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Abortion and Crime in Canada: A Test of the BMDL Hypothesis

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A thesis submitted in partial fulfillment of the requirements for the Master of Arts degree in Sociology

Abortion and Crime in Canada: A Test of the BMDL Hypothesis

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by

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Abstract

Donohue and Levitt (2001) argued that the legalization of abortion in the US during the 1970s contributed to 50 percent of the dramatic decline in crime that occurred in the 1990s. Although a lengthy debate in the literature has proliferated and remains inconclusive, this controversial theory has been popularized by the *Freakonomics* (2005) franchise. The liberalization of abortion services that occurred in Canada in 1988 offers an improved focal intervention to perform an empirical test of this theory. The methods that have emerged from the debate are reviewed. The most promising strategies, namely time-series plots of crime, “effective abortion rate” analyses, and age-specific crime rate analyses, are employed. Using data from the *UCR2* and the *TAS*, this study finds no consistent relationship between abortion and crime rates in Canada. The theory that an increase in abortion rates is associated with declines in crime, therefore, must be regarded with serious skepticism.

Keywords: Canada, Freakonomics, hypothesis testing, legalized abortion, R v Morgentaler, youth

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Chapter 1

1 INTRODUCTION

Levitt and Dubner's book, *Freakonomics* (2005), has been enormously popular since its first publication in 2005. The focus of this thesis is one of their more controversial claims, which will be referred to as the Bouza-Morgentaler-Donohue-Levitt (BMDL) Hypothesis.[1] This hypothesis states that the legalization of abortion in the United States in the early 1970s contributed to nearly 50 percent of the enormous decline in crime in the 1990s. Levitt and Dubner (2005) explain that

> the women most likely to seek an abortion – poor, single, black or teenage mothers – were the very women whose children, if born, have been shown most likely to become criminals. But since those children *weren't* [emphasis as in original] born, crime began to decrease during the years they would have entered their criminal prime (218).

Although *Freakonomics* (2005) has presented this theory as fact, its original formulation in Donohue and Levitt's (2001) study has been met with much criticism and the validity of the theory remains unproven. Given the popularity of the *Freakonomics* franchise and the potentially far-reaching policy and legislative implications this theory could have, it is crucial that the claim be subjected to the scrutiny of further empirical testing. Not to do so, as Joyce (2010) notes, would border on "social science negligence" (453).

1.1 The BMDL Hypothesis

In the 1990s, the United States experienced the most extensive and prolonged decline in crime in recent American history (Figure 1.1). This decline in crime was particularly fascinating for several reasons. First, it was the longest decline in recorded history, spanning nearly a decade from its start in 1991 (Zimring 2007). It was a large decline; the overall crime rate declined 43 percent, the violent crime rate declined 33.5 percent, and the property crime rate declined 28.8 percent between 1991 and 2001. The decline in rates of crime was broad; declines were found in every major category of crime. The

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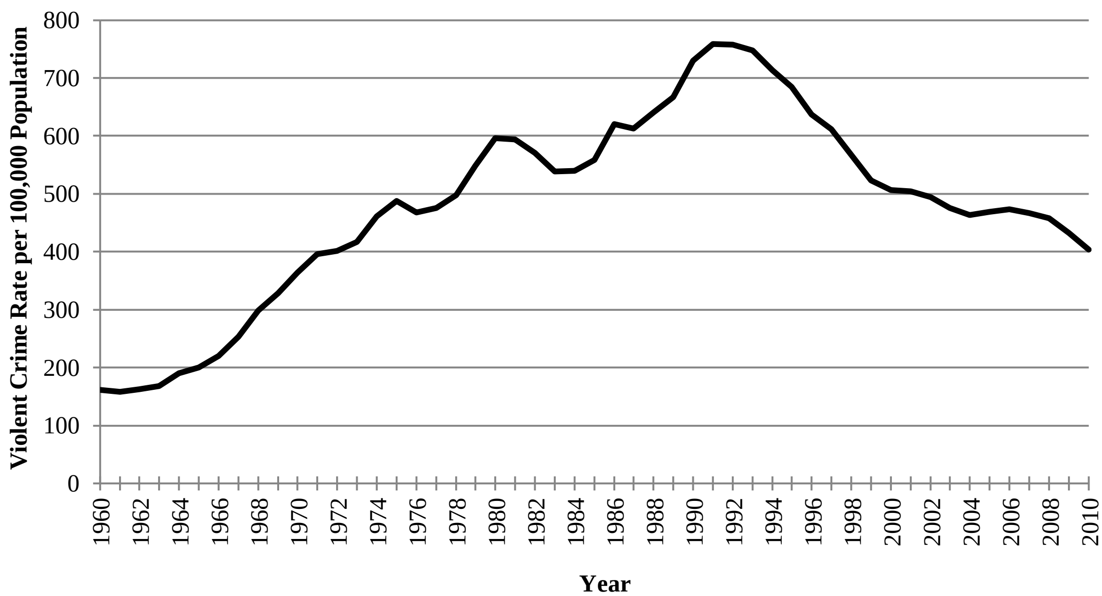
[1] The "Bouza-Morgentaler-Donohue-Levitt Hypothesis" was originally coined by Lott and Whitley (2007: 305).

decline in crime also spanned the entire United States (Zimring 2007). What made this decline in crime even more remarkable was the inability of experts to predict it. To the contrary, leading criminologists and scholars had forecast that crime would rise to "epidemic" proportions (Levitt 2004). Even in the mid-1990s, when crime rates had already began to fall, scholars continued to predict rises in crime rates to be seen well into the 2000s (Zimring 2007). Many of the "excessively punitive" criminal charging practices characteristic of mid-1990s America were fueled by these pessimistic predictions (Zimring 2007; Zimring, Hawkins, and Kamin 2001).

Figure 1.1: Violent Crime Rate, United States, 1960-2010



Source: FBI, Uniform Crime Reports, prepared by the National Archive of Criminal Justice

As the decline in crime became more apparent, criminologists, scholars, and policy makers provided an assortment of explanations. Levitt (2004) found that the most commonly cited explanations during the 1991-2001 period were changes in gun control laws, innovative policing strategies, increased numbers of police officers, increased reliance on prisons, increased use of capital punishment, changes in crack-cocaine markets, changing demographics, and a strong economy. Levitt (2004) argued that only three of these could explain an appreciable part of the 1990s decline in crime: increases in the number of police (accounting for 10-20 percent of the decline in crime), the rising prison population (accounting for 33 percent of the decline in crime), and the waning of the crack-cocaine epidemic (accounting for 10-15 percent of the decline in crime).

Against these competing theories, Donohue and Levitt (2001) proposed a novel explanation. In 1973, abortion was legalized nationally with the US Supreme Court ruling in *Roe v. Wade*. Donohue and Levitt (2001) argued that this exogenous change in abortion legislation explained nearly 50 percent of the 1990s decline in crime. Two main mechanisms were argued to be at work simultaneously: cohort and selection effects.[2] The legalization of abortion reduced total fertility, which in turn reduced the number of people born in each subsequent *cohort* leaving fewer people available to be both criminals and victims, and consequently less crime. Access to legal abortions also afforded women the ability to choose, or *select*, the outcome of "unwanted" pregnancies. The national legalization of abortion lowered the overall costs of abortion, financially as well as socially, allowing greater access to and use of abortion services. The women who were most likely to give birth to children who would engage in criminal activity, specifically poor, black, teenage, and unmarried women, were also the women who were most likely to seek abortions. These "at risk" women sought abortions at higher rates after the legalization of abortion made the procedure more accessible. The selective abortion of "unwanted" children reduced the likelihood that children would be born into adverse family environments including poverty, maternal rejection, neglect, drug and alcohol abuse, and various other unfavorable parental behaviours. Donohue and Levitt (2001) cited evidence that children born into such adverse environments were disproportionately more likely to be involved in criminal activity in adulthood. Accordingly, they argued that fifteen to twenty years after *Roe v. Wade*, when the children born after the national legalization of abortion in 1973 were reaching their "high-crime" years, rates of crime declined because "unwanted" children were absent. Women were better able to time births and fewer children were born into socioeconomic risk. If "unwanted" children had been born, they would have grown up in more abusive, neglectful, and socioeconomically disadvantaged environments. There were, therefore, fewer children "at risk" of being criminals and consequently less crime.

