue, a VAT seems an attractive policy.

Also important for the validity of the results, the overall difference in changes before adoption between adopters and controls is not statistically different from zero for all five variables. There is also no difference in any of the given variables in any of the years before adoption. If there were a differences, or differences in changes, of any variable, it would call into question the interpretation of the results, as it would imply a difference in trajectories between controls and treatment prior to undertaking the treatment. As there is no such difference in

trajectories before, and a difference in trajectories after, there is evidence that VAT adoption caused changes over time among these variables.

Discussing specific variables, it would thus appear over time after adoption, VATs caused a gain in investment as a share of GDP. Ferretti and Roubini (1998) forecast that investment should rise when taxes are collected more from consumption relative to income, which is borne out here. This finding would seem to disagree with Cai and Harrison (2011) who found that Chinese firms did not change investment behavior in response to temporary changes in the VAT rate. However, as Cai and Harrison studied repeal of VATs for firms, not imposition, and looked at a firm rather than a national level, a number of reasons that could explain the difference in findings.

There is no overall difference in changes in GDP growth either before or after the VAT is passed, but several years after adoption growth is statistically significantly higher in countries that adopt compared to similar countries who did not. This would seem to demonstrate against any large efficiency losses as forecast by Stiglitz and Shahe (2005) and Stiglitz (2008) due to the shifting of production from the observed formal sector to the unobserved, less efficient informal sector. If VAT is causing distortions that drive economic activity underground, which should show up in reduced GDP growth numbers, the distortions such as increased evasion efforts are either too small to detect or are offset by GDP gains. Positive changes in growth would agree with the proposed efficiency gains, as would be forecast by Zagler and Durnecker, (2003); Arnold, (2011); and Hines and Summers, (2009) from moving toward consumption taxes. However, it is unlikely that this higher growth would continue indefinitely.

There is no statistically significant effect of VAT adoption on trade as a share of GDP as seen in Table 4. Before adoption, trade as a share of GDP is the same among adopters and

matched non-adopters. Compared to all countries, the unmatched controls, countries that would adopt a VAT traded less. After adopting a VAT, while adopters do see trade increase as a share of GDP, so do the matched controls. Thus there does not appear to be a causal effect of VATs on trade. However, compared to unmatched controls, after adopting a VAT, countries no longer trade less than other countries.

These results are not inconsistent with the results of Hines and Desai (2005) who found countries with a VAT traded less. While it is true countries that adopt VATs trade less than countries that do not adopt VATs, when matched against countries with similar characteristics, they do not trade any more or less. Even after adoption, when countries with VATs see their trade numbers rise, they still trade (albeit statistically insignificantly) less than unmatched controls. Thus panel methods here could find a negative correlation of VAT adoption with trade. However, this may be because countries who adopt a VAT trade less to begin with, and they may adopt VATs in order to attempt to raise trade; VATs themselves do not lower trade.

Due to wide ranges of possible values for inflation, and its high variance, matching is particularly useful in determining the effects of VATs on inflation. While before adoption countries with VATs tended to have roughly the same inflation as matched controls, in the years following adoption countries with a VAT see a less inflation in several years compared to matched controls. Due to large yearly swings in average inflation, this result must be interpreted with caution.

The changes in government consumption as a share of GDP prove to be the most interesting. First, compared to unmatched countries, VAT adopters have significantly smaller government spending as a share of GDP, but not when compared to matched countries. There is no difference in spending in any given year before or after adoption, and no difference in

changes before adoption, but following the decision to adopt there is a statistically significant decrease over time in government consumption as a share of GDP among countries with a VAT compared to matched controls. As Keen and Lockwood (2010) found sometimes conflicting results of the effects of VATs on government revenue, this result is not unexpected. If collecting public funds and as a result government consumption has become less distortionary due to a more efficient tax system, one would expect government consumption to grow rather than due to the lower marginal cost of collecting public funds makes spending easier. However, if government changes other taxation or spending behaviors at the same time, such as lowering taxes collected by more distortionary means, that could explain this net fall in spending.

