The Impact of Assisted Dying on Suicidality: A Synthetic Control Analysis of Population Suicide Rates

by

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The above committee determined that the thesis is acceptable in form and content and that a satisfactory knowledge of the field covered by the thesis was demonstrated by the candidate during an oral examination. A signed copy of the Certificate of Approval is available from the School of Graduate and Postdoctoral Studies.

ABSTRACT

Adoption of assisted dying is growing, but one argument against these policies is that they cause a suicide contagion, thus increasing suicides. Evidence for the assisted dying-suicide link does not currently exist, and in general, research is scarce. I add to this body of literature to help stakeholders understand the consequences of such policies. I performed a time series study to determine how suicide rates changed in Belgium after assisted dying was introduced in 2002. The synthetic control method was chosen for the analysis, and additional European nations were included as placebos. The results show no change in suicide rates following the assisted dying policy change. A comparison of MSPE ratios with placebo countries also suggest statistical insignificance ($p = 0.29$). I conclude that there is no evidence that Belgium's assisted dying policy is associated with a change in suicide rates or a suicide contagion.

Keywords: assisted dying; synthetic control method; suicide rate; suicide contagion; Belgium

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STATEMENT OF CONTRIBUTIONS

I hereby certify that I am the sole author of this thesis and that no part of this thesis has been published or submitted for publication. I have used standard referencing practices to acknowledge ideas, research techniques, or other materials that belong to others. Furthermore, I hereby certify that I am the sole source of the creative works and/or inventive knowledge described in this thesis.

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LIST OF ABBREVIATIONS AND SYMBOLS

CCA	Council of Canadian Academies
CR	Czech Republic
DC	District of Columbia
MAID	Medical Assistance in Dying
MSPE	Mean Squared Prediction Error
OECD	Organisation for Economic Co-operation and Development
PAS	Physician assisted suicide
SCM	Synthetic Control Method
UK	United Kingdom
USA	United States of America

Chapter 1. Introduction and Background

Suicide contagion, formally known as the Werther effect, is a social phenomenon where suicide by a family member, peer group, or in the media can trigger suicides in susceptible (i.e., have pre-existing suicidal ideation) individuals (Gould & Lake, 2013). Several groups have raised concerns that assisted dying policies can act as a suicide contagion (Callahan, 1994; Jones & Paton, 2015; Kheriaty, 2015; Dunn, 2017; Francis, 2017), which could lead to an increased number of suicides. Proponents of this connection argue that assisted dying sets a precedent that death is an acceptable solution for suffering, leading to a greater risk of suicide in a society that embraces the practise (Callahan, 1994; Dunn, 2017; Jones & Paton, 2015; Kheriaty, 2015). Evidence suggests that suicide contagion spreads via differential identification, meaning that victims must feel a sense of shared identity with the contagion's source (Stack, 1990). This sense of shared identity can involve a number of characteristics, including similar health or socioeconomic problems.

To my knowledge, a paper authored by Jones and Paton (2015) is the only study that attempts to find evidence of a link between assisted dying and suicide rates. Jones and Paton studied assisted dying policies in Oregon and Washington State, and they found no change in suicide rates after the introduction of these policies. However, a major limitation exists in this study: assisted dying policies in Oregon and Washington permit only patients with a terminal illness to access the procedure (Nicol & Tiedemann, 2015). More specifically, the problem is that these states' assisted dying policies would be unsuccessful in setting a precedent that death is an acceptable solution for suffering because the vast majority of suicidal individuals do not identify with the suffering of

terminal illness; instead, individuals with suicidal ideation largely identify with mental disorders (Hawton & van Heeringen, 2009). As it stands, the risk of a false negative is high in Jones and Paton's study because the population in Oregon and Washington that is vulnerable to a suicide contagion is very small. Detection of a contagion, if it exists, would be much more likely if mental disorders were also eligible for assisted deaths. To build on the existing work, this study asks the following question: how do assisted dying policies, that also include assisted dying for mental disorders, affect the suicide rate of the general population?

This paper will build on the Jones and Paton paper by using a form of time series analysis called the synthetic control method (Bouttell, Craig, Lewsey, Robinson, & Popham, 2018). Additionally, it will analyze data from jurisdictions that have permitted assisted dying for a broad range of disorders, including mental disorders. These jurisdictions include Belgium, the Netherlands, Luxembourg, and Switzerland (Nicol & Tiedemann, 2015). The results of this study should be informative for any jurisdiction that allows assisted dying, or those planning to expand the scope of their policies to allow assisted dying for persons with a broader range of health conditions.

This thesis will begin with a background of the assisted dying policies in American states and European nations. A background on suicide contagion is also provided. Following the background material is a literature review of the existing evidence, along with details of the limitations in past evidence that need to be overcome to produce a more robust study. This paper then goes on to outline the methods that are used, followed by the results and a discussion.

1.1 Background: Assisted Dying Policies

Different jurisdictions often use different terminology to describe assisted dying, and the distinctions are important because they signal differences in practise (Nicol & Tiedemann, 2015). Euthanasia generally refers to the act of someone else administering a lethal agent on behalf of the patient, such as when a physician uses an injection to induce death directly. Physician assisted suicide, sometimes called physician assisted death, defines the act where patients are prescribed a lethal medication but consume it themselves. Some jurisdictions explicitly outlaw one practice while allowing the other. This paper will not make a distinction between euthanasia and physician assisted suicide because several nations allow both; separating their individual contributions to a suicide contagion would not be possible or conducive to answering the research question. In this paper, both practises are referred to as "assisted dying."

1.1.1 Assisted dying in the United States. Oregon was the first American state to legislate assisted dying with the 1994 *Death With Dignity Act* (Nicol & Tiedemann, 2015). The legislation took effect in 1997, decreeing that adult residents of Oregon, with a life expectancy of less than six months due to terminal disease, could qualify for an assisted death (Nicol & Tiedemann, 2015). Because of the 6-month rule, a large majority of assisted dying cases involve patients with end-stage cancer or ALS, making up 69% and 16% percent of all assisted deaths respectively (Nicol & Tiedemann, 2015). The state of Washington modeled its policy based on Oregon's and has similar figures, with 70% of assisted deaths related to cancer and 13% with ALS (Nicol & Tiedemann, 2015). Washington's bill took effect in 2009, making it the second state to legislate assisted dying.

The state of Vermont also based their assisted dying policy on Oregon's. Because the legislation took effect in 2013, and because no health reports on assisted dying will be released until 2018, statistics are not available for this state (Nicol & Tiedemann, 2015). The state of Montana is a unique case. Rather than introducing legislation to legalize the practise, a state constitutional challenge found that prohibiting the right to die for competent and terminally ill adults violated human dignity, and that physicians can protect themselves from homicide charges by proving the patient's consent(Nicol & Tiedemann, 2015). A lack of official framework is somewhat problematic as no statistics or other information can be garnered from the state, despite the decision having been made eight years ago (Nicol & Tiedemann, 2015).

The past two years have seen four additional districts introduce legislation to allow assisted dying. Colorado, California, and Washington DC had their assisted dying laws take effect in 2016-2017, and Hawaii's will take effect in 2019 (Nicol & Tiedemann, 2015). Because of how new these policies are, nopublished information is available to analyze.

1.1.2 Assisted dying in Europe. European countries have the most permissive assisted dying laws in the world. The Netherlands has a long history in conducting the practise and is thus often the subject of research (Nicol & Tiedemann, 2015). Assisted dying legislation officially came into effect in 2002. However, the practise of assisted dying has existed for three decades prior to the implementation of the 2002 legislation (Rietjens, van der Maas, Onuwuteaka-Philipsen, van Delden, & van der Heide, 2009). The legitimacy of assisted dying in the Netherlands was first acknowledged in a 1973 court case where a physician was prosecuted for helping her mother die; the sentence the

physician received was later suspended by the court. This incident sparked the gradual acceptance and utilization of assisted dying, but with no real oversight, and no clear-cut statement of decriminalization (Rietjens et al., 2009). During this period, assisted deaths were usually provided for terminally ill patients, but a court battle in 1994 oversaw the earliest case of assisted dying for grief that was solely psychological (Rietjens et al., 2009). The Supreme Court of the Netherlands agreed that assisted dying could extend to cases where no physical illness is present, and the practise was not forbidden when assisted dying was legislated in 2002. Aside from the Netherlands and a select few European countries, no jurisdiction allows assisted dying for diseases that do not reduce life span (Nicol & Tiedemann, 2015).

Belgium's assisted dying legislation also took effect in 2002 and is also one of the select few jurisdictions that grant requests based solely on psychological grief (Nicol & Tiedemann, 2015). Unlike the Netherlands though, Belgian law explicitly states that mental disorders can qualify patients for an assisted death, whereas this clause is inferred through the wording of legislation in the Netherlands and previous court battles (Naudts et al., 2006). Also, unlike the Netherlands, Belgium did not have a history of assisted dying practise prior to the implementation of assisted dying legislation (Naudts et al., 2006).

Luxembourg's assisted dying legislation came into effect in 2009 (Wirtz, 2017). It bears many similarities with Belgium's framework, and Luxembourg also has no history of physicians providing assisted deaths prior to the legislation (Wirtz, 2017). Like the other European nations that allow assisted dying, mental disorders, or other disorders that

do not reduce the lifespan, do not exclude patients from receiving an assisted death (Wirtz, 2017).