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[2] For clarity, this paper defines cohort effects as those that affect the size of birth cohorts. The reduction in number of children born in a given year due to increases in abortion, in other words, will be referred to as "cohort effects." Selection effects will refer to the changes in the relative composition of cohorts born after the legalization of abortion. According to the BMDL Hypothesis, the legalization of abortion afforded women the ability to select whether or not to take pregnancies to term. Children who would have been more likely to be criminal were therefore selected out of birth cohorts.

Although this was presented as a novel explanation, the idea had been expressed earlier. In 1970, President Nixon formed the Commission on Population Growth and the American Future. The Commission argued for stable population growth through state-funded, on-demand abortions, with the "admonition that abortion not be considered a primary means of fertility control" (1972:178). They cited a Swedish study (Forssman and Thuwe 1966) that found that the children of women who sought but were denied abortions had worse health, behavioural, and economic outcomes. The costs of the "unwanted" birth also extended to society in the form of health care costs, social assistance burdens, and criminality (US Commission on Population Growth and the American Future 1972). In 1990, Anthony V. Bouza, the former police chief of Minneapolis, wrote that abortion was "arguably the only effective crime-prevention device adopted [in the US] since the late 1960s" (275). Henry Morgentaler (1999: 40), the leading Canadian advocate of abortion services, wrote that "one of the most important consequences [of abortion] is the declining violent crime rate…. To prevent the birth of unwanted children through family planning, birth control, and abortion is preventative medicine, preventative psychiatry, and prevention of violent crime." Donohue and Levitt's (2001) study took these ideas a step further and conducted the first empirical examination of the claim, hence Lott and Whitley's (2007) coining of the Bouza-Morgentaler-Donohue-Levitt (BMDL) Hypothesis.

The theory gained attention as it was incorporated into the controversial debate surrounding abortion legislation. It was more recently popularized in the *NY Times* Bestseller *Freakonomics* (2005) by Levitt and Dubner and the media coverage that accompanied it. It has sold over 4 million copies world-wide and has spurred a franchise including a revision and thirty five translations, another book titled *SuperFreakonomics* (2009), a television focus on the book in 2006 by ABC's *20/20*, a documentary film titled *Freakonomics: The Movie* in 2010, a popular blog, a podcast radio show, and an enormous amount of media attention for both the authors and their work (Freakonomics 2011). Although this franchise is by no means hinged on the BMDL Hypothesis, it is undoubtedly one of the more controversial topics and has received recurring attention. It is of concern that this theory has the potential to be considered valid in the general population when it remains contentious and unproven among social scientists.

The present study, therefore, aims to submit the BMDL Hypothesis to further scrutiny by testing it on the case of Canada. Although abortion was decriminalized in 1969, access to abortion services in Canada was greatly increased in 1988 following the Supreme Court decision in *R v. Morgentaler*. By taking advantage of this exogenous intervention in abortion legislation and by investigating its impact on Canadian crime rates, the credibility of the BMDL Hypothesis will be tested and verified. If the increase in access to abortion services are found to be associated with a decline in Canadian crime rates, the case of Canada would provide further support for the BMDL Hypothesis. If supportive evidence is not found, however, the continued public dissemination of the BMDL Hypothesis may require amendment and retraction.

Chapter 2

2 LITERATURE REVIEW

2.1 The Econometric Debate

Donohue and Levitt's (2001) study sparked an enormous academic debate that has lasted over a decade. This debate remains, however, primarily within the field of econometrics by virtue of its original formulation. Joyce (2010), who has been heavily involved in the debate, reviewed the major econometric arguments in a summary chapter far better than could be attempted here.[3] From the perspective of a non-econometrician, reviewing the debate has demonstrated the importance of critically and thoughtfully analyzing the BMDL hypothesis. Above all else, however, the literature has demonstrated how sensitive the econometric methods used thus far have been to small variations in specification. Depending on the way researchers have specified their models, what variables they have included, and how they have designed their analyses, studies have found supporting evidence in some cases, null effects in others, and inverse effects in still other cases. Overall, the validity of the original theoretical link has been left obscure. Important elements required for the accurate investigation of the BMDL Hypothesis that have emerged during the debate will be emphasized in section 2.3 as an improved empirical test is designed. It is important, however, to review the state of the academic debate to appreciate the level of consensus established on the BMDL Hypothesis thus far.

2.1.1 *The Donohue and Levitt (2001) Study*

Donohue and Levitt's (2001) study deserves attention and explication to situate the debate. The empirical strategy they employed involves two main parts: national time series and regression analyses. In the first strategy, three main pieces of evidence are presented. When looking at the time series of crime rates from the 1990s, the break in the national crime trend from its peak in 1991 fits temporally with the legalization of abortion in 1973. By 1991, children born after the legalization of abortion would have

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[3] Please refer to Joyce, Theodore J. 2010. "Abortion and Crime: A Review." Pp. 452-487 in *Handbook on the Economics of Crime*, edited by Bruce L. Benson and Paul R. Zimmerman. Northampton, MA: Edward Elgar.

been approximately seventeen years of age, just entering into their "high-crime" years. The absence of the "at risk" children who had been aborted after 1973 coincided with the beginning of the decline in crime in 1991. Next, they took advantage of the fact that five states (Alaska, California, Hawaii, New York, and Washington, hereafter referred to as "pre-*Roe* states") repealed antiabortion laws in 1969-70, before *Roe v. Wade* in 1973. When compared to the rest of the US (hereafter referred to as "*Roe* states"), pre-*Roe* states experienced declines in crimes earlier, a trend that is also consistent with the BMDL Hypothesis. Finally, they ranked states by their abortion rates into the highest, medium, and lowest groups. Consistent with the rest of their evidence, Donohue and Levitt (2001) found that declines in crime were at least 30 percentage points greater in high abortion states relative to low ones in murder, violent, and property crime rates. The decline in crime rates of states with intermediate abortion rates fell between the high and low abortion rate states in all three categories of crime.

The authors further substantiated their evidence by characterizing the type of causal agent necessary to satisfactorily explain the decline in crime in the 1990s. The decline in crime was abrupt, included many types of crime, and was experienced nationally. A satisfactory causal explanation would, therefore, have to be equally rapid in development, cause a broad array of effects, and also be a nationally experienced intervention. Increases in imprisonment, increased numbers of police, and expenditures on crime deterrence were dismissed as having had too long an implementation period to be satisfactory explanations. City-specific interventions and experiences were also dismissed as the decline in crime was nationwide. A strong economy, although fitting with the general time period trends, had only a weak association with violent crime and was dismissed as well. Although they acknowledged that all of these explanations may have had some effect on dampening crime rates, Donohue and Levitt (2001) looked to a new strategy to empirically assess whether their proposed link between the legalization of abortion and declines in crime rates was truly a satisfactory explanation.

The second empirical strategy employed by Donohue and Levitt (2001) involved regression analyses of panel data. Their first model employed a constructed "effective legalized abortion rate" (EAR) for a given year using arrest data and abortion rates. This term sums the product of the ratio of arrests for a given cohort and the abortion rate for the year prior to that cohort's birth (i.e., approximately when the cohort was *in utero*). This term was intended to isolate the influence of abortions on criminal arrests in a given year by taking into account the fraction of arrests committed by individuals born after abortion legalization. They regressed the rates of annual state-level crime on the constructed EAR term and found that increases in the EAR were associated with declines in aggregate crime rates.

Next, they drew a more direct link between abortion and crime rates by regressing age-specific arrest data, which were available by single year of age of the arrestee, on the abortion rate of the year prior to each cohort's birth. Again, the abortion rate of the year prior to a cohort's birth was used as a proxy for the likelihood of abortion that cohorts experienced while *in utero*. After performing their analyses, they concluded that an increase of 100 abortions per 1000 live births reduced a cohort's crime by approximately 10 percent. Their calculation of the effective abortion rate for 1997 suggested that crime rates were approximately 15-25 percent lower in 1997 because of the legalization of abortion. Since homicide, violent, and property crime rates fell more than 30 percent in the 1990s, Donohue and Levitt (2001) argued that the legalization of abortion could account for at least 50 percent of the total decline in crime between 1991 and 1997 (i.e., the 15-25 percent decline explained by the legalization of abortion accounts for at least half of the total 30 percent decline in crime).