It is possible that the earlier results from VAT adoption on various variables arises from the choice of matching method, so six other methods are tested; single nearest neighbor without replacement, nearest three neighbors with replacement, caliper matching with a .025 and .05 radius, and kernel matching with an Epanechnikov as well as Normal kernel. Results are shown in Table 5, comparing differences in changes over time before adoption and after adoption. Results for investment as well as government spending and inflation are reasonably robust. On the other hand, growth is less robust, although one may expect single higher years of growth rather than long term growth. The impact of VAT adoption on trade is not statistically significantly different from zero. Finally, before adoption, there is no systematic difference in changes between control and treatment groups on any variable regardless of matching method, which one must see for the inferences above to be valid. Table 5 provides strong evidence these changes observed after adoptions are causal effects of the policy and not driven by choice of matching methods.

VI. CONCLUSION

VATs are believed to be able to raise a large amount of money with little overhead or distortion, and so have become more common around the world in recent decades. However, studies actually analyzing the impacts of VATs are rare and their results are often not conclusive. Resolving this lack of conclusive results is important, as signs are that VATs and consumption taxes are likely to grow in importance for government funding in future years (Hines and Summers 2009; Bird 2012). This policy deserves more analysis before rushing headlong into more VAT reliance.

As VATs are adopted voluntarily by nations, it can be hard to obtain causal estimates of the effects of the policy due to endogeneity, as those who choose the tax may fundamentally differ from those who did not. This paper proposes solving the problem through the use a of a two-stage process. In the first stage, country's decisions to adopt a VAT are modeled as a Cox hazard regression, with countries surviving for a number of years, based on a vector of covariates, before adopting a VAT. Countries' survival times are random conditional on the linear hazards from the survival, so any difference in times of VAT adoption is random between two country-year pairs with sufficiently close fitted values. In the second stage, each country-year pair in which an adoption event occurs is matched to a country-year pair in which it does not occur with the closest linear hazards. Then, I compare the difference in means for investment, growth, trade, inflation and government spending between treated and controls at time of adoption and several years after the adoption event, comparing outcomes before the event as a falsification test. There is evidence that VATs lower inflation and government spending, as well as raise investment and growth. Countries most likely to adopt VATs trade less as a share of GDP than countries as a whole, and those that adopt VATs do see a statistically

sigificant gain in trade, but no larger than those of matched controls that do not adopt the treatment.

These findings can be of use to many policymakers considering implementing a VAT. VATs carry with them no major side effects in the form of lowering trade and may raise growth and investment, with no feared adverse effects on inflation or government consumption. As these results are the mean effects of VAT adoption, they leave the door open to investigating more detailed aspects of VAT adoption. This study only includes VAT adoption events in over roughly 2 decades, and many earlier VAT adopters may have had systematically different experiences, and future ones may see different impacts as well. This study does not keep track of VAT rates themselves, how many different VAT categories are used by the country, the minimum firm size to register for the VAT, or the speed at which one receives rebates. All of these are potentially major determinants of the ultimate effect of a VAT which could prove important to the actual effect of VAT adoption (Edminston and Fox 2006). Finally, many VATs did not arise in a vacuum but rather took their place among some existing taxes and perhaps replaced others. Thus, the true impact of any VAT is likely dependent on a rich set of nation-specific tax and other factors. Rather, this paper provides a jumping off point for empirical studies of finer points of the VAT, while at the same time applying a new method to solve important unanswered questions about the empirics of this popular tax policy.