The situation in Switzerland is unique due to the length of time that the policy has been in effect. Assisted dying is not explicitly regulated in Switzerland, but is implied based on the wording of the 1942 Swiss Penal Code (Schwarzenegger & Summers, 2005). The lack of regulation results in two unique approaches to the practise; the first allows non-physicians to provide an assisted death, and the second allows foreigners to travel to the country and receive an assisted death (Hurst, 2003). Switzerland has been involved in several high profile assisted dying cases because of this latter feature (Hurst, 2003). The Netherlands, Belgium, and Luxembourg do not have residency requirements either, but it is necessary to have a working relationship with a physician in the country (Nicol & Tiedemann, 2015). Due to practical limitations, this essentially prohibits foreigners from travelling to the nation for an assisted death.

1.2 Background: The Werther Effect

The Werther effect is named after the protagonist of a book, published in 1774, called *The Sorrows of Young Werther* (Niederkrotenthaler, Herberth, & Sonneck, 2007). The book depicts a young man named Werther performing suicide by shooting himself with a pistol. Not long after the book's publication, reports began to surface that an abnormal number of young men were found to have performed suicide using the same method that Werther used (Devitt, 2017). Whether this book truly caused a spike in suicides has not been firmly established, but researchers have substantial evidence of this phenomenon occurring in the modern era (Gould & Lake, 2013; Stack, 2003). This phenomenon is also known as suicide contagion because of how news of a notable

suicide spreads across a population and seems to invoke suicidal thoughts in some individuals as if it were an abstract infection (Cheng, Li, Silenzio, & Caine, 2014; Gould & Lake, 2013). Suicide contagion is the term favored in the literature that connects assisted dying with an increase in suicides, and that prompted my adoption of the term in this thesis.

Just as young men in the late 18th century used a pistol to perform suicide, mirroring Werther's actions in the novel, victims of modern-era suicide contagions also tend to use the same method as the contagion's source (Cheng et al., 2014; Stack, 2003). It is believed that modern suicide contagions are most often the result of extensive media coverage of celebrity suicides (Jang, Sung, Park, & Jeon, 2016). It also appears that not just anyone can become the victim of a suicide contagion. Having pre-existing suicidal ideation or displaying various risk factors for suicide are traits that are seen in individuals that have become victims of suicide contagions (Gould, 2006; Cheng et al, 2007; Gould & Lake, 2013). Consequently, this means that there is a specific subset of the population that is vulnerable. One final characteristic of suicide contagion relevant to this study is the detection of evidence that a contagion had occurred. Past studies have shown that contagion-starting events, especially when it involves the suicide of a high-profile celebrity, can alter suicide rates at the aggregate or population level (Cheng et al., 2014). This implies that detecting evidence of a suicide contagion using population suicide rates is a feasible initiative.

1.2.1 Differential identification. The primary driver of the Werther effect is explained by a social theory known as differential identification (Stack, 2003). The application of this theory to the Werther effect suggests that a suicide contagion spreads

to victims that can in some way identify their personal traits or circumstances with the source of the contagion (Stack, 2003).

Identity characteristics that can result in the spread of a contagion include admiration, occupation, age, sex, and other demographic factors. (Gould, 2006; Jang et al., 2016). Circumstances such as divorce and other marital problems that already serve as risk factors for suicide can also result in the spread of a suicide contagion because victims begin to think of one person's suicide as a solution to their own problems (Stack, 1990; Olson, n.d.). Researchers have found evidence that a broad range of difficulties can cause differential identification and lead to suicide (Ministry of Health, 1999).

1.2.2 Assisted dying and suicide contagion. One argument that cautions against implementing assisted dying policies is that they can act as a suicide contagion (Callahan, 1994; Dunn, 2017; Francis, 2017; Jones & Paton, 2015; Kheriaty, 2015). It is claimed that legalizing assisted dying normalizes, destigmatizes, and legitamizes death as a valid solution for suffering (Callahan, 1994; Dunn, 2017; Francis, 2017; Jones & Paton, 2015; Kheriaty, 2015).

However, this connection may be limited by differential identification. A society that accepts assisted dying for terminal illness would at most convey that a hastened death is an acceptable solution to suffering when patients are already on the verge of death. If society rejects assisted dying for other forms of illness, such as mental disorders, then such behaviour would not be normalized, destigmatized, or legitimized; this sends a message that an early death is not an acceptable solution to the suffering brought on by that illness. Assuming that suicide contagion does occur, a consequence of differential identification is that different assisted dying policies will create contagions of differing

severity. If only a narrow range of medical circumstances are deemed acceptable for assisted dying, such as with American states and their 6-month prognosis rule, then a contagion will have very few victims to spread to. In such a scenario, a detectable increase in the population's suicide rate may not occur, even if the hypothesis was true. This is because few people would have been exposed to the intervention. If suicide rates for only the vulnerable cohort were available, then detecting a contagion would be easy. Because suicide rates are collected for the entire population, test sensitivity becomes a problem. For this reason, a suicide contagion is most likely to be detected in jurisdictions that have implemented an assisted dying policy that includes a very broad range of medical conditions. It is especially crucial that mental disorders are included in the policy because they are the diseases that place patients at greatest risk for suicide, along with being attributed to the greatest number of suicides in high-income western nations (Hawton & van Heeringen, 2009).

1.2.3 Limitations in the assisted dying-suicide contagion hypothesis.

Proponents of the connection between assisted dying policy and suicide contagion base their statements on intuition derived from pre-existing literature about suicide contagion, such as studies of celebrity suicides. While there may be an intuitive relationship between assisted dying and suicide contagion, this connection is based on a number of implicit assumptions that are not supported by corroborating evidence.

The first assumption is that assisted dying normalizes, destigmatizes, and legitimizes suicide. However, the differences between these two acts mean that they cannot be so easily equated. Assisted dying is a very medicalized procedure that involves consultation and coordination with healthcare practitioners and family members (CCA,

2018). Suicide is the opposite, carried out in isolation, and is not a decision that healthcare practitioners support. It is unclear if the general public would perceive these acts in a way that one legitimizes the other. Considering that health professions still denounce suicide, there is reason to doubt this assumption.

Second, suicide contagion typically involves the replication of a suicide method. This phenomenon was thought to have occurred when the story of Young Werther allegedly caused a suicide contagion. It was reported that young male suicides were often performed with a pistol, just as Young Werther had done (Devitt, 2017). Modern evidence of this phenomenon shows that when a celebrity's suicide method is reported, suicides in the general population that use that particular method suddenly increase (Jang, Sung, Park, & Jeon, 2016). In regard to assisted dying however, replicating the method of death is not feasible. Once again, this a highly medicalized procedure, and attaining easily ingestible drugs that can induce death is difficult. Replicating the method of death is especially difficult in jurisdictions that favor euthanasia because this requires a second person to directly administer a lethal agent.

Third, suicide contagion relies on identification with the characteristics and/or circumstances of the individual who has died. It is unclear how this identification will occur in circumstances where the characteristics of individuals or the circumstances of their death are not made public. Even if they were, the general public relating their medical disorders to those that are approved for assisted deaths may not be enough to establish identification. Another characteristic of suicide contagion to consider is that young people, including adolescents, are most often affected; this is thought to be because they are more impressionable (Cheng et al., 2014). On the other hand, assisted

dying typically involves older adults with diseases of old age. (Nicol & Tiedemann, 2015). The age groups that are most vulnerable to a suicide contagion are unlikely to relate to or feel inspired by the people and circumstances that are prolific in assisted dying.

1.3 Considerations for Policy Development

This thesis aims to help guide policy development by allowing stakeholders, such as physicians and legislators, to understand and weigh the benefits and harms of assisted dying policies.

If evidence of a contagion is found, it would likely be at the forefront of any discussion that involved legalizing assisted dying for mental disorders. Furthermore, this discussion would have to extend to other incurable disorders, such as chronic physical disorders. This is because after mental disorders, chronic physical disorders are also strongly associated with suicide and suicidal ideation (Shen & Millet, 2012). Evidence that favours the contagion theory might also cause a shift in physician support for assisted dying.

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Chapter 2. Effects of Assisted Dying on Suicidality: A Literature Review

The following review explores studies of assisted dying and suicide rates in Oregon and Washington, as well as a single study, cases, and anecdotes from Europe that relate assisted dying and suicidal ideation.

2.0.1 Research strategy. The databases consulted for this literature review include PubMed, PsycINFO, and Social Science Citation Index (Web of Science Core Collection). Key words were structured with Boolean operators as (*"suicide rate" OR suicidality OR "suicidal ideation") OR (“suicide contagion” OR “Werther effect” OR “copycat suicide”)AND (euthanasia OR "physician-assisted suicide" OR "assisted dying" OR "assisted death")*. References used in the extracted articles, and articles that cited the extracted articles were also assessed for relevant material.

The search results were limited to peer-reviewed journal articles and articles in English. The search range was limited to exclude articles prior to 1940 as this was approximately when the world’s first assisted dying policy came into effect. There were 145 results from PubMed, 13,455 from PsycInfo, and 11,039 from Social Science Citation Index. Articles were included if they discussed how assisted dying, as a policy or as a medical procedure, affected suicides, suicidality, or a general desire to die. Additional material was found through a general Google search, but these items were limited to informal cases and anecdotes.

2.1 How Suicidal Ideation is Affected by Assisted Dying

There is an abundance of anecdotal evidence and cases where individuals explain how their suicidality is affected by assisted dying; this paper does not present an exhaustive list of this evidence, but it will go over some of the more prominent cases. Two particularly well-known cases come from two separate documentaries, with both featuring Belgian patients seeking assisted deaths due to psychiatric suffering. *Road to Mercy* is a documentary produced in association with the Canadian Broadcasting Corporation and features several different stories of patients seeking assisted deaths (Pequeneza, Bachinski, Bielz, & Khaskin, 2017). One of the stories in this documentary focuses on Amy De Schutter, a 29-year old female with a mental disorder that was declared treatment resistant after several years of failed therapies. Amy's request for an assisted death is eventually approved, but in a twist, and despite unsuccessful past suicide attempts, she decides not to go through with the procedure, stating that the option being available is enough to comfort her (Pequeneza et al., 2017). In a very similar story published by The Economist (2015), *24 & Ready to Die* is a film about a Belgian woman who is also losing her lengthy battle with mental illness. The film follows her as she goes through the various procedures required to receive an assisted death and ends with her deciding to cancel for the same reason as Amy.