2.1.2 *Joyce's (2004) Criticisms and Donohue and Levitt's (2004) Responses*

Their study was met with swift criticism from many sources, but the three critiques that were potentially the most relevant and damaging came from economists. Theodore Joyce has been one of the most involved critics and has engaged with Donohue and Levitt at length on multiple occasions.[4] In Joyce's (2004a) first rebuttal, he argued that Donohue and Levitt (2001) erroneously assumed that no abortions took place before legalization, and therefore their use of a zero abortion ratio for cohorts born before 1973 flawed their

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[4] After the first appearance of Donohue and Levitt's work in the *Chicago Tribune* (1999), they have been in a lengthy back-and-forth with Joyce for over a decade of working manuscripts and publications (Donohue and Levitt 2000, 2001, 2003, 2004, 2006, 2008; Joyce 2001, 2004a, 2004b, 2006, 2009, 2010; Joyce et al 2012)

equations. Joyce (2004a) argued instead that nearly two-thirds of legal abortions post-*Roe v Wade* simply replaced illegal ones that occurred before legalization. Citing data from the Centers for Disease Control (CDC), Joyce (2004a) further argued that those states that had the highest rates of abortion after legalization in 1973 also had the highest rates of abortion before legalization in 1972. If so, there would have been no dramatic change in abortion rate, negating any of the causal force that the legalization of abortion could have had. Further, the abortion rate data from the Alan Guttmacher Institute (AGI) that Donohue and Levitt (2001) used was inaccurate as it measured abortion rates by the state of occurrence as opposed to the state of residence of the woman. These data were, therefore, susceptible to misrepresenting the magnitude of influence that an increase in abortion rates in a given state could have had on that state's future crime rates if non-trivial numbers of women had crossed state borders to obtain abortions. This would have artificially inflated abortion rates in some states while artificially lowering the abortion rate in others.

In response, Donohue and Levitt (2004) first argued that Joyce (2004a) was mistaken and abortions did increase substantially after legalization in 1973. The financial costs dropped significantly from $400-500 to as little as $80 after legalization, making abortions more easily accessible. The increasing trend in abortion rates also took seven years after legalization to stabilize, suggesting that the observed change in rates of legal abortion was a genuine change as opposed to simply a replacement of illegal abortions. The number of children being put up for adoption also declined after abortion legalization from nine percent of premarital births to just four percent. The authors argued with these pieces of evidence that the change in abortion rates that occurred as a result of the legalization of abortion in 1973 were real and did not represent the simple replacement of illegal abortions.

Further, Donohue and Levitt (2004) asserted that even if Joyce (2004a) was correct, his claim did not reduce the influence of legalizing abortion on rates of crime and instead made their original estimates more conservative. They argued that if, in reality, there was a smaller increase in abortions than had originally been assumed, the magnitude of the association between abortion and crime would be even greater as each unit increase in abortions would account for a larger share of the decline in crime that was experienced in the 1990s. The authors re-estimated their original analyses with data from the CDC as well as improved measures from the AGI and found that their original estimates generally increased in magnitude. With these pieces of evidence, Donohue and Levitt (2004) dismissed Joyce's (2004a) critiques of the quality of data on abortion rates and defended their original hypothesis.

A second issue that Joyce (2004a) raised was the distinct possibility that variables that had been omitted may have been responsible for period effects that were erroneously being attributed to the legalization of abortion; specifically, the crack-cocaine epidemic in the late 1980s and early 1990s. The rise and fall in crime rates in the early 1990s may reflect the rise and fall of crack-cocaine markets as they generally rose and fell between 1985-90. This period effect is a particularly difficult issue to model as it affected different regions of the US at different times and was not nationally encompassing. Furthermore, no credible measures of the actual extent of the crack-cocaine epidemic exist. If the changes in crime rates seen in the early 1990s were in fact attributable to the crack-cocaine epidemic, there is little variation remaining for an increase in abortions to explain and the association between the legalization of abortion and declines in crime rates becomes artificial and spurious. Joyce (2004a) replicated Donohue and Levitt's (2001) regressions, but divided the original 1985-97 sample frame into 1985-90 and 1991-97. When analyzed this way, Joyce (2004a) found that the original estimates were sensitive to the period being analyzed. He argued that this lack of temporal homogeneity suggests that period effects, specifically the crack-cocaine epidemic, were behind the trends in crime witnessed in the late 1980s and early 1990s as opposed to any potential selection effect from the legalization of abortion.

In response, Donohue and Levitt (2004) conceded that Joyce's (2004a) findings were indeed consistent with the effect of the crack-cocaine epidemic of the 1980-90s but argued that this did not directly negate their original claims. Donohue and Levitt (2004) argued that the crack-cocaine epidemic influenced the pre-*Roe* states, particularly California and New York, the most. The authors argued that period effects like the crack-cocaine epidemic, which was particularly pronounced between 1985-90, confound the time period and make it difficult to investigate causal claims. Failing to satisfactorily control for the crack-cocaine epidemic would bias regression estimates against finding an association between abortion legalization and declines in crime rates. The narrow focus of Joyce (2004a) on the time frames of 1985-90 and 1991-97, therefore, biased his regression analyses against finding an association between abortion legalization and crime trends. Donohue and Levitt (2004) argued that to adequately control for the crack-cocaine epidemic, it is necessary to study a longer time period so that crime trends before and after the crack-cocaine epidemic could be taken into account. They argued, therefore, that the use of their original time frame of 1985-97 was necessary for credible analyses. Further, they argued that the potential impact of the legalization of abortion during 1985-90 would have been very small because individuals born after 1973 would still comprise a small proportion of the total population. Joyce's (2004a) focus on 1985-90 to investigate abortion and crime associations was, therefore, dismissed as flawed because of the failure to control for and contextualize the crack-cocaine epidemic and because regression analyses were biased against finding an association between abortion and crime by design.

Joyce (2004a) also conducted several difference-in-difference (DD) analyses to address the issues he raised with Donohue and Levitt (2001). The DD technique is a quasi-experimental strategy that attempts to measure a treatment effect by differencing the outcomes of a control group from the outcomes of a treatment group.[5] In the first, Joyce (2004a) constructs a comparison group of states that legalized abortion in 1973, but were more similar to the pre-*Roe* states in their histories of crack-cocaine markets.[6] The use of these states offers an improved estimate of the counterfactual of the period effects experienced by the pre-*Roe* states[7] as opposed to including all of the remaining American states. Joyce (2004a) divided the study sample again between 1985-90 and 1991-96 and

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[5] Please refer to Joyce (2009) for a more thorough elaboration on the DD and DDD strategy employed by Joyce (2004a; 2009).

[6] Based on Grogger and Willis (2000), Joyce (2004a) constructed a comparison group consisting of Colorado, Florida, Georgia, Illinois, Indiana, Louisiana, Maryland, Massachusetts, Michigan, Missouri, New Jersey, Ohio, Pennsylvania, Texas, and Virginia as these states experienced crack-cocaine use in their major cities between 1984 to 1989. These states also have urban centres with large African-American populations, which were argued to improve the estimate of the counterfactual of the pre-*Roe* states that include California and New York.

[7] Unlike Donohue and Levitt (2001), Joyce (2004a) included the District of Columbia into the pre-*Roe* states.

ran separate regressions for the two periods of time. He found that when compared to a more credible comparison group, estimates of differences in violent crimes, property crimes, murders, and murder arrests between cohorts born before and after abortion legalization were small in magnitude and statistically non-significant. Further, when the time frame was divided, exposure to legalized abortion was positively associated with criminality between 1985-90, negatively associated with criminality between 1991-96, and generally not statistically significant. He then used a difference-in-difference-in-difference strategy that compared the arrest and homicide rates from 1985 to 1990 between: 1) teens and young adults who were 2) exposed and unexposed to legalized abortion in 3) pre-*Roe* states and the previously mentioned comparison states. In this specification, exposure to legalized abortion was positively associated with arrest and offending rates for murder, violent, and property crimes. These patterns were inconsistent with the BMDL Hypothesis and Joyce (2004a) instead pointed to age and period effects as causal agents for the crime trends as opposed to exposure to legalized abortion while *in utero*.