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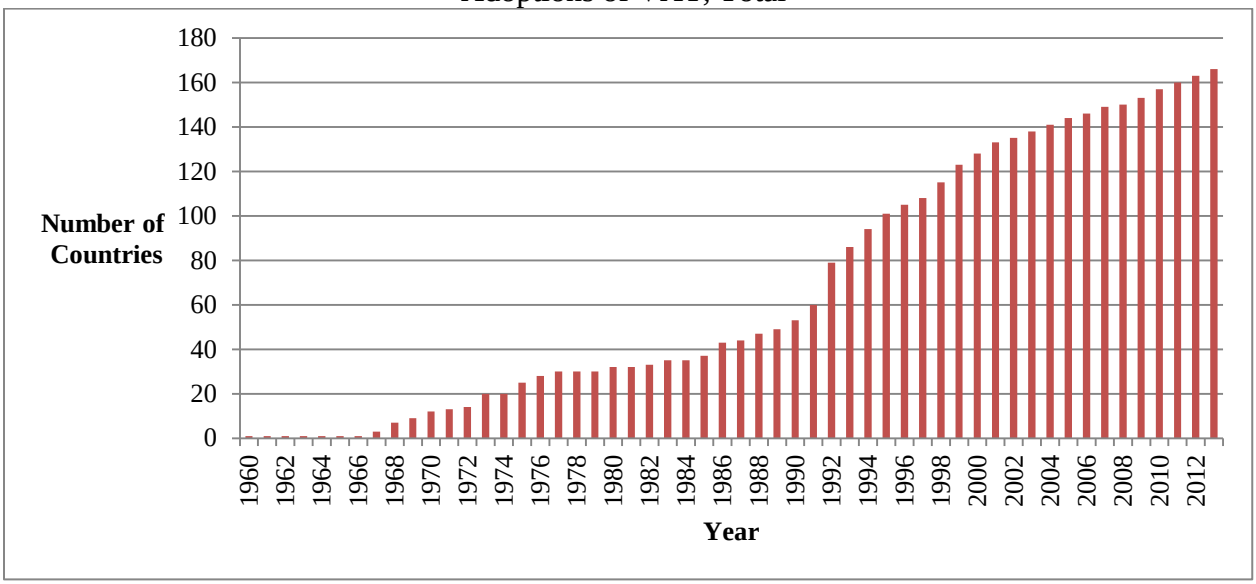
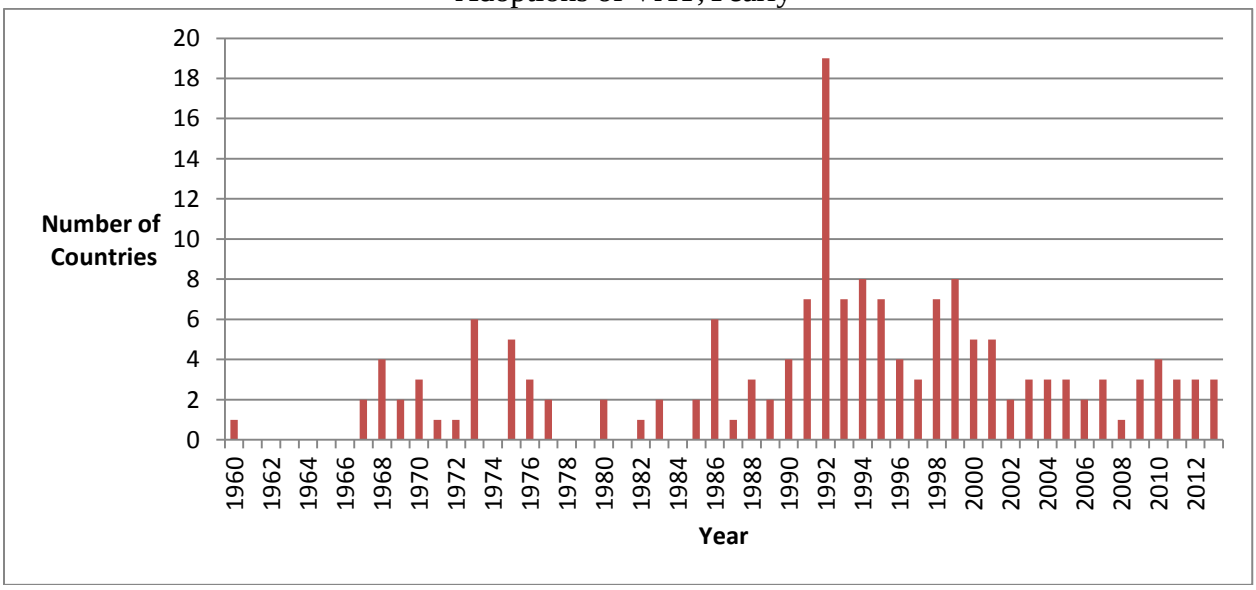
FIGURE 1
Adoptions of VAT, Total