The decisions of Amy and Emily are not particularly uncommon, as physicians involved with providing assisted deaths relay similar anecdotes. Dr. Ellen Wiebe is Canada's foremost practitioner of assisted dying, and while discussing Amy's story, she describes seeing this phenomenon in her own patients (Martin, 2017). Other assisted dying practitioners claim to see this happen as well. EXIT is an organization in Switzerland that provides assisted deaths, and they claim that half their patients decide

not to go through with the procedure (EXIT, n.d.). It is important to note though that because Canada does not allow assisted dying for mental disorders, Dr. Wiebe's patients generally do not suffer from them.

2.1.1 Study of Belgian patients. Additional evidence from Belgium comes from a study conducted by Thienpont et al. (2015). This study aimed to analyze the different patterns and characteristics of psychiatric patients that requested assisted deaths in Belgium. The retrospective study observed medical files of 100 consecutive applicants, from October 2007 through December 2011, who had requested an assisted death on the sole basis of their mental disorder. In total, 48 of the 100 applications were accepted. Of the 48 accepted applicants, only 35 went on to receive an assisted death. Of the remaining 13, two performed suicide before the date of their procedure, while the other 11 cancelled their requests. The authors attempted to follow up with this latter group to understand why their requests were cancelled. Two applicants withdrew their requests due to strong family resistance, and one was deemed ineligible because the patient was imprisoned. The remaining eight stated that knowing they had the option was enough to give them peace of mind and continue living. Of the 52 applications that were not accepted, 38 had withdrawn before an accept/reject decision was reached. It is possible that some of these patients experienced feelings similar to the aforementioned eight patients, but the authors did not attempt to follow up with this group. It should be further noted that the follow-up was conducted at the end of 2012, and the patients were still alive at this point. The authors did not state at what approximate date each patient had their requests accepted, and no further follow-ups were carried out, so it is difficult to say how long these

individuals' new-found peace of mind lasted. The authors also recognized that the small sample size of the study makes it difficult to generalize the findings.

2.2 How Suicide Rates are Affected by Assisted Dying

The only study that specifically examines the effect of assisted dying on suicidality was conducted by Jones and Patton (2015). Their study does not look at how individual suicidality is affected, but instead focuses on suicide rates across a population. Four American states were the subject of analysis: Oregon, Vermont, Washington, and Montana; only two of these, Oregon and Washington, could ultimately be analyzed because suicide rate and assisted dying data sets were not available in the other two states. Several factors known to influence or predict the suicide rate were controlled for: unemployment rate, per capita disposable income, black and Hispanic population compositions, percent of religious adherents, whether marijuana was medically or recreationally decriminalized, and if a 0.08 blood alcohol content law was in effect. In addition to comparing suicide rates before and after the policy was implemented, the two states with the intervention were also compared against the other American states without the policy.

The study aimed to understand how suicide rates were affected by assisted dying, but also whether the average age at the time of suicide had increased. The primary method utilized in this study was a grouped logistic regression to determine the association between assisted dying legalization and suicide rates. Suicide rates were split into two categories for this study: total suicides [the combined rate of "traditional" suicides + legally provided assisted deaths] and non-assisted suicides [simply "traditional" suicides]. Though not made entirely clear, total suicides appear to have been

used to test an assumption that legalizing assisted dying results in a substitution effect where an assisted death takes the place of a suicide. Non-assisted suicides were used to test how the actual suicide rate changed in response to the assisted dying policies. The results of the study are adequately summarized with the following findings and their discussion:

> Controlling for state- and year-fixed effects, PAS is estimated to be associated with a 1.6% increase in nonassisted suicide rates; however, this is not statistically significant (95% CI -0.8% to 3.9%). The estimated effect is larger and statistically significant once other covariates are included (4.4%, 95% CI 1.9% [to] 6.8%). When we include state specific linear trends, however, the estimated increase is 1.1% and not statistically significant (95% CI -2.5% to 4.8%)....The results pertaining to nonassisted suicide rates were equivocal. Some estimates suggested that PAS [physician-assisted suicide] also was associated with a significant increase in the rate of nonassisted suicide. When we included state-specific trends, however, the estimated association, although positive, was smaller and no longer statistically significant. (p. 603)

In other words, the suicide rate showed no statistically significant change when controlling for measured confounders.

This study suffers from methodological limitations that necessitate further research on this topic. The chosen statistical model was an unorthodox approach to time series analysis, primarily because it appears to be a rarely used technique with possible validity issues, and no references were provided to indicate otherwise (Gertler, Martinez, Premand, Rawlings, & Vermeersch, 2011). The confounders that were controlled for are

another limitation that Jones and Paton acknowledged in their discussion. Francis (2017) evaluated the covariates that were used by Jones and Paton and found that marijuana criminal status and blood alcohol content laws were not very predictive of the suicide rate; additionally, some covariates highly predictive of the suicide rate, such as population density and divorce rates, were not included in the study.

Another limitation that prevents detection of a suicide contagion is the policies that were the subject of analysis. As mentioned previously, assisted dying laws in Oregon and Washington are highly restrictive. This fact becomes a problem because suicide is overwhelmingly tied to mental disorders. As an example of this, Shen & Millet (2012) found that 70% of suicide victims in Oregon between 2003-2012 had a diagnosed psychiatric disorder, substance abuse problem, or depressed mood. In comparison, 24% of suicide victims had a physical health problem, with the majority involving chronic disorders. The fraction of patients with physical health problems that had a terminal illness with a 6-month prognosis would be fairly small. These aspects of American assisted dying laws are important because of the principle of differential identification. A very small portion of the Oregon and Washington populations would identify their circumstances with those of patients that are eligible for assisted dying, and so a similarly small fraction of the population is at risk of experiencing a suicide contagion, if any, from the assisted dying laws. Even if a suicide contagion did occur, it would be virtually undetectable.

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Chapter 3. Methods

The purpose of this analysis was to determine whether the introduction of an assisted dying policy in the European regions was associated with a suicide contagion. While suicide contagion is not observed directly, suicide rates are used as a proxy outcome. As a time series analysis, this study observed pre-intervention suicide rates and analyzed how they changed in the post-intervention years. The synthetic control method helped control for confounders and other validity threats, which is detailed further in section 3.3.

3.0.1 Ethics. This research project did not have a component requiring human participants. All data used in this study was taken from publicly available data sets, such as the OECD's online statistics portal. This study was approved by the University of Ontario Institute of Technology's research ethics board (REB File #14683).

3.1 Variables

As mentioned previously, suicide rates were the dependent variable of interest. The data sets for suicide rates were taken from the OECD's online statistics database (OECD, 2018e). The data was collected by national civil registration systems and curated in the World Health Organization's (WHO) Mortality Database as crude suicide rates (suicide rates were age adjusted by the OECD). The WHO has attempted to standardize the classification of mortality between countries through its International Statistical Classification of Disease and Related Health Problems (WHO, n.d.). The 10th revision of this classification system has been in effect since 1993 and is still used today, thus providing consistency for the duration of the synthetic control analysis (WHO, n.d.). The data was available from the years 1960-2015, but some countries had missing data for

certain time periods. Although suicide rate data was available up until 2015, the missing data points restricted the use of data up until the year 2014 because of a conflict with the synthetic control model; this limitation is explained further in section 4.1. There is no strict requirement for how many years of data should be used for SCM, but Abadie, Diamond, & Hainmueller (2015) state that more time periods are preferable. Suicide rates were reported per 100,000 population.

3.1.1 Predictor variables. I included predictor variables that were strongly correlated with suicide rates (Abadie, Diamond, & Hainmueller, 2010). These variables were derived from the literature.

Andriessen, Krysinska, and Lester (2015) looked through time series data of the 10 Belgian provinces to find suicide correlates for Belgium. Unemployment and divorce rates were found to be positively correlated with suicide, while population density was negatively correlated. Bernal et al. (2007) examined the risk factors for suicidality in the general population of Europe. They also found that divorce was significantly correlated with suicide, along with alcohol consumption, physician density, and healthcare expenditure. Maag (2008) reviewed numerous studies on the topic which reaffirmed these correlates and also found that life expectancy and higher education were correlated with suicide. We included predictors that were obtained from either the OECD or from Eurostats, the European Union's official database of statistics. These predictor variables are defined as follows:

- Crude divorce rate, per 1,000 population (Eurostats, 2018a). The data was collected by national statistics institutions and is based on official records.

- Highest education attained as a percentage of population aged 15-64 (Eurostats, 2018b). Education levels were split into three categories: 0-2, 3-4, and 5-8. The definition of education levels is based on the International Standard Classification of Education. In this system, each number represents a level of education as follows:
    - Level 0 – Less than primary education
    - Level 1 – Primary education
    - Level 2 – Lower secondary education
    - Level 3 – Upper secondary education
    - Level 4 – Post-secondary non-tertiary education
    - Level 5 – Short-cycle tertiary education
    - Level 6 – Bachelor's or equivalent level
    - Level 7 – Master's or equivalent level
    - Level 8 – Doctoral or equivalent level

  Data on educational attainment was taken from a quarterly labour force sample survey. The surveys were defined and processed by Eurostats and conducted by national statistics institutions.
- Population density per km^2 (Eurostats, 2018c).
- Unemployment as a percentage of the total population (Eurostats, 2018d). Data was taken from a quarterly labour force sample survey. The surveys were defined and processed by Eurostats and conducted by national statistics institutions.
- Alcohol consumption in liters per capita (OECD, 2018a). Data on consumption was reported by each nation and is based on alcohol sales.