In his final analysis, Joyce (2004a) compared the violent, property, and murder arrest rates as well as the homicide rates of cohorts at 18-19 years of age that were born before and after exposure to legalized abortion.[8] If the BMDL Hypothesis were accurate, there should be a decline in arrest rates for 18-19 year olds when comparing 1993-94, when the age group was born after legalization, and 1990-91, when they were born before legalization. He used 21-22 year olds as a comparison group as they were born before legalized abortion throughout the time frame and would control for within-state variation. In these analyses, he found no support for the BMDL Hypothesis. The only coefficients that indicated a negative association between exposure to legalized abortion and declines in arrest rates were substantively marginal and statistically non-significant.

Donohue and Levitt (2004) responded to these criticisms by raising still more issues with Joyce's (2004a) specifications. First, Donohue and Levitt (2004) took issue

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[8] Joyce (2004) performed the analysis again, comparing 20-21 year olds in 1992-93, who were unexposed to legalized abortion, to 20-21 year olds in 1993-94, who were exposed to legalized abortion. He used 23-24 year olds as the comparison group. Again, he found no support for the BMDL Hypothesis.

with the inclusion of the District of Columbia (D.C.) by Joyce (2004a) as an "early legalizer" state (see footnote 10 of Donohue and Levitt 2004). Joyce (2004a) included D.C. citing evidence that their abortion facilities had 20 000 patients in 1971 and the state's resident abortion ratio was double that of California or New York in 1972. Donohue and Levitt (2004) argued that although the D.C. Supreme Court decision in *U.S. v. Vuitch* in 1969 repealed anti-abortion laws, the decision was quickly overturned in 1971 by the U.S. Supreme Court and therefore abortion services were not legal in D.C. until 1973 with the rest of the nation. Even though Joyce's (2004a) results were not sensitive to the inclusion of D.C. as a pre-*Roe* state, Donohue and Levitt (2004) took issue with Joyce's (2004a) decision and devoted a substantial footnote to the topic.

More importantly, Donohue and Levitt (2004) argued that Joyce's (2004a) focus on the time frame of 1985-90 only allowed for the examination of the criminal outcomes of cohorts during a very specific, "well chosen" period of time during the crack-cocaine epidemic, which primarily influenced younger individuals, and therefore concluded that there was no association between abortion legalization and crime (42). As discussed earlier, failure to account for the crack-cocaine epidemic could bias estimates, particularly when focusing on the years 1985-90. Further, the authors argued that ignoring the lifetime criminal involvement of exposed cohorts who were born in the areas most affected by the crack-cocaine epidemic and focusing on the crimes they committed between 1985-90 biased estimates towards finding no association between the exposure to legalized abortion and crime. When Donohue and Levitt (2004) re-estimated their analyses with a longer time frame to cover more of the lifetime criminal involvement of exposed cohorts, they found that exposed cohorts committed fewer crimes both before and after the crack-cocaine epidemic. The authors argued that this was even more compelling evidence for their hypothesis as Joyce's (2004a) analyses were unable to definitively distinguish between "exposed" and "unexposed" cohorts or between states where abortions were relatively easy or difficult to obtain because he only used a dichotomous indicator for the legal status of abortions. Donohue and Levitt (2004) concluded that Joyce's (2004a) study was generally biased by design to find no association between the *in utero* exposure to legalized abortion and later criminality.

2.1.3 *Foote and Goetz's (2008) Critique and Donohue and Levitt's (2008) Response*

A second econometric critique of note came from Foote and Goetz (2008). The first, and most crippling point they raised, was a flaw in the final regression of Donohue and Levitt's (2001) paper, arguably their most convincing piece of evidence. To review, their final analysis involved directly linking the arrest rates of cohorts to the abortion rates they experienced while *in utero*. Foote and Goetz (2008), however, pointed out that Donohue and Levitt (2001) did not include important regressors in their equation, namely a state-year interaction term that absorbs the influence of various unobserved differences between states over time that are difficult to explicitly measure. Its omission meant that their regression estimates were biased by attributing the variation that would have been absorbed by the state-year interaction term to the other terms in the regression, including the term for the effect of abortion.

A second flaw in the final regression that Foote and Goetz (2008) identified was the use of the total number of arrests of a cohort as opposed to arrests per capita of a cohort. Donohue and Levitt (2001) used this measure because they felt that there was no credible measure of cohort size per year by state to calculate a cohort rate. Foote and Goetz (2008), however, found that these data were available from 1980 on. When they corrected for these issues and re-estimated the regressions, Foote and Goetz (2008) found that the coefficients for the effect of abortion on property and violent crime arrest rates decline to essentially zero (Table 1, Column 4, p.412).

A third issue raised by Foote and Goetz (2008) was the likelihood that variables that were omitted were biasing the association between abortion and crime rates. They noted that states that had high levels of abortion also had high levels of crime before 1985, when the legalization of abortion could not have influenced crime. When regressed, there was a large and significant positive association between abortion and crime rates between 1970-1984, suggesting that both abortion and crime rates were being driven by common, state-specific factors. Declines in crime rates after 1985 that were driven by other factors may, therefore, be erroneously attributed to abortion rates. Thus, like Joyce (2004a), the authors argued that some other omitted variable, most likely a within-state period effect, must be driving the association.

In response, Donohue and Levitt (2008) acknowledged the omission of the state-year interaction term in their original (2001) analysis. They noted, however, that although the magnitude of their estimates decline after adding the state-year interaction term, the sign and statistical significance of their estimates remained the same. Although Foote and Goetz (2008) demonstrated that after correction, the violent crime coefficient declined and the property crime coefficient actually changed signs, Donohue and Levitt (2008) argued that even more corrections were necessary. Namely, the cross-state mobility of both the women who sought abortions as well as the children who were exposed to legalized abortion needed to be controlled. Donohue and Levitt (2008) also attempted to more accurately model the exposure to legalized abortion that cohorts experienced while *in utero*. When the data on rates of abortion were improved and more directly linked to crime rates, the new coefficient estimates increased to be far greater than the original estimates. Finally, when Donohue and Levitt (2008) re-estimated Foote and Goetz's (2008) analysis using several more adjustments (e.g., division-year interactions, the inclusion of D.C., the functional form of interaction terms, etc.), the new coefficients remained as strong or stronger than their original estimates, providing support yet again for the BMDL Hypothesis.

2.1.4 *Joyce's (2009) Response to Donohue and Levitt (2008)*

Joyce (2009) countered Donohue and Levitt (2008) by raising some of the recurring issues again and performing modified replications. He concluded that "the association between legalized abortion and crime rates is weak and inconsequential" (113). Specifically, Joyce (2009) argued that Donohue and Levitt (2008) underestimated standard errors in their abortion rates, their results remained inconsistent with age-specific time series plots, and their improved abortion data suffered from measurement errors. Joyce (2009) replicated Donohue and Levitt's (2008) analysis while adjusting the standard errors for within-state serial correlation and limited the sample to cohorts born between 1974-81 when abortion data were available. His results provided no support for an association between abortion rates and age-specific crime rates.

Joyce (2009) then performed analyses using two models. The first used a difference-in-difference-in-difference (DDD) estimator that compared the crime rates of cohorts born before 1974 (i.e. 1972-73), and therefore unexposed to legalized abortion, with cohorts born after (i.e. 1974-75) between 1985-2001 in the 45 states that legalized abortion with *Roe v. Wade*. This sample had the added benefit of representing cohorts born during the largest three-year increase in abortion rates, variation that Joyce (2009) argued was based on changes in the price of abortions and was thus a better test of the BMDL Hypothesis. The DDD estimator also compared the crime rates of cohorts born in states with above median changes in abortion rates to the crime rates of cohorts born in states at or below median abortion rates. The results of this model generally had positive coefficients and were not statistically significant, which provided no support for the BMDL Hypothesis. The second model that Joyce (2009) used was similar to Donohue and Levitt's (2004, 2008) strategy where crime rates by single year of age were regressed on lagged abortion rates. Unlike Donohue and Levitt (2008), however, Joyce (2009) used cohorts born between 1972-75 for the reasons cited above. These regressions similarly provided no evidence for the BMDL Hypothesis. The majority of the coefficients were positive and the negative coefficients were small in magnitude and never statistically significant. Joyce (2009) therefore concluded that "there is little evidence that legalized abortion lowers crime through a selection effect" (121).