Source: Author's Calculations

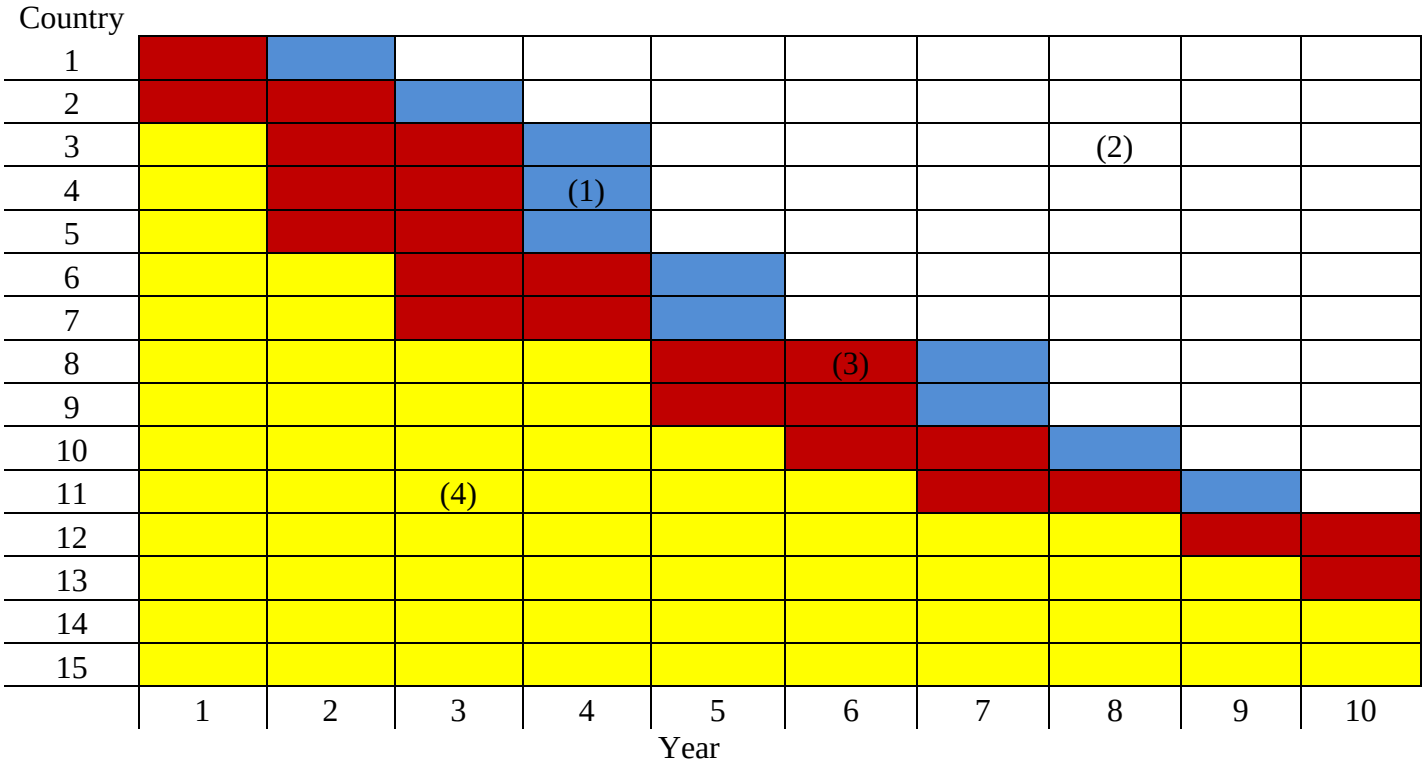
FIGURE 2
Adoptions of VAT,Yearly



Source: Author's Calculations

Note for Figures 1 and 2: Vietnam in the 1970s, Grenada in 1986, Ghana in 1995, Malta in 1998, and Belize in 1999 all passed a VAT and later repealed it. Vietnam, Grenada, and Ghana would later re-adopt a VAT in 1999, 2010, and 1998 respectively, and these re-introduction events are included in this paper. Belize and Malta are included with original adoption dates. This is in line with what was done by Keen and Lockwood (2010). Omitting these observations does not qualitatively change results.

FIGURE 3
Visual Explanation of Matching



Region (1) are country-years that have adoption events. These are the treatment observations. Information from these country-years are used in both the selection equation and matching.

Region (2) are country-years after adoption events. Information from this region is neither included in the selection equation nor the matching equation.

Region (3) are the 5 country-years pre-adoption that cannot be used for matching due to the need to have control country-years that do not adopt VATs for 5 years after being assigned as a control. They still provide information to the selection equation.

Region (4) are country-years 5 years or more before an adoption event. This is the pool of controls. Information from these country-year pairs are included in both the selection equation and matching.