- Medical doctors per 1,000 population (OECD, 2018b).
- Total health expenditure as a percentage of gross domestic product (OECD, 2018c). Data was compiled by national statistics institutes and taken from organizational financial reports.
- Life expectancy at birth in years (OECD, 2018d).

For the most part, the existing literature showed that economic factors were more weakly correlated with suicide rates than sociodemographic factors. It should also be noted that the variables discussed in this paper are not an exhaustive list of correlates, but instead are variables for which sufficient data exists for inclusion in this study. Factors such as mental health expenditure and recent mental illness are very predictive of the suicide rate, but data on these variables was often unavailable and thus excluded from the study.

3.2 Country Selection

Switzerland, Luxembourg, Netherlands, and Belgium are the countries of primary interest. However, there are certain criteria that must be met in order to be eligible for inclusion in the synthetic control analysis. Switzerland's assisted dying policy is the oldest and took effect before the year 1960, which is the earliest year for which suicide rate data is available (Hurst & Mauron, 2003). Consequently, a pre-intervention analysis was not possible and so Switzerland was excluded. In contrast, Luxembourg has the most recent assisted dying policy of the four nations. Luxembourg drafted its assisted dying policy in 2008, and it came into effect in 2009 (Nicol & Tiedemann, 2015). This experiment was only able to make use of suicide rate data up until 2014, leaving just five useable post-intervention data points. For this reason, Luxembourg was not suitable for analysis.

The move to legalize assisted dying in the Netherlands was a very drawn out process, and assisted deaths were provided for over 30 years prior to the legislation. Rietjens, van der Maas, Onwuteaka-Philipsen, van Delden, & van der Heide (2009) found that 1.7% of all deaths were due to euthanasia in 1990, and it made up 1.7% of all deaths in 2005 as well, three years after legalization. These figures illustrate that the Netherlands was not suitable for study because the time between pre- and post-intervention periods was very long and imprecise. The legislation of the assisted dying policy did not serve as a point that caused adoption of assisted dying procedures due to the Netherland's existing history with the practice.

Like the Netherlands, assisted dying legislation came into effect in 2002 in Belgium (Nicole & Tiedemann, 2015). However, Belgium's case was different in that there was no gray area to conduct the practice prior to the legislation. The year 2002 represents a point that separated assisted dying into well-defined before and after periods, making Belgium a suitable candidate for time series analysis (Vermeersch, 2002). The policy intervention year of 2002 also allowed numerous pre- and post-intervention time points.

3.2.1 Donor countries. The donor countries had two purposes: first, the donor countries helped construct the synthetic control of the primary intervention group (i.e., a synthetic Belgium); this was done by analyzing data on suicide rates as well as the predictor variables for each donor country (Abadie et al., 2010). Second, the donor countries were used as placebo groups to perform placebo testing that helped ensure validity of the model and to determine statistical significance of the findings (Abadie et al., 2010). This process is detailed in section 3.3.1. The donor countries were required to

be characteristically similar to the intervention country in order to minimize differences across unmeasured confounders, but they should not have had the same intervention (Abadie et al. 2010).

In order to utilize donor countries that were characteristically similar to Belgium, all other European nations with OECD data were included, with the exception of Switzerland and the Netherlands because they had already implemented assisted dying policies. Choosing European nations also increased the likelihood that exogenous factors that affected suicide rates were shared between Belgium and the donor countries. Luxembourg still served as a donor country because its policy was not introduced until much later. This narrowed the list of eligible donor countries to the following: Denmark, Czech Republic (CR), United Kingdom (UK), Ireland, France, Spain, Italy, Germany, Austria, Norway, Hungary, Poland, Slovakia, Sweden, Estonia, Finland, Greece, Iceland, Lithuania, Latvia, Portugal, Luxembourg, and Slovenia.

3.3 The Synthetic Control Method

SCM is a technique first introduced by Abadie & Gardeazabal (2003) and was later refined by Abadie et al. (2010) with packages to support the use of SCM in various statistical software. SCM is a data driven procedure that produced an artificial control group, called the synthetic control, whose ultimate purpose was to represent a counterfactual of Belgium (the intervention group); in other words, the synthetic Belgium estimates what Belgium's suicide rates would have looked like if the policy intervention in 2002 did not take place. If Belgium and the synthetic Belgium significantly diverged after 2002, the interpretation was that the policy caused a significant change in suicide rates. If Belgium and the synthetic Belgium both appeared to be approximately the same

after 2002, then the interpretation was that the policy did not have a meaningful effect on suicide rates.

SCM works with software that creates a synthetic control by analyzing data from Belgium's suicide rates and predictor variables, and also the donor countries' suicide rates and predictor variables. The software then attempts to find the best combination of this data, by assigning weights, that most accurately resembled Belgium's suicide rates in the pre-intervention period only. The software then simulates the value of each data point in the post-intervention years by using the established combination of data and weights. Because the synthetic control only fits itself to pre-intervention data, it is void of any potential influence brought on by the policy change in the post-intervention periods. This effectively gives us an estimation of what Belgium would have looked like in the post-intervention years had the policy change not occurred. The validity of this counterfactual is highly dependent on achieving a good fit between Belgium and the synthetic Belgium in the pre-intervention period. The accuracy of the fit can be determined qualitatively with a plot, or quantitatively through a calculation of the mean squared prediction error (MSPE), where a smaller MSPE signifies a better fit (Abadie et al., 2010).

After conducting a review of studies that used SCM along with other time series methods, Bouttell, Craig, Lewsey, Robinson, and Popham (2018) determine that SCM performed at least as well as robust methods that have been traditionally used. The technique protects against a number of threats to internal validity, such as maturation, statistical regression, and history (only when the intervention country and donor countries are equally affected). The method is also capable of accounting for unobserved (only when intervention and placebo units are equally affected) and observed time-varying

confounders (Bouttell et al., 2018). This method was also chosen because it negated the necessity of the “parallel trends” assumption required for matching intervention groups with controls in methods such as difference-in-differences analysis (Bouttell et al., 2018). A suitable control that satisfied the parallel trends assumption was not available to pair with Belgium.

3.3.1 Placebo and inference testing. Placebo tests are generally performed to help ensure the validity of a testing procedure. A placebo is an intervention that should not produce a positive test result, and thus can be used to determine if the testing method produces false positives. Placebos are also used to establish reference values to help determine if the real intervention had a meaningful effect on the outcome variable, and what the magnitude of that effect is. Abadie et al. (2010) described techniques for placebo testing with the SCM approach to fulfill both of these roles. This study used a procedure where SCM was applied one-by-one to each donor country using parameters identical to those initially used for Belgium. An SCM trial that shows suicide rates of a country and its synthetic control diverging after the policy change implies that the policy change had an effect. Because the donor countries did not implement the policy change, there should be no significant difference between the donor countries and their synthetic control. If type I errors do not appear in the placebo trials, it gives confidence that the statistical model is valid in its assessment of whether the policy change had an effect. On the other hand, the presence of type I errors would render the model as dubious.

Upon completing the placebo trials, one way to detect type I errors was to visually compare the plot of Belgium, along with its synthetic control, against the plots produced by the donor countries as placebos. The entire plots can be compared against each other,

but a “gaps” plot was produced in this study which shows the deviation of a country’s suicide rates from its synthetic control’s suicide rates, where the synthetic control’s value is set to equal zero. Gaps plots were produced for Belgium and the donor countries and were presented in a figure that showed them overlaid on each other. For this stage of placebo testing, Abadie et al. (2010) recommended including only the results of placebo trials with pre-intervention MSPEs similar to those of the primary intervention group, which they defined as being no more than twice the MSPE of the primary intervention group.

SCM is largely a visual method and thus does not rely on traditional significance testing, but it is still possible to establish quantitative reference values from the placebo trials and compare them with Belgium’s trial to determine if the policy change had a significant affect on suicide rates (Abadie et al., 2015). This process involved calculating the post-intervention MSPE between the real and synthetic values of each country’s suicide rates, and then dividing it by the respective pre-intervention MSPE to generate post-intervention to pre-intervention MSPE ratios (Abadie et al., 2015). In this stage of placebo testing, a larger MSPE ratio indicates that the real and synthetic values diverged by a larger magnitude. The MSPE ratios of the donor countries were used to establish reference values that the MSPE ratio of Belgium was compared against. A pseudo p-value was then calculated to determine whether Belgium’s MSPE ratio was significantly greater than the ratios of the donors. If $p < 0.05$, then Belgium’s MSPE ratio would have been significantly larger than that of the donors, indicating that the policy change affected Belgium’s suicide rates. Comparing MSPE ratios only tells us if the magnitude of change was significant and does not tell us whether the policy change caused suicide rates to

increase or decrease. A plot of Belgium and its synthetic control would need to be viewed to determine the direction of this relationship. A bar graph was produced to compare the MSPE ratio of each country.

3.3.2 The 2008 Recession. Trends of suicide rates show that numerous European countries experienced increases in suicide rates after 2007, even when a downwards trend had been established for many years prior (OECD, 2018e). The sudden increases in suicides and analyses of the event suggest that the recession played a large role in raising suicide rates (Chang, Stuckler, Yip, & Gunnell, 2013). Though SCM is a very capable design, it struggles with exogenous factors that affected the countries included in this study differently. Evidence in the form of financial data indicates that the 2008 Recession is exactly one such exogenous factor, and a very significant one at that (Emmerson, 2015). Consequently, a sensitivity analysis was performed to control for the recession by truncating the analysis at 2007, just prior to the start of the recession.