2.1.5 *Lott and Whitley's (2007) Critique*

A third major econometric critique came from Lott and Whitley (2007). Joyce (2010) has noted that their regression analyses were unconvincing and their motives for critiquing Donohue and Levitt (2001) have also been questioned (Zimring 2007).[9] Although their regressions were weak, Lott and Whitley (2007) made other arguments worth noting. They presented data collected by the CDC between 1969 and 1972 comparing abortion rates between the pre-*Roe* states and those of states that allowed abortions when the health or life of a woman was in danger. They showed that several states in the latter category had abortion rates higher than pre-*Roe* states between 1969 and 1972. The crux of Donohue and Levitt's (2001) theory, however, argued that it was likely only wealthier women who were able to obtain abortions before legalization. It was the change in access

[9] Zimring (2007) noted that Lott and Donohue have been involved in a dispute over prior studies on the impact of the liberalization of permit-to-carry legislation on crime rates.

to abortions for "at risk" women that occurred after legalization that Donohue and Levitt (2001) argued was the driving force behind their theory. To investigate this, Lott and Whitley (2007) compared the racial composition of women who obtained abortions before legalization as a proxy for their wealth. Although the reliability and validity of such a proxy is debatable, they found that in *Roe* states, "Blacks and other women" made up 24 percent of live births, but 30 percent of abortions. In pre-*Roe* states, however, "Blacks and other women" made up 33 percent of live births but only 21 percent of abortions. With these data, Lott and Whitley (2007) argued that poorer "at risk" women made up a larger proportion of abortions in *Roe* states than in pre-*Roe* states.

Lott and Whitley (2007) also produced a series of time series plots to investigate changes in crimes rates. Based on the magnitude of Donohue and Levitt's (2001) original estimates, Lott and Whitley (2007) argued that if the legalization of abortion explained such a large proportion of the decline in crime in the 1990s, patterns should be visually apparent in basic time series plots.[10] Specifically, if the selection effect of the legalization of abortion was an important causal agent, declines in crime should be evident in younger age groups and then in successively older age groups over time. Declines in crime should also be evident in the pre-*Roe* states approximately three years before declines in crime in the *Roe* states. Lott and Whitley (2007) also presented time series graphs that plotted the teen and adult crime rates for the cohorts born immediately before and after the legalization of abortion in both the pre-*Roe* and *Roe* states to look for diverging patterns based on their exposure to legalized abortion. Although definitive conclusions cannot be drawn from these graphs, the crime trends do not provide any support for the BMDL Hypothesis. Careful examination of these graphs suggests that crime trends were heavily influenced by period effects that influenced younger individuals particularly around the early 1990s. These trends provide more support for the argument that the crack-cocaine epidemic was a major driving force behind the rise and fall in crime rates during the late 1980s and the early 1990s.

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[10] The inconsistency of Donohue and Levitt's (2001) theory and basic time series has been argued by several other scholars as well, but was done most extensively by Lott and Whitley (2007) (Joyce 2010).

For individuals who have not been thoroughly immersed in this econometric debate or are not an expert in econometric techniques, the debate seems “to end in a ‘he said, she said’ stalemate” leaving the validity of the BMDL Hypothesis uncertain (Joyce 2010:471). Of the major controversies, three stand out as particularly difficult to manage through econometric controls and methods. Although estimates exist, it is difficult to determine the credibility of data on illegal abortion rates before 1970-73. Depending on the source and preparation of abortion data, researchers have produced convincing evidence both supporting and refuting the BMDL Hypothesis. The crack-cocaine epidemic in the US has also been cited as an important driving force behind crime rates in the late 1980s and early 1990s. There are no sources of credible data, however, on the proportion of crimes that were directly related to crack-cocaine markets (Joyce 2010). Econometric methods have been used in an attempt to control for the influence of the crack-cocaine epidemic, but given its magnitude, it is difficult to reliably account for this period effect. Slight differences in model specifications have produced contradictory results and Levitt (2004) has conceded that its influence during the late 1980s was quite large. The crack-cocaine epidemic has also obscured basic time series plots of crime rates in the 1980-90s, but again, its exact impact is difficult to specify. The inconsistency of the BMDL Hypothesis with these time series plots has been evidence enough for many criminologists to dismiss the theory (Joyce 2010). Based on the causal magnitude of abortion legalization purported by Donohue and Levitt (2001), patterns supporting the BMDL Hypothesis should be evident in basic plots before such complicated and heavily specified methods are used to search for associations. What is needed for a test of the BMDL Hypothesis is an improved source of data rather than a new and potentially more complicated econometric method.

2.2 Abortion and Crime in Canada

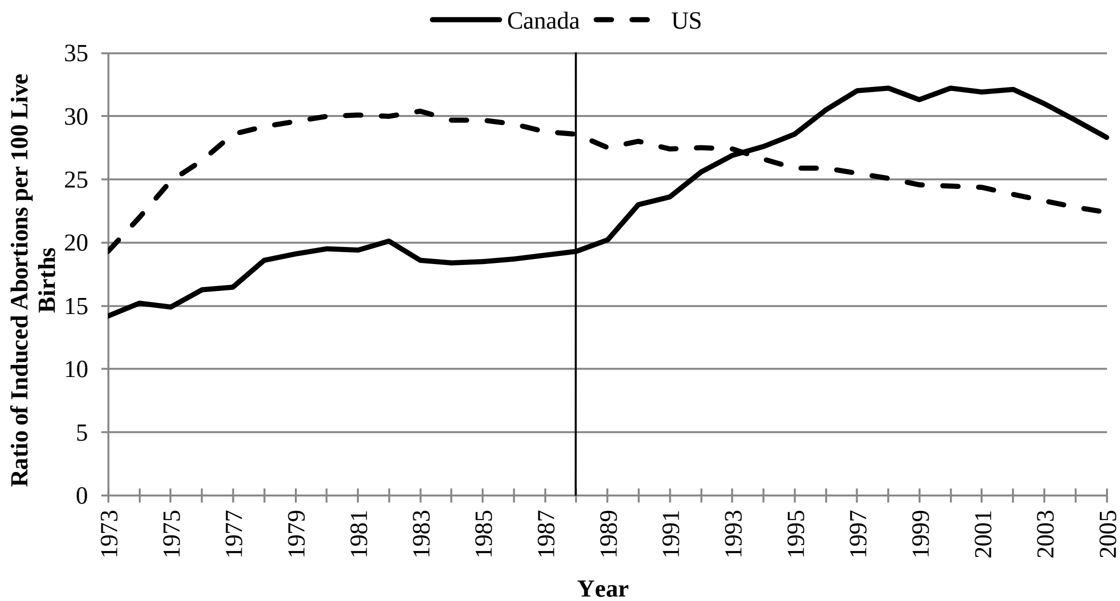
The changes in Canadian abortion legislation offers a new intervention to test the BMDL Hypothesis prospectively. Thus far, researchers have looked to the past for causes to explain the decline in crime during the 1990s in America. Proponents of the BMDL Hypothesis have argued a causal link between the legalization of abortion and the decline in crime. They have then used the declines in crime of the 1990s to provide evidence for their claims. This circular logic, although potentially valid, is difficult to substantiate (Zimring 2007). Zimring (2007:76) wrote, "[t]esting a theory only against the history that provoked it is a specially constrained empirical inquiry. The chances of coincidental timing are inescapably greater." An improved test of the BMDL Hypothesis must, therefore, use a new change in abortion legislation as the focal intervention and then look at future crime rates for evidence of a causal link.