Matching for this paper covers more countries than the 15 listed and a longer period than the 10 listed above, and so this figure is presented solely for demonstration purposes.

TABLE 1
Summary Statistics, Right Hand Side Variables, 1965-2010

Variable	Obs.	Mean	Std. Dev.	Min	Max
Government Consumption	3692	14.22	10.86	0.64	69.83
Inflation	3357	29.01	488.18	-49.42	26762.02
Investment	3060	22.89	10.53	-17.40	113.58
GDP per Capita Growth	3350	1.91	7.12	-50.29	92.59
Trade	3213	78.72	53.33	0.31	432.30
IMF Lending Dummy	1929	0.49	0.50	0	1
Agriculture	2861	23.45	17.51	0	93.98
GDP, Log	3364	22.19	2.31	16.13	30.09
GDP Per Capita, Log	3360	7.32	1.54	4.00	11.02
Natural Resource Rents	3003	10.00	15.93	0.00	155.22
Bordering Countries with VAT	4823	0.15	0.29	0	1
% of Population 0-14	4425	38.25	8.81	11.51	53.03
% of Population 65+	4425	4.87	2.94	0.43	14.63
Population Growth	4790	2.10	1.71	-11.00	18.59
Population, Log	4791	14.75	2.18	8.83	20.88
Size, Log Square Kilometers	4823	10.93	2.87	0.69	16.61
Association of Small Island States	4823	0.23	0.42	0	1
Former Soviet Republic	4823	0.07	0.25	0	1
Former Warsaw Pact	4823	0.07	0.26	0	1
British Commonwealth	4823	0.33	0.47	0	1
Francophonie	4823	0.28	0.45	0	1
Island	4823	0.24	0.43	0	1
Landlocked	4823	0.22	0.42	0	1
Federal	4823	0.08	0.27	0	1
Asia	4823	0.30	0.46	0	1
Europe	4823	0.16	0.37	0	1
Africa	4823	0.31	0.46	0	1
North America	4823	0.11	0.32	0	1
South America	4823	0.04	0.19	0	1
Pacific	4823	0.08	0.27	0	1

Notes: Observational units are country-year pairs.