3.4 Materials

R was used to carry out the analysis. The package *Synth* was installed to access the required libraries. The command *install.packages("Synth")* was used to install this package (Abadie, Diamond, & Hainmueller, 2011). The code used for this analysis can be found in appendix A2.

3.4.1 Commands for SCM. There were two *R* commands used to prepare data for the synthetic control that will be referred to in the *Results* section of this thesis The command *time.predictors.prior* described the predictor variables in the pre-intervention years that were analyzed to create the synthetic control. The data analyzed by this command, and the years for which it was analyzed, affect the pre-intervention fit. The

objective was to use the most time periods possible while minimizing MSPE (Abadie et al., 2010). The command *time.optimize.ssr* describes the pre-intervention years that were optimized when fitting the intervention group and the synthetic control to each other. Using more time periods was again preferable (Abadie et al., 2010).

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Chapter 4. Results

Figure 4.1 shows suicide rates plotted against time in Belgium from the years 1984-2014. The solid line displays Belgium's suicide rates for each calendar year. The vertical segmented line depicts the year of the policy change. Overall, suicide rates in Belgium fell from 24.6 in 1984 to 19.5 in 2002. Suicide rates continued to fall to 16.1 in 2014.

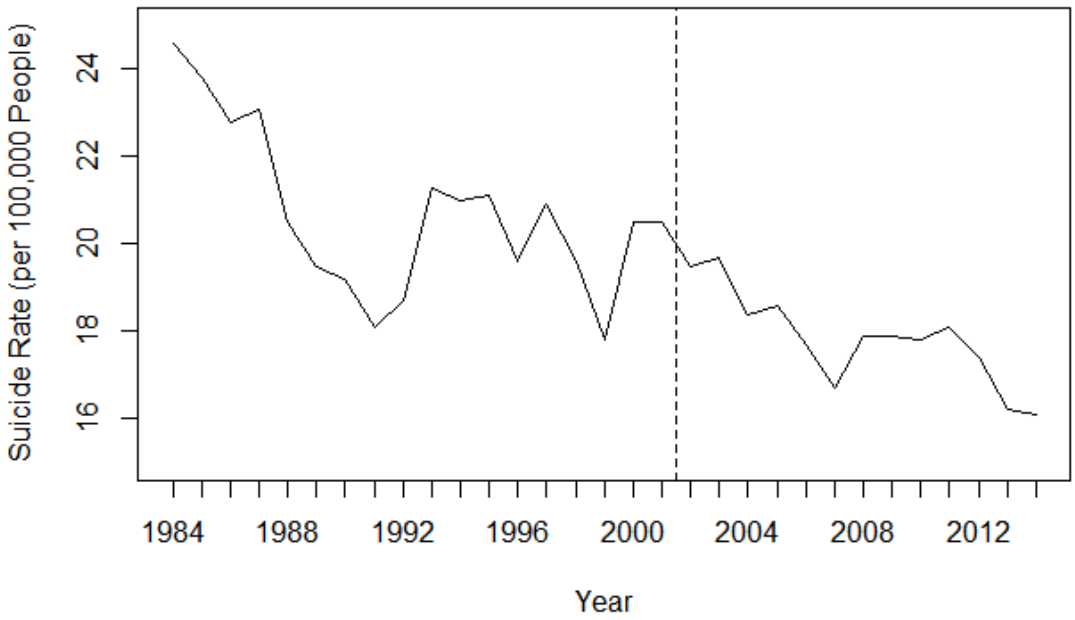

Figure 4.1: Belgium's suicide rates from 1984-2014. The intervention is labelled between the years 2001 and 2002 by the vertical dashed line.

4.1 Synthetic Control Analysis

The implementation of the SCM analysis began with data preparation. Details about the following commands can be found in section 3.2.1. The command *time.predictors.prior* was given the value 1980-2001; 2001 was chosen because it is the last year before the intervention, and 1980 because it is a value that includes the greatest number of pre-intervention years while also yielding a pre-intervention MSPE smaller than any of the years prior to it. The command *time.optimize.ssr* was given the value 1990-2001; 2001 again represents the last year before the intervention, and 1990 was

selected because missing dependent variable data for Germany prevented a time point prior to 1990 from being selected.

Three donor countries were removed from the analysis because of missing dependent variable data. Poland was missing data in 1997 and 1998, Slovakia was missing data in 2006 and 2007, and Portugal was missing data from 2003-2005.

Three predictor variables were removed from the final analysis because no data points exist in the pre-intervention years for one or more donor countries. *Medical doctors* was removed because data is missing for Ireland and Greece; *unemployment* was removed because data was missing for Iceland, Latvia, and Lithuania; and *life expectancy* was removed because data was missing for Latvia. The donor countries with the missing data could have been removed instead of removing *medical doctors* and *unemployment*, but it was found that including the donor countries was better able to minimize MSPE (sensitivity analyses of the alternatives are included in appendix A1). Removing Latvia in order to keep *life expectancy* reduced the MSPE, but by a very small amount; the resulting MSPE would be 0.65, just 0.01 less than the final MSPE achieved otherwise. Keeping Latvia was preferred so as to increase the statistical power of our significance tests.

With these variables and paramters established, the *synth* command returned the most optimized MSPE of 0.66, indicating that a very good fit was achieved based on the the MSPE obtained in Abadie et al. (2010). Figure 4.2 shows the resulting plot of the synthetic Belgium with the real Belgium laid on top of it. The range of the plot begins at 1990 because of missing outcome variable data from Germany prior to that, and ends at 2014 because of missing outcome variable data in several placebo units beyond this time

point. Despite these limits, there is more than enough time charted to demonstrate the progression of the synthetic model. From Figure 4.2, we can see that there is a separation between the synthetic and real Belgiums at 2001, just before the policy change occurs; this was likely caused by difficulty modelling the pre-intervention data due to the large dip at 1999. Despite that, the two plots either got closer or maintained this approximate distance until 2006; this can be taken as evidence of the policy change having no effect during this time period. Even though the plots diverge a bit after 2006, they still manage to maintain a close distance and even converge again towards the end of the figure. There are two time points on the graph where the synthetic control and intervention group intersected each other: 2004 and 2013. These intersections further indicate that the real and synthetic Belgiums did not go on significantly different paths following the policy change. The time period during which the two plots showed a greater divergence also

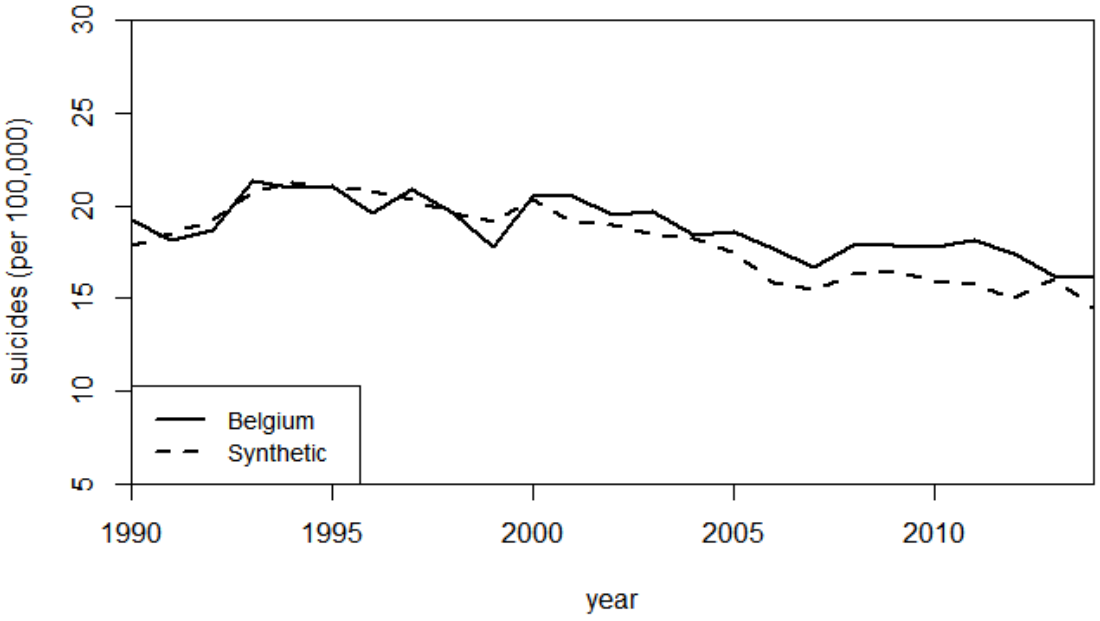

Figure 4.2: Plot of Belgium's suicide rates from 1990-2014 laid over its synthetic control.

coincides with the 2008 Recession, a very important event that, as discussed in chapter 3, can cause some modelling difficulty.

Table 4.1 quantifies the difference between the suicide rates of the real and synthetic Belgiums for each time point in the plot. Aside from the year 1990, the largest discrepancy between the two Belgiums for the pre-intervention period was in 1999, an indication of how the dip at that time point skewed the synthetic model. While not significant from a policy standpoint, it does show that there is a degree of error in the model that is reflected in the resulting pre-intervention MSPE.

Year	1990	1991	1992	1993	1994	1995	1996	1997	1998
Value	1.35	-0.38	-0.34	0.85	-0.09	0.21	-1.26	0.49	-0.07
Year	1999	2000	2001	2002	2003	2004	2005	2006	2007
Value	-1.32	-0.02	1.20	0.59	1.33	0.16	1.06	1.91	1.21
Year	2008	2009	2010	2011	2012	2013	2014		
Value	1.51	1.37	1.74	2.33	2.30	-0.04	1.47		

Table 4.1: Differences between the suicide rates of the synthetic and real Belgiums at each time point in the plot.