2.2.1 *Abortion in Canada*

In Canada, abortion legislation changed in a two-step process. Abortion was partially decriminalized in 1969 with the passage of the Omnibus Bill C-150, which amended Section 251 of the Criminal Code. The revision specified that abortions required the approval of Therapeutic Abortion Committees (TAC), which were to be voluntarily established by hospitals, that were to consist of at minimum three doctors. Approval for abortion procedures was reserved for situations where the woman's health was in serious jeopardy due to a pregnancy. If approved by the TAC, abortions could only be performed in "accredited or approved" hospitals; most hospitals at the time were not accredited or approved and many of those that were did not perform abortions (Browne and Sullivan 2005). Although legal, abortion procedures were highly controlled and relatively inaccessible, particularly to the "at risk" groups of women that Donohue and Levitt (2001) identified. After the 1969 legalization of abortion in Canada, an increase in abortions was evident, but not to the extent that it was in the US (Figure 2.1). The change in the legal status of abortion in Canada was not nearly as permissive as in the US after *Roe v. Wade* (Zimring 2007). What is most important for the BMDL Hypothesis is that this change in legislation did not constitute the improvement in access to abortion services for "at risk" women that was necessary to influence crime rates through a "selection" effect.

Abortion legislation was liberalized in Canada in 1988 with amendments to the *Canada Health Act* after the Supreme Court decision in *Regina v. Morgentaler*. The existing Criminal Code legislation that covered abortion was found to be unconstitutional and struck down. Specifically, Section 251 of the Criminal Code violated Section 7 of the Canadian Charter of Rights because they interfered with women's rights, liberty, and freedom of choice (Browne and Sullivan 2005). Attempts to re-enact legislation have been unsuccessful and there remains no unique abortion restrictions under the Canadian Criminal Code (Erdman 2007). Legislation only stipulates that a qualified medical practitioner is required to perform an abortion. After 1988, therefore, obtaining an abortion no longer required an approval process and became a pregnant woman's choice. Theoretically, the 1988 liberalization of abortion is an intervention that is more in accordance with the BMDL Hypothesis as "at risk" women were able to "select" pregnancy outcomes. Abortion rates also visibly increased after 1988, suggesting that the liberalization of abortion services translated into greater use (Figure 2.1).

Figure 2.1: Ratio of Induced Abortions per 100 Live Births, Canada and US, 1973-2005



Source: Canadian data from Statistics Canada, Therapeutic Abortion Survey, CANSIM Table no.106-9005; US data from the Alan Guttmacher Institute, Jones et al (2008).

The *Canada Health Act* stipulated that only "medically necessary" procedures were to be covered by provincial tax funds. It was left to the provinces, however, to determine which procedures were considered "medically necessary" and, consequently, publicly funded. Provinces have interpreted the *Act* and implemented funding for abortions differently and thus there remains differential access to abortions across Canada. At present, some provinces fund abortions performed in both hospitals and clinics (Alberta, British Columbia, Manitoba, Newfoundland and Labrador, Ontario, and Quebec only recently) and others only abortions performed in hospitals (New Brunswick, Northwest Territories, Nova Scotia, Nunavut, Saskatchewan, Yukon). No abortions are performed in Prince Edward Island; women must leave the province and the procedure will only be covered if the abortion is considered "medically necessary." Territorial health plans cover hospital abortions and travel expenses to the nearest facility.[11] Considering the costs of abortion (approximately $500 in clinics, over $1000 in hospitals), "at risk" women are also the ones who would have the most difficulty accessing abortion services (AbortionInCanada.ca 2012). Further, clinic and hospital availability varies substantially by province. British Columbia, Ontario, and Quebec host the most facilities (23, 36, and 54 respectively) while the rest of the provinces and territories range from zero to five (Canadians for Choice n.d.). The gestational limits for abortion procedures also fluctuate widely by province from ten to over twenty-three weeks (National Abortion Federation 2010).

To perform an effective test of the BMDL Hypothesis, access to abortion services must be available to "at risk" women and rates of abortion must also be amenable to statistical analyses. That is, the sample size must be large enough and the change after intervention great enough to allow for statistical verification of the theory. Four provinces fit this description: British Columbia (BC), Alberta (AB), Ontario (ON), and Quebec (QC).[12] These four provinces also constitute the largest provinces by population (86.1 percent of the Canadian population),[13] by incidents of crime (78.5 percent of all Canadian incidents of crime),[14] as well as by number of abortions (88.7 percent of all Canadian abortions).[15] These provinces are not only the largest by population, but theoretically demonstrate the requirements for an appropriate tests of the hypothesis. They demonstrate increases in abortions after both the 1969 and 1988 interventions; the first requirement for abortions to be a causal agent (Figure 2.2). Between 1983 and 1993, the ratio of abortions per 100 live births by area of residence increased 15 percent in British

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[11] Yukon and Northwest Territories will cover travel expenses only after 12 and 14 weeks when their in-territory facilities will not perform abortions (National Abortion Federation 2010)

[12] Please refer to Appendix A for trend graphs of provincial abortion rates.

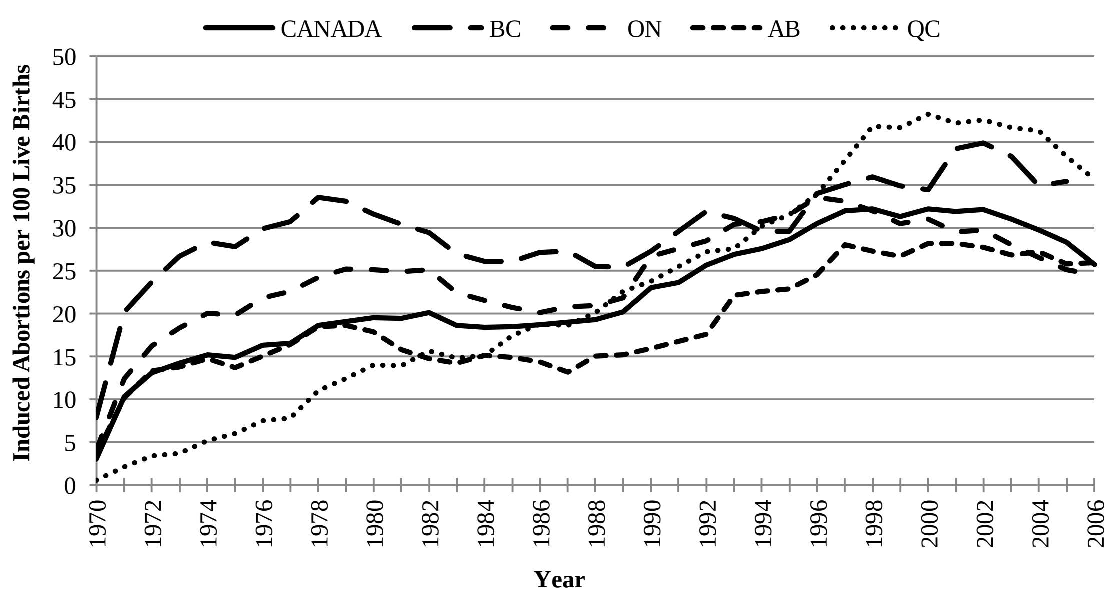
[13] Estimates based on 2011 figures taken from Statistics Canada, CANSIM table no. 051-0001.

[14] Estimates based on 2011 figures taken from Statistics Canada, CANSIM table no. 252-0051.

[15] Estimates based on 2011 figures taken from the Canadian Institute for Health Information, *Induced Abortions Reported in Canada in 2011.*

Columbia, 56 percent in Alberta, 36 percent in Ontario, and 86 percent in Quebec. Since these provinces experienced the largest increases in abortion rates following the liberalization of 1988, the BMDL Hypothesis predicts that they should also exhibit the largest declines in crime rates. The 1988 liberalization of abortion access was, however, a national legislative change and national abortion rates increased accordingly as well. Between 1983 and 1993, the national ratio of abortions per 100 live births increased 45 percent. Testing the impact on abortion liberalization on crime should, therefore, examine national crime rates as well. Recognizing the differential access to and use of abortion by province, however, may eliminate noise in the data and provide a potentially more direct test of the BMDL Hypothesis. Investigating the impact of the increased use of abortion services on the rates of crime in the four aforementioned provinces (i.e., BC, AB, ON, and QC, hereafter referred to as the "focal provinces") will provide for an improved test of the BMDL Hypothesis. Examining the focal provinces will, moreover, increase the sample size available for analysis and also provide more variation in both the independent and dependent variables, allowing analyses to produce more robust estimates and results.