TABLE 2

Cox Hazard Regressions for VAT Adoption

	(1) 1986-2012	(2) 1986-2012	(3) 1986-2012	(4) 1967-2012	(5) 1967-2012
Government Consumption	-0.022				
	(0.017)				
Inflation	-0.007				
	(0.005)				
Investment	0.011	0.011			
	(0.015)	(0.014)			
GDP per Capita Growth	-0.010	-0.002			
	(0.019)	(0.018)			
Trade	-0.003	-0.002			
	(0.004)	(0.004)			
IMF Lending Dummy	1.253***	1.293***	1.302***		
	(0.360)	(0.347)	(0.342)		
Agriculture	-0.008	-0.005	-0.009		
	(0.014)	(0.013)	(0.012)		
GDP, Log	6.359	6.453	6.566		
	(4.505)	(4.184)	(4.246)		
GDP per Capita, Log	-5.968	-5.953	-6.106		
	(4.491)	(4.172)	(4.236)		
Natural Resource Rents	-0.030**	-0.034**	-0.035**		
	(0.014)	(0.014)	(0.013)		
Bordering Countries with VAT	0.512	0.589	0.608	-0.071	
	(0.450)	(0.441)	(0.428)	(0.324)	
% of Population 0-14	-0.004	0.000	-0.005	0.038	
	(0.042)	(0.040)	(0.039)	(0.024)	
% of Population 65+	-0.043	-0.017	0.002	0.152**	
	(0.112)	(0.108)	(0.106)	(0.073)	
Population Growth	0.004	0.011	0.012	-0.017	
	(0.090)	(0.086)	(0.086)	(0.069)	
Population, Log	-5.930	-5.978	-6.066	0.362***	
	(4.491)	(4.171)	(4.236)	(0.094)	
Size, Log of Square Kilometers	-0.032	-0.071	-0.078	-0.095	0.133***
	(0.106)	(0.103)	(0.103)	(0.071)	(0.046)
Association of Small Island States	-0.372	-0.419	-0.393	-0.674	-1.085***
	(0.587)	(0.551)	(0.534)	(0.412)	(0.403)
Former Soviet Republic	-	0.368	0.369	0.245	0.409
	-	(1.281)	(1.214)	(0.407)	(0.356)
Former Warsaw Pact	-0.953	-1.109	-1.290*	-0.958**	-0.679*
	(0.716)	(0.692)	(0.687)	(0.432)	(0.374)
British Commonwealth	0.005	-0.001	-0.012	0.303	0.308
	(0.316)	(0.317)	(0.311)	(0.226)	(0.220)
Francophonie	0.681**	0.683**	0.685**	0.487**	0.404**
	(0.300)	(0.293)	(0.290)	(0.206)	(0.196)
Island	0.685	0.794*	0.780	0.012	0.189
	(0.484)	(0.477)	(0.476)	(0.388)	(0.347)
Landlocked	0.076	0.117	0.143	0.049	-0.092
	(0.321)	(0.303)	(0.298)	(0.214)	(0.204)
Federal	-2.056***	-2.184***	-2.201***	-1.047***	-0.695**
	(0.654)	(0.649)	(0.649)	(0.374)	(0.349)
N	1287	1317	1400	4421	4823
Pseudo-R^2	0.118	0.112	0.117	0.071	0.051
Log Likelihood	-313.114	-328.122	-334.086	-635.170	-697.526
Chi Squared	83.686	82.685	88.906	96.815	74.938
Subjects	112	113	114	178	192
Adoptions	90	95	96	150	158

t statistics in parentheses.

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: Observational units are country-year pairs; Values are coefficients for linear hazards, not hazard ratios. A coefficient of 0 thus implies no effect. To find the hazard ratios, exponentiate the coefficient; All independent variables are lagged 2 periods; All estimates performed with regional dummies for North America, South America, Europe, Asia, Africa, and Pacific, the last being omitted for co-linearity.