	Divorce	Alcohol	Health Expenditure	Density	Education 0-2	Education 3-4	Education 5-8
Belgium	2.20	12.01	7.29	334.25	45.17	33.14	21.67
Synthetic	2.11	11.61	7.25	99.34	41.27	38.01	20.72
Placebos	2.18	10.65	6.97	94.43	35.28	47.04	17.68

Table 4.2: The mean values for each predictor variable in the pre-intervention time period. The first row of values represents the real Belgium, the second row represents the synthetic Belgium, and the third row represents the average of all placebo units.

Table 4.2 shows the average value of the predictor variables for each pre-intervention time point. The table shows us that the analysis was able to create a synthetic Belgium that resembles the real Belgium fairly well for most of the predictor variables. This was aided by the fact that the means of the placebos also resemble Belgium quite well, a requirement for the synthetic analysis to be valid (Abadie et al., 2010). The one variable that did not capture Belgium at all was *population density*. However, a sensitivity analysis that involved running the *synth* command with this variable removed does not alter the pre-intervention fit, as the produced MSPE is still 0.66. An analysis of the relative contribution each predictor variable made to the construction of the synthetic Belgium is presented in Table 4.3. It is shown that the model did not utilize *population density* at all to construct the synthetic Belgium, explaining why its removal made no difference. Regardless, *population density* was kept in the model because it can still be used to make synthetic controls of the donor countries.

	Divorce	Alcohol	Health Expenditure	Density	Education 0-2	Education 3-4	Education 5-8
Relative Contrib-ution	0.017	0.095	0.314	0.000	0.100	0.083	0.390

Table 4.3: The weight of each predictor variable when used by the model to construct the synthetic control. *Density* made no contribution while *Education 5-8* was given the greatest weight. Note that predictors with synthetic values close to the real value, depicted in Table 4.2, do not necessarily have greater weights when creating the synthetic control.

4.2 Placebo Trials

The next phase of the SCM analysis was the placebo trials. The donor countries removed in section 4.1 (Poland, Slovaki, and Portugal) were not included in this analysis either. In order to eliminate donor countries with a pre-intervenion MSPE twice that of Belgium's

Figure 4.3: This gaps plot depicts the difference between each country's real and synthetic suicide rate. The horizontal dashed line represents the synthetic control value, which has been set to equal zero, while the solid lines represent the deviation of each country's real suicide rate from their respective synthetic controls. The vertical dashed line on the left is set between 2001 and 2002 when the policy change ocurred. The vertical dashed line on the right shows the year 2007 to aid with the sensitivity analysis

(MSPE = 0.66), donors that had MSPEs greater than 1.32 in the placebo trials were excluded. This criterion means that France, Italy, UK, Ireland, Sweden, and Austria were included in the placebo trials while Germany, Spain, Czech Repiublic, Norway, Denmark, Finland, Luxembourg, Slovenia, Greece, Hungary, Lithuania, Latvia, Iceland, and Estonia were not. Results of the placebo trials can be found in Figure 4.3 in the form of a gaps plot. None of the donor countries showed significant divergence from each other and most of them converged to a value of zero towards the end of the plot,

providing evidence that the model has avoided type I errors. Belgium's line was well within the limits of those that represent the donors, suggesting that the difference between the real Belgium and its synthetic control's suicide rates was not meaningful.

4.3 Significance Tests

Figure 4.4 depicts the post-intervention to pre-intervention MSPE ratios of the 21 countries, including Belgium. A higher value indicates that the suicide rates of the real country and the synthetic country are very different in the post-intervention period but were not so different in the pre-intervention period; therefore, a larger MSPE ratio implies that the policy change had a larger effect on the suicide rates of the analyzed group. The median value was 1.04 (Iceland), the mean was 3.07, and Belgium's MSPE ratio was 3.34. The calculated pseudo p-value for Belgium was 0.24, indicating that the MSPE ratio was not statistically significant. In other words, the policy change had no meaningful effect on Belgium's suicide rates, considering that the donors show a statistically similar change.

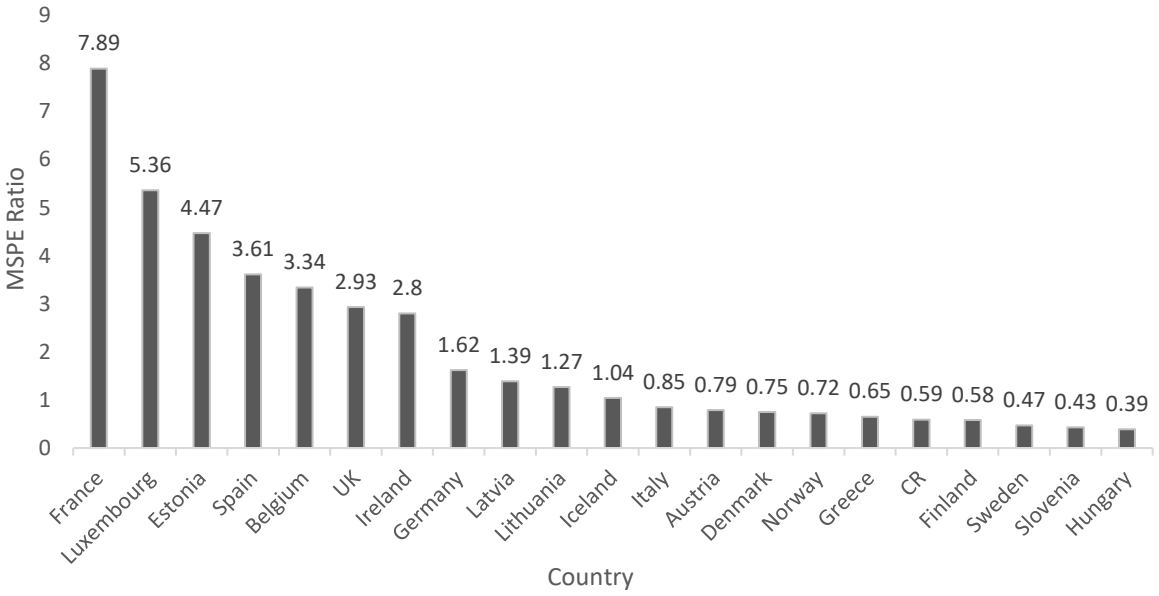

Figure 4.4: The post-intervention to pre-intervention MSPE ratios of each country in the synthetic analysis trials. The precise value of the ratio is labelled at the top of each bar.

4.4 Controlling for the 2008 Recession

To control for the recession, the plot depicted in Figure 4.2 and the placebo trials depicted in Figure 4.3 would be the same, but with data points cut off after 2007. To avoid redundancy, these plots were not reproduced for this sensitivity analysis, but a label was included in Figure 4.3 due to the numerous lines. The same cannot be said for the MSPE ratios, and so a sensitivity analysis was performed to ensure that the recession had not meaningfully altered the results. Figure 4.5 shows the results of this analysis. In this trial, the median value was 0.86 (Ireland) and the mean was 1.64. Belgium's MSPE ratio was 2.13, again having the 5th largest MSPE ratio and a pseudo p-value of 0.24, and thus, it was not statistically significant. Even when controlling for the 2008 Recession, the policy change did not have a meaningful impact on suicide rates given that the placebo trials showed similar MSPE ratios.

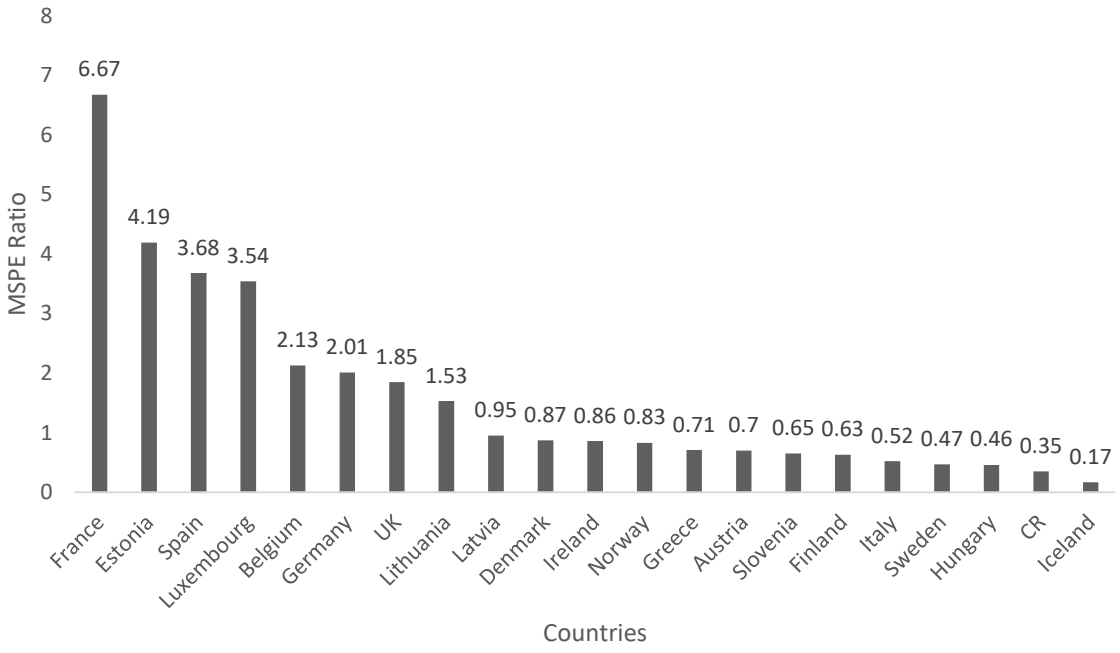

Figure 4.5: The post-intervention to pre-intervention MSPE ratios of each country when controlling for the 2008 Recession. The precise value of the ratio is labelled at the top of each bar.