Figure 2.2: Ratio of Induced Abortions per 100 Live Births, Canada and Focal Provinces, 1970-2006



Source: Statistics Canada, Therapeutic Abortion Survey, CANSIM Table no.106-9005.

2.2.2 *Crime in Canada*

The similarity of trends in Canadian and American crime during the 1980-90s make Canadian data a relevant source for testing the BMDL Hypothesis as well. The 1990s decline in American crime was a unique experience in comparison to many other similarly developed nations including France, Italy, Japan, and the UK (Zimring 2007). Canada, on the other hand, experienced relatively similar crime trends to the US (Figure 2.3 and 2.4). Between 1990 and 2000, the US experienced an average decline of 33 percent in all seven of the FBI index crimes: homicide, rape, serious assault, robbery, burglary, index larceny, and auto theft. In Canada, the declines in crime were similarly broad and substantial with an average decline of 33 percent in six of the seven index crimes[16] (Zimring 2007). These similarities are especially striking as the two nations followed very different crime policy and policing approaches. For instance, between 1980 and 2000, rates of incarceration were highly divergent between the two nations, increasing 57 percent in the US while declining 6 percent in Canada. In the 1990s, the employment of police per 100 000 population increased 14 percent in the US while it declined 10 percent in Canada (Zimring 2007).

Zimring (2007) argued that "joint causes" must be identified to reasonably explain the similarity in crime trends and dissimilarity in crime policy. That is, factors that were similar in timing, abruptness, and magnitude that were experienced in both the US and Canada were the only reasonable causal agents to explain the parallel crime trends of the 1990s. He therefore argued that the legalization of abortion was not an attractive explanation because the American and Canadian experiences differed in both legislation and timing. Canadian abortion legislation followed a "two-step" process of which only the first was relevant for explaining the decline in crime in the 1990s. The 1969 legalization of abortion in Canada was less permissive than the 1973 legalization in the US and the rates of abortion in Canada also increased more modestly than in the US.

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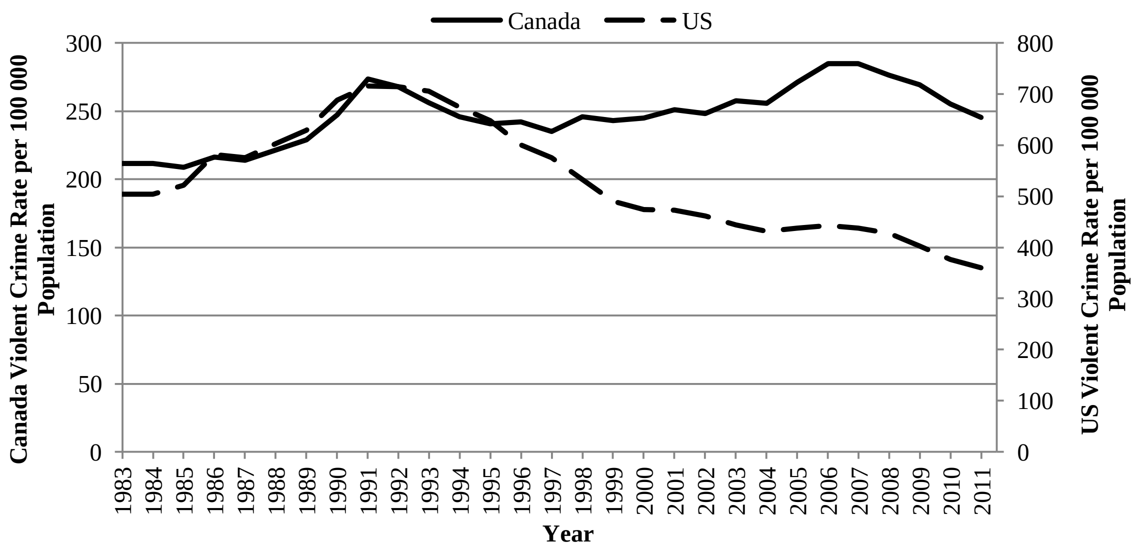
[16] Auto theft was the only index crime in Canada that did not decline between 1990 and 2000 and instead increase of 26%. When compared to available insurance data, however, a decline in auto theft remains plausible (Zimring 2007). Based on partial data from Ontario, Alberta, the Atlantic provinces, Yukon, Nunavut, the Northwest Territories, and Quebec, Zimring (2007) found that auto theft declined 32% between 1990 and 2000; a finding very close to the 37% decline in auto theft in the US during the same time frame.

Consistent with the BMDL Hypothesis, however, the relative decline in violent crime rates in the 1990 was also less dramatic in Canada (Figure 2.3). The legalization of abortion as an explanation for the 1990s decline in crime, therefore, remains plausible; its magnitude of influence, however, remains to be specified and validated.

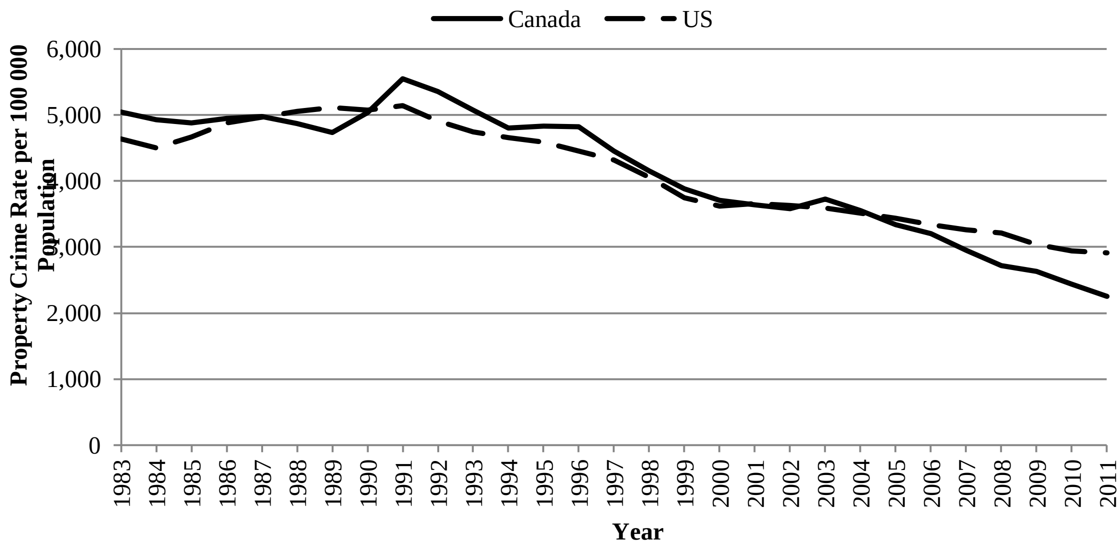
Figure 2.3: Total Selected Violent Crime Rate per 100 000 Population, Canada and US, 1983-2011



Source: Canadian data from Canadian Centre for Justice Statistics, Uniform Crime Reporting Survey; US data from FBI, Uniform Crime Reports as prepared by the National Archive of Criminal Justice Data. Canadian crime rates were calculated based on the population estimates from CANSIM table no. 051-0001.

Note: Canada and the US employ different definitions of violent crimes. Based on Gannon (2001), only comparable violent crimes have been included in Figure 2.3. These are homicide, aggravated assault, and robbery for the US, and homicide, aggravated assault, assault with a weapon, attempted murder, and robbery for Canada. Figure 2.3 presents data from 1983 on due to a change in the classification of Canadian assault categories that occurred in 1983.

Figure 2.4: Total Selected Property Crime Rate per 100 000 Population, Canada and US, 1983-2011



Source: Canadian data from Canadian Centre for Justice Statistics, Uniform Crime Reporting Survey; US data from FBI, Uniform Crime Reports as prepared by the National Archive of Criminal Justice Data. Canadian crime rates were calculated based on the population estimates from CANSIM table no. 051-0001.
Note: Canada and the US employ different definitions of property crimes. Based on Gannon (2001), only comparable property crimes have been included in Figure 2.4. These are burglary, larceny-theft, and motor vehicle theft for the US, and break and enter, total theft, and motor vehicle theft for Canada. Figure 2.4 presents data from 1983 on to maintain consistency with Figure 2.3.