TABLE 3
Balance of Covariates for Matching

Variable	Sample	Treated	Controls	Difference	S.E.	T-stat
Government Consumption	Unmatched	11.82	15.03	-3.21	1.18	-2.71***
	ATT	11.82	9.84	1.98	1.32	1.5
Inflation	Unmatched	12.20	77.66	-65.46	105.71	-0.62
	ATT	12.20	11.11	1.09	4.07	0.27
Gross Capital Formation	Unmatched	22.21	22.73	-0.52	1.25	-0.41
	ATT	22.21	22.23	-0.02	1.78	-0.01
GDP per Capita Growth	Unmatched	1.63	1.63	0.00	0.73	0.00
	ATT	1.63	1.06	0.57	0.92	0.61
Trade	Unmatched	74.66	84.43	-9.77	5.50	-1.78*
	ATT	74.66	62.99	11.67	8.07	1.45
IMF Lending Dummy	Unmatched	0.72	0.47	0.26	0.06	4.63***
	ATT	0.72	0.69	0.03	0.08	0.4
Agriculture	Unmatched	22.47	22.38	0.09	1.91	0.05
	ATT	22.47	23.78	-1.31	3.04	-0.43
GDP, Log	Unmatched	22.66	22.04	0.63	0.26	2.41***
	ATT	22.66	23.27	-0.60	0.47	-1.29
GDP Per Capita, Log	Unmatched	7.07	7.10	-0.03	0.17	-0.19
	ATT	7.07	7.14	-0.07	0.31	-0.24
Natural Resource Rents	Unmatched	6.76	11.82	-5.06	1.61	-3.14***
	ATT	6.76	7.92	-1.16	1.94	-0.6
Bordering Countries with VAT	Unmatched	0.38	0.21	0.17	0.04	4.60***
	ATT	0.38	0.28	0.10	0.07	1.43
% of Population 0-14	Unmatched	35.99	39.83	-3.84	0.85	-4.52***
	ATT	35.99	36.53	-0.55	1.72	-0.32
% of Population 65+	Unmatched	5.62	4.14	1.48	0.27	5.41***
	ATT	5.62	5.53	0.09	0.64	0.15
Population Growth	Unmatched	1.82	2.17	-0.35	0.17	-2.12**
	ATT	1.82	2.01	-0.19	0.23	-0.8
Population, Log	Unmatched	15.60	14.93	0.67	0.24	2.82***
	ATT	15.60	16.12	-0.52	0.36	-1.46
Size, Log Square Kilometers	Unmatched	11.77	11.25	0.52	0.31	1.7
	ATT	11.77	12.44	-0.66	0.45	-1.46
Asia	Unmatched	0.19	0.31	-0.12	0.05	-2.37**
	ATT	0.19	0.23	-0.04	0.07	-0.6
Europe	Unmatched	0.18	0.02	0.16	0.02	8.33***
	ATT	0.18	0.12	0.06	0.06	1
Africa	Unmatched	0.44	0.43	0.01	0.06	0.25
	ATT	0.44	0.48	-0.03	0.09	-0.37
North America	Unmatched	0.07	0.14	-0.08	0.04	-2.05**
	ATT	0.07	0.09	-0.02	0.05	-0.41
South America	Unmatched	0.06	0.04	0.02	0.02	0.9
	ATT	0.06	0.03	0.02	0.04	0.58
Association of Small Island States	Unmatched	0.17	0.27	-0.11	0.05	-2.17**
	ATT	0.17	0.11	0.06	0.06	0.9
Former Warsaw Pact	Unmatched	0.11	0.01	0.10	0.01	7.41***
	ATT	0.11	0.01	0.10	0.04	2.66***
British Commonwealth	Unmatched	0.40	0.46	-0.06	0.06	-1.09
	ATT	0.40	0.44	-0.04	0.09	-0.5
Francophonie	Unmatched	0.39	0.28	0.11	0.05	2.25**
	ATT	0.39	0.34	0.04	0.08	0.52
Island	Unmatched	0.21	0.26	-0.05	0.05	-0.98
	ATT	0.21	0.18	0.03	0.07	0.48
Landlocked	Unmatched	0.23	0.21	0.03	0.05	0.55
	ATT	0.23	0.19	0.04	0.08	0.58
Federal	Unmatched	0.08	0.11	-0.03	0.03	-0.97
	ATT	0.08	0.13	-0.06	0.06	-0.89
Year	**Unmatched**	**1996.98**	**1994.46**	**2.52**	**0.70**	**3.6*** **
	ATT	**1996.98**	**1992.39**	**4.59**	**1.13**	**4.06*** **
Propensity Score	**Unmatched**	**8.07**	**6.31**	**1.76**	**0.80**	**2.22** **
	ATT	**8.07**	**8.05**	**0.02**	**0.17**	**0.14**