Change	-1	0	-3	+4	+1	+2	0
Country	Austria	Belgium	CR	Denmark	Estonia	Finalnd	France
Change	+2	+3	+2	-10	-4	-5	+5
Country	Germany	Greece	Hungary	Iceland	Ireland	Italy	Latvia
Change	+2	-2	+3	+5	+1	+1	-1
Country	Lithuania	Luxembourg	Norway	Slovenia	Spain	Sweden	UK

Table 4.4: This table shows how each country change position amongst the entire pool of countries when comparing the second set of MSPE ratios to the first. A positive (+) notation indicates that the country has a larger MSPE ratio than other countries, relative to its position in the first trial. A negative (-) notation indicates that a country has dropped in the ranks and has a smaller MSPE ratio than the other countries.

While the order of countries in this analysis was different compared to the order presented in Figure 4.4, there was a general consistency between the two results. Table 4.4 shows how the position of each country changed when going from the first trial of MSPE ratios to the second. Almost all countries had a change of five or less (five is approximately 1/4 of the total units), with just one country moving by a greater number. Two countries did not change position, and over half changed by no more than two positions. This consistency shows that synthetic model performed well, despite the challenge presented by the recession. In the next chapter, I discuss how the statistical model performed and its consistency in more detail.

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Chapter 5. Discussion and Conclusion

The time range of analysis was limited to 1990-2014, or 25 years, but this was sufficient to understand how the synthetic Belgium differed from the real Belgium. Boutell, Craig, Lewsey, Robinson, and Popham (2018) state that there is currently no reference point to determine what a good pre-intervention fit is, but Abadie, Diamond, and Hainmueller (2010) achieve a pre-intervention MSPE of approximately 3.0 in their analysis and consider it to be a good fit. Using this as a benchmark, the 0.66 MSPE achieved in this study is an excellent result. The pre-intervention fit is crucial for validity, so this result brings confidence to the findings.

Even before the policy change took effect, the synthetic and real Belgiums were slightly separated from each other. The two models were very similar in the year 2000, but separated from each other in 2001. This sudden change in level just prior to the onset of the intervention is a known occurrence when performing time-series analyses (Wagner, Soumerai, Zhang, & Ross-Degnan, 2002). The effect is attributed to the "anticipatory demand" that occurs when the public suspects or knows that a policy change is coming. As the public shifts their behaviour in anticipation of the policy change, the outcome variable will suddenly increase or decrease (Wagner et al., 2002). This effect is dependent on whether the policy has an association with the outcome variable, but if so, the variable will change before the policy change is actually legislated or takes effect. I determine that this explanation is not suited to this case because the real suicide rate in 2000 and 2001 is the same, meaning the anticipatory demand effect could not have occurred – it is the synthetic suicide rate that experiences the change in the level. I conclude that this discrepancy is not evidence of the policy change causing a suicide

contagion via anticipation effect. The discrepancy is likely due to a small degree of error in the model caused by the wild data point in the year 1999. SCM models the outcome variable over many time periods to determine long-term trends, and the study also achieved an overall strong pre-intervention fit, so this error ultimately does not complicate the findings.

Even though the synthetic model was slightly skewed going into the post-intervention period, the main SCM output (Figure 4.2) shows that the synthetic control was not all that different from the real Belgium because they remain nearly parallel for the duration of the analysis. This is most obvious from 2002-2005, where the difference between the two Belgiums is always smaller than the difference at 2001, a pre-intervention year. This gap becomes larger at 2006, and then even larger at two time points during the recession period. In an ideal scenario, any impact the recession had on suicide rates in Belgium would have equally happened in the other European countries. Because European countries are being used as donors, this ideal scenario would mitigate confounding caused by the recession. Reality is different however, and each country experienced the recession in different ways. This asymmetry means that confounding is not entirely controlled for by the SCM, thus causing this time period to be a modelling challenge. Despite this, the synthetic and real Belgiums do not wildly differ, with the greatest separation occurring in 2011 by a value of 2.33. Instead of seeing a consistent trend of Belgium and the control diverging from each other over time, the two plots instead converged and intersected each other in 2013. Because the two plots remain very close to each other, are nearly parallel, and fail to show divergence, I interpret the

synthetic control as evidence that the assisted dying policy change did not affect Belgium's suicide rates.

When again looking at the main output but cutting off values after 2007, I effectively performed a sensitivity analysis that controlled for the recession. As mentioned above, the period from 2002-2005 shows that the synthetic and real Belgiums stay very close to each other, and the separation eventually becomes larger in 2006, but not in 2007. Given that there are only have six time points to examine in the sensitivity analysis, it is difficult to say that the slight divergence shown is evidence of the synthetic and real Belgiums trending apart from each other. With so few time points, the divergence would need to be more substantial to consider this as evidence of the policy change having had an effect on suicide rates. Ergo, I conclude that the sensitivity analysis of the main output which controlled for the 2008 Recession does not show evidence that the policy change had an effect on Belgium's suicide rates, but confidence in this result is reduced because of the limited time points available to analyze.

In total, seven countries had placebo trials conducted and were included in the gaps plot (Figure 4.3). The gaps plot has the synthetic model set to equal zero, so the lines shown belong to the real countries and how the real suicide rates deviate from the synthetic suicide rates at each time point. None of the countries show a deviation greater than three. Most countries show a similar pattern of diverging and reaching their greatest degree of separation from the synthetic model in the years associated with the recession. After reaching a peak, the synthetic and real models converge towards the end of the final years of the figure. I interpret the placebo analysis as not showing type I errors because the real and synthetic models show no obvious sign of diverging from each other. The

placebo trials also provide evidence that Belgium's suicide rates were not significantly altered by the policy change because the deviation from its synthetic control is very similar to the deviation shown by the donor countries. The sensitivity analysis involving the gaps plot is less clear in its interpretation. The discrepancy between Belgium and its synthetic control is nearly the same in 2001, 2003, and 2007, giving the plot an appearance that is almost parallel to the synthetic control. I interpret this as evidence that the policy change did not affect suicide rates. However, it is not so clear as to whether one of the placebo trials resulted in a type I error. The donor country at the very top appears to be significantly diverging from its synthetic control. This trend eventually reverses after 2008, but it is difficult to state that this would have also happened in the absence of the recession.

The first two outputs in this study are very dependent on visuals to present and interpret the results. This demands some degree of subjectivity. In contrast, the final test involving post-intervention to pre-intervention MSPE ratios is entirely quantitative and is specifically meant to help determine if the effect the policy change had on Belgium's suicide rates was significant. Belgium's MSPE ratio was the 5th largest, which is well within the placebo ranges established by the donor countries. As per the explanation provided by Abadie, Diamond, and Hainmueller (2015), if one were to choose a country at random in this analysis, there is a 24% chance of picking a country with an MSPE ratio equal to or greater than Belgium's. This high probability shows that Belgium's MSPE ratio is not a statistical anomaly. This test was performed again to control for the recession and the result was the same.

The three outputs produced a consistent result that suggests suicides rates were not meaningfully altered by the intervention. The same is true for the three sensitivity analyses, though the analysis involving the placebo trials is less clear. I conclude that Belgium's assisted dying policy did not have an impact on the nation's suicide rates. This conclusion is the same that Jones and Paton (2015) ultimately reached in their study for Oregon and Washington. Because suicide rates were used as a way to estimate the presence of a suicide contagion, I also conclude that this study does not show evidence that a suicide contagion occurred.

5.1 Strengths and Limitations

The use of the SCM is a major strength of this study. This procedure is a powerful study design because of its data driven nature and because it is essentially a form of machine learning. The ability to account for observable and unobservable confounders while also protecting against a range of other validity threats provides ample reason to be confident in the results. The small pre-intervention MSPE, the similarity between Belgium and the synthetic control in terms of predictor variables, and the lack of type I errors further reinforce validity. Another strength of the study is that Belgium was an ideal subject for this investigation. In addition to meeting all of the inclusion criteria, Belgium is considered to have the world's most permissive assisted dying policy (PBS News Hour, 2015; Nicol & Tiedemann, 2015); this trait makes Belgium the most likely jurisdiction to experience a suicide contagion, and therefore the best jurisdiction for detecting this phenomenon. If Belgium with its extreme assisted dying policy did not experience a suicide contagion, then it is all the more likely that other jurisdictions have

also not experienced it, and future jurisdictions that legislate assisted dying will not either.

The data driven nature of this study is certainly one of its strengths, but there are several data quality issues that make it a weakness as well. Certain variables highly predictive of the suicide rate, such as mental health funding and serious mental illness in the past year, were typically not available for Belgium's pre-intervention years and were thus excluded from the study. Additionally, some predictors which were at first included in the model later had to be removed because of missing data for placebo units. Overall, the missing data limited the number of confounders that were directly controlled for. Even though the donor countries allow unmeasured confounders to be controlled for, confounders that asymmetrically affected the included countries could not be entirely controlled. The models were also limited by missing suicide rate data in Germany, which prevented optimisation and plotting of time points prior to 1990. Even if Germany was excluded from the study, several other nations also had missing data in the 80's; for each nation removed, only two or three additional years would be gained for optimisation, thus making this a vain effort with little positive effect. The number of donor groups available for use in this study is another data limitation. Three nations had to be excluded because of missing dependent variable data, but there are many more European nations that were never considered because data was not available in the OECD database. Increasing the number of donor groups would have led to a more powerful significance test by providing more opportunity to understand what sort of results should be expected in regions that did not have the policy intervention. Finally, the fact that only Belgium could be tested as a treated group represents a significant sample size limitation. While

Belgium's permissive assisted dying policy helps to generalize the findings, the sample size limitation hinders it greatly. This limitation exemplifies the overall difficulty in studying the relationship between suicide rates and assisted dying, a conclusion that Jones and Paton came to as well.