2.3 Designing an Empirical Test

The dramatic decline in crime in the 1990s was the phenomenon that generated the BMDL Hypothesis. The econometric debate has, in turn, relied on 1990s crime data from the US to provide the evidence to support or refute the BMDL Hypothesis. This methodological handicap suggests that an improved empirical test should look to different sources of data (Zimring 2007). Canada offers such an opportunity because of the 1988 liberalization of abortion. Using this intervention will allow for an improved test of causality as different, but relevant, crime data are used.

Two of the major issues that prior American studies have been confounded by are the lack of reliable data on abortions before legalization and the crack-cocaine epidemic. Previous studies have used various adjustments and controls to manage these issues, but none have been completely reliable. The issue with American abortion data arises due to the unreliability of measures and sources of data before abortion legalization in 1973. The controversy surrounding the credibility of data from the AGI and/or the CDC (the two main sources for US Abortion statistics) also complicate analyses. In Canada, focusing on the liberalization of abortion in 1988 allows for the use of more reliable abortion data from both before and after the year of intervention because abortions were legal in Canada since 1969 and reporting was mandatory. Further, Canada was not as influenced by the guns, gangs, and crack-cocaine epidemics of the 1980s and 1990s (Joyce 2010; Zimring 2007). Not only was the crack-cocaine epidemic not as severe in Canada, but the crime rates of interest in this study begin in the late 1990s and 2000s; a decade after the waning of the epidemic. Prior research has found the crack-cocaine epidemic a difficult period effect to manage using reliable post-hoc controls. Using Canadian data from a time period when the crack-cocaine epidemic was not a prevalent influence may, therefore, allow for an improved empirical test by predominantly avoiding these issues rather than relying on post-hoc controls.

Canadian data have been used in analyses conducted by Sen (2007), which have been cited in *Freakonomics* as international evidence supporting the BMDL Hypothesis.[17] Sen (2007) attempted to replicate the regression from Donohue and Levitt's (2001) seminal study that used the "effective abortion rate" to predict changes in aggregate crime rates. Sen (2007) also added teen and general fertility rates to the model in an attempt to model the "unwantedness" of pregnancies. Specifically, he argued that "a significant correlation between higher abortion rates and lower crime is plausible if the decline in crime can also be associated with lower fertility rates" (2007:3). He found a significant correlation between abortion and violent crime, but not property crime. Further, he found that over a quarter of the 1990s decline in crime was attributable to increases in teen abortion rates and over half to the decline in teen fertility rates overall in the 1970s and 1980s (Sen 2007).

[17] Internationally, Sen (2007), along with Pop-Eleches (2006) on Romania, Kahane et al (2008) on the UK, and Leigh and Wolfers (2000) on Australia, have analysed the impact of changes to abortion legislation on later crime. The Romanian study looked at criminal outcomes of cohorts born after abortions was banned in 1967 and found some evidence to support the BMDL Hypothesis. The UK study found little support for the BMDL Hypothesis and instead found evidence more congruent with Lott and Whitley (2007) in that the criminal outcomes of UK cohorts born after the legalization of abortion *increased*. The Australian "study" was not an empirical analysis at all, and instead was based on "speculation" as data were unavailable.

Important issues exist, however, in this study's justifications and execution. The study was an attempt to replicate a weaker portion of Donohue and Levitt's (2001) analysis using Canadian data relying on using the "effective abortion ratio." As noted earlier, this was an innovative method for incorporating abortion data with aggregate crime data, but does not directly link the actual abortion rates experienced by cohorts while *in utero*. In Donohue and Levitt's (2001) final analysis, they used crime data that were age-specific, which allowed them to more directly link each cohorts *in utero* experience to their crime outcomes. Sen (2007), unfortunately, did not have age-specific arrest rates and therefore could not perform the age-specific crime rate analyses. Further, Sen (2007) used the 1969 change in abortion legislation as the focal year of intervention and thus examined crime data from 1983 to 1998. He argued that although the degree of legalization of abortion in Canada in 1969 was not as extensive as in the US in 1973, the difficulty of obtaining an abortion was nonetheless quite low. This argument directly contradicted the findings of the 1977 Badgley Report. The committee that produced the Badgley Report was established by the Canadian federal government in 1975 to assess the operation of the abortion law. They concluded that the procedures set out in the Criminal Code for obtaining an abortion were not operating equitably across Canada and access to abortion services were "illusionary" for many women (Committee on the Operation of the Abortion Law 1977).

Sen (2007) had abortion data, moreover, only from 1970 onward and therefore assumed zero abortions prior to 1970, in the same fashion as Donohue and Levitt (2001). As argued earlier, it is impossible to substantiate the number of abortions prior to legalization. Sen (2007) argued that his abortion data were an improvement to Donohue and Levitt's (2001) as additional years were available for analysis. The benefits of having three more years of abortion data, particularly when both sets are post-legalization, seem trivial. An improved empirical test should, therefore avoid such controversies altogether and rely more heavily on credible data. Instead of constructing an "effective abortion ratio" that theoretically weighs the influence of abortion rates in lagged years, empirical analysis should rely on directly linking the actual *in utero* abortion experience of cohorts with their age-specific criminality. In his review, Joyce (2010) noted other issues with Sen's (2007) study, criticizing it as a "step backwards in the debate" (475). For Joyce

(2010), the preferred strategy for investigating the BMDL Hypothesis is the DDD estimator, discussed above. Joyce (2010) noted, however, that since Canada has only thirteen provinces and territories to America's fifty, there is less variation in Canada than in the US. Unfortunately, the DDD strategy would also be inappropriate in Canada as there is simply not enough variation for this strategy. The most damaging of Joyce's (2010) critiques, however, is Sen's (2007) lack of age-specific data on crime rates. He is, therefore, unable to link the crime rates of cohorts directly with the abortion rates they experienced *in utero*. This is the most direct link and therefore the best test of the BMDL Hypothesis.

Berk et al. (2003), although not featured prominently in the debate, made profound arguments about the econometric debate from a non-economic perspective. They conducted a time-series analysis examining a more proximal test of the BMDL hypothesis, namely, the impact of legalized abortion on youth homicides. They raised many methodological concerns that were quite damaging for much of the econometric debate that they cited (e.g., Donohue and Levitt 2001; Joyce 2004a; Lott and Whitley 2007). The econometric literature's reliance on regression analyses posed many problems including issues of missing data, measurement error, untestable assumptions, overfitting, and multicollinearity. For instance, Berk et al (2003) argued that the differing results of the debate have been due to the selection of different control parameters, a point that has been emphasized by Moody and Marvell (2010). Prior studies have used a variety of different socio-demographic, economic, policing, and other social service variables in each of their models. Berk et al (2003) argued that this has produced complex results that have been difficult to interpret and has made meaningful conclusions difficult to draw. The authors also highlighted the issue surrounding abortion data. As there is no definitive measure of abortions before legalization, regression-based associations that employ unreliable data can be misleading.

To address the issues of previous econometric studies, Berk et al. (2003) proposed the use of time series analyses. This approach was an attractive method as it avoided many of the pitfalls of previous regression analyses. The authors claimed that previous studies have "asked too much of the data and of the causal modeling approach"

(2003:48). Alternatively, Berk et al (2003) employed a quasi-experimental perspective with an interrupted time series design to look for a relatively discrete change in homicide rates of 15-24 year olds between 1970 and 1998 in the US. The timing of the intervention's impact was set at 1988, 15 years after the 1973 *Roe v. Wade* decision. The authors conducted analyses on 40 time series (ten ages by two race categories by two sexes) and overall found evidence supporting the BMDL Hypothesis.

Given the ability of Berk et al. (2003) to so sharply identify the shortcomings of previous studies, their solutions seem insufficient, particularly for the case of Canada. Firstly, they employed national measures of abortion and crime. The rationale for this decision rested on the use of the US as a whole as their unit of analysis in an attempt to control for inter-state migration (i.e., when women