$^{*}p < .1$, $^{**}p < .05$, $^{***}p < .01$

Total Observations: 863
Untreated Observations: 773
Treated Observations: 90

TABLE 4
Summary of Matching Results, Single Nearest Neighbor Matching

T Statistic, Mean Difference, Treated and Matched Controls					
	Investment	GDP per Capita Growth	Trade	Inflation	Government Consumption
7 Years Pre-Adoption	0.14	0.03	0.87	-0.32	0.86
6 Years Pre-Adoption	-0.52	0.51	0.48	-0.77	0.67
5 Years Pre-Adoption	-0.36	-1.22	0.52	-0.36	0.79
4 Years Pre-Adoption	0.41	-1.45	0.83	0.35	0.99
3 Years Pre-Adoption	-0.27	0.32	0.95	0.25	0.83
2 Years Pre-Adoption	-0.55	0.67	1.07	0.48	0.92
1 Year Pre-Adoption	0.00	0.59	0.86	0.00	0.57
Year of Adoption	-0.37	2.17**	0.88	0.40	0.18
1 Year Post-Adoption	0.31	0.38	1.10	-0.68	0.58
2 Years Post-Adoption	0.69	1.20	1.09	-0.60	0.47
3 Years Post-Adoption	1.11	1.79*	1.16	-1.51	0.65
4 Years Post-Adoption	0.97	1.93*	0.80	-1.82*	0.35
5 Years Post-Adoption	1.21	1.38	0.87	-1.89*	0.01
Change 2 Years Pre-Adoption to 5 Years Post-Adoption	**1.69***	**0.49**	**-0.25**	**-1.97***	**-1.75***
Change 7 Years Pre-Adoption to 2 Years Pre-Adoption	**-0.79**	**0.37**	**0.35**	**0.47**	**-0.21**
Treated Observations	77	78	76	78	80
Untreated Observations	639	683	662	684	716

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: T statistics above for mean differences, treated (adopters) minus untreated (matched controls); Propensity scores for matching are generated by hazard regression (1) on Table 2.

TABLE 5
Comparison of Matching Methods

Variable, Change 2 Years Pre-Adoption to 5 Years Post-Adoption	1 Nearest Neighbor	1 Nearest Neighbor, No Replacement	3 Nearest Neighbors	Radius, .025	Radius, .05	Kernel, Epanechnikov	Kernel, Normal	Total Significant at 10% level
Investment	1.69*	2.13**	2.03**	1.56	1.60	2.40**	2.13**	5
GDP per Capita Growth	0.49	1.71*	0.64	0.22	0.79	0.06	0.85	1
Trade	-0.25	0.23	0.47	-0.40	-0.25	1.32	0.94	0
Inflation	-1.97**	-1.41	-1.14	-0.94	-2.52***	-1.66*	-1.78*	4
Government Consumption	-1.75*	-1.77*	-1.53	-1.90*	-1.96*	-1.18	-1.14	4

Variable, Change 7 Years Pre-Adoption to 2 Years Pre-Adoption	1 Nearest Neighbor	1 Nearest Neighbor, No Replacement	3 Nearest Neighbors	Radius, .025	Radius, .05	Kernel, Epanechnikov	Kernel, Normal	Total Significant at 10% level
Investment	-0.79	-0.74	-0.79	-1.20	-1.18	-1.28	-0.75	0
GDP per Capita Growth	0.37	-0.07	0.07	-0.03	-0.20	-0.30	0.11	0
Trade	0.35	-0.02	0.00	-0.05	-0.16	0.01	0.20	0
Inflation	0.47	0.08	0.29	1.33	1.49	1.01	0.05	0
Government Consumption	-0.21	-0.46	-0.30	-0.49	-0.31	-0.41	-0.34	0

t statistics in parentheses.
* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: Propensity scores for matching are generated by hazard regression (1) on Table 2.