5.2 Possible Explanations for the Results

The hypothesis of a relationship between assisted dying and suicide contagion is intuitively based on what is known about suicide contagion when it occurs in other scenarios, such as with celebrity suicides. Considering this, what are some possible explanations for why suicide rates were not found to have been altered in this study? The first explanation to explore is that the hypothesis is fundamentally incorrect because of important distinctions between assisted dying and suicide. For instance, Francis (2017) points out that suicide contagion often involves victims replicating the suicide method used by the contagion's source. Assisted deaths are wrapped in medical procedures and often involve planning and the presence of family and friends. This stands in contrast to suicide which must be carried out in isolation, is dissuaded by healthcare practitioners, and generally retains a stigma. The exact method of death cannot be recreated in suicide, and the optics of the entire process is different as well. Belgian law also requires that mental disorders must be treatment resistant to be considered for an assisted death. This process can involve years of consultations to exhaust therapy and pharmaceutical options before treatment resistance is reached (Boer, 2017). These distinctions may cause a disconnect between suicidal individuals and the situations that assisted dying patients find themselves in, thus interfering with their ability to identify with the concept of assisted dying.

Further building on the possibility that the hypothesis is fundamentally invalid, there may also be a disconnect between individuals susceptible to a suicide contagion and the concept of assisted dying because of age. Some research studies have suggested that adolescents are the most susceptible to a suicide contagion (Cheng, Li, Silenzio, & Caine, 2014). On the other hand, assisted dying is only relevant to older age groups because of direct age restrictions and because the diseases which are eligible naturally present themselves in older age groups. Until a jurisdiction and the society therein accepts assisted dying for all adolescents, a suicide contagion is not expected to be seen while working with this explanation.

Another explanation to consider is that contagion victims are not attempting suicide but are instead motivated to receive an assisted death themselves. If this is the case, we might expect to see a decrease in the number of suicides. If this were the case, the contagion may not be detectable in the suicide rate, but there may be evidence of it in assisted dying statistics. However, this is unlikely to be the case since the number of assisted deaths is small relative to the number of suicides. For instance, assisted deaths for psychiatric disorders would make up just 1.6% of suicides in Belgium in 2012. Furthermore, there were zero assisted deaths based solely on psychiatric disorders in 2005 and 2007. Unfortunately, information on the number of applicants for medically assisted death is not collected. In addition, because this study analyzed population-level suicide rates, we observed only net effects. It is possible that some individuals experienced reduced suicide ideation after the implementation of assisted dying policy because an alternative option became available, while others experienced increased suicide ideation due to a suicide contagion effect. The net of these two effects may have

resulted in zero change. Thus, the results of this study suggest that assisted dying policy did not result in a meaningful increase in overall suicide rates at the population-level. However, this finding may not translate to the individual-level.

5.3 Policy Implications

The analysis presented above used time-series data to determine whether the introduction of assisted dying policies in Belgium generated a suicide contagion that increased suicide rates. The findings of this analysis suggest that there is no reason to believe a suicide contagion occurred at the population-level.

This conclusion is useful in terms of guiding policy formation. First, the four nations that presently allow assisted dying for mental disorders, which includes Belgium, have seen mixed reception from residents towards this policy (Council of Canadian Academies [CCA], 2018). Part of this concern is around the possibility of a suicide contagion. However, this study does not support these concerns.

Second, jurisdictions that are considering the implementation of an assisted dying policy or those who plan to expand the scope of their policies may benefit from these findings, which may assuage concerns about a possible suicide contagion effect.

To our knowledge, Canada is the only country actively looking to make significant modifications to its assisted dying policy. Recently, a panel of experts released a report that was intended to give the Canadian Parliament information about three possible ways to expand assisted dying, and allowing assisted dying for mental disorders is one of those possibilities (CCA, 2018). In their report, the expert panel raised a discussion on the association between legalizing assisted dying and general suicide rates. The expert panel reviewed the findings of Jones and Paton's study, but noted that

research on the association between assisted dying, where mental disorders are involved, and suicide rates does not exist. This knowledge gap brought forth by the expert panel is addressed through our study, and it may help inform the panel to confidently state that no reason currently exists to believe that expanding Canada's assisted dying policy will increase suicide rates. Canada is also reviewing the expansion of assisted dying for mature minors. The expert panel raised a concern that this policy would normalize suicide among youth (i.e. a suicide contagion), and that the risk is greater if the individual has a mental disorder. This knowledge gap is partially addressed by this study; I do not perform a detailed analysis of how different age groups have their suicide rates influenced by the policy change, but overall, the findings indicate that suicide is not normalized for mentally ill individuals in the general population when individuals in similar circumstances access assisted dying. More research is needed to confidently state how different age groups, including youth, have their suicide risk affected by assisted dying policies. Performing such a study with the synthetic control is ideal but unlikely to be feasible because of the large quantity of data required. Across many different variables, data on specific age groups is not collected or collection has begun very recently, thus requiring more time to pass before a valid study can be conducted.

Several American states have also dealt with the question of suicide contagion brought on by assisted dying policies. America has seen a much greater prevalence of this discussion relative to other nations. The argument that assisted dying can increase suicide rates has been disseminated through media and also through direct legislative testimonies despite there being no evidence yet to support the claim (Gordon Conwell Theological Seminary, 2017; Francis, 2017; Kheriaty, 2015). States that have already created their

policies and states that are currently exploring the option to legislate assisted dying have all seen this discussion appear at some point. Interestingly, the suicide contagion discourse seems to be more common in America even though it is home to the most restrictive assisted dying policies in the world. Whether this is actually true has not been studied, but if so, understanding why Americans have so fervently adopted this attitude may be worth looking into.

5.4 Conclusion

Using the synthetic control method, I was unable to find evidence that the legislative adoption of assisted dying affected suicide rates in Belgium. Suicide rates were used as an indirect measure of suicide contagion, and thus I do not find evidence of suicide contagion. The different tests performed as part of the synthetic control analysis were fairly consistent in determining this result. This conclusion is also similar to that of the sole previous study that examined this topic. However, this study is able to add to the literature by using the SCM and focusing on a jurisdiction that allowed assisted dying for mental disorders, as well as a broad range of terminal and chronic diseases.

Even though the assisted dying-suicide contagion hypothesis seems to make intuitive sense at first glance, the numerous claims that assisted dying can cause a suicide contagion have been made without supporting evidence. For stakeholders that may feel confused about these claims, especially if they were to feel morally complicit in associated suicides, this study brings much needed clarity.

Conducting further studies on this topic is presently a difficult challenge, and it would be especially difficult to construct one using valid methodology. However, with many jurisdictions having legalized assisted dying, and many more to come, the pool of

subjects available to study will increase. Time is a major obstacle to this, but studies that more directly measure suicide contagion at the individual level can be executed right now. What makes this such an appealing topic to follow up on is the reports of how individuals with suicidal ideation can experience a reduction in symptoms when they have access to assisted dying. There are many questions regarding this that can be addressed in future studies.

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Appendices

Appendix A.

A1. MSPE sensitivity analyses.

The best optimized synthetic control analysis returned an MSPE of 0.66. Using that as a baseline and then removing Latvia, Lithuania, and Iceland in order to introduce *unemployment* into the analysis results in an MSPE of 2.14. Returning back to baseline and then removing Greece and Ireland to introduce *medical doctors* to the analysis results in an MSPE of 0.68. Thus, removing the control variables to include more predictor variables is unfavorable for the statistical model.

A2. *R* code for the synthetic control analysis.

```
#open synthetic control library
library("Synth")

#resolves: "Error in dataprep(foo = data, predictors = c("divorce", "alcohol", "life", : unit.variable not found as numeric variable in foo."
data1 <- as.data.frame(data1)

#dataprep
dataprep.out <-
	dataprep(foo = data1,
		predictors = c("divorce", "alcohol", "expenditure", "density", "edu1", "edu2", "edu3") ,
		predictors.op = "mean" ,
		time.predictors.prior = 1980:2001,
		dependent = "suicide",
		unit.variable = "regionno",
		unit.names.variable = "regionname",
		time.variable = "year",
		treatment.identifier = 2,
		controls.identifier = c(1,3,4,5,6,7,8,9,10,11,12,13,14,15,16,17,21,23,24),
		time.optimize.ssr = 1990:2001,
		time.plot = 1990:2014
		)
```

```
#synth.out

 synth.out <- synth(data.prep.obj = dataprep.out,
                 method = "BFGS")


#dependent variable discrepancy between actual and synthetic, 1993-2007

 gaps <- dataprep.out$Y1plot - (dataprep.out$Y0plot %*% synth.out$solution.w)
 gaps[1:25, 1]


#produce tables

synth.tables <- synth.tab(dataprep.res = dataprep.out,
                      synth.res = synth.out
                      )


#pretreatment predictor values, treated vs synthetic vs mean

synth.tables$tab.pred[1:7, ]


#weight of each predictor

synth.tables$tab.v[1:7]


#weight of each control unit

synth.tables$tab.w[1]


#produce plot

 path.plot(synth.res = synth.out,
         dataprep.res = dataprep.out,
         Ylab = "suicides (per 100,000)",
         Xlab = "year",
         Ylim = c(5,30),
         Legend = c("Belgium","Synthetic "),
         Legend.position = "bottomleft"
         )

#produce gaps plot

 gaps.plot(synth.res = synth.out,
         dataprep.res = dataprep.out,
```

```
Ylab = "suicides (per 100,000)",
Xlab = "year",
Ylim = c(-5,5),
Main = NA
)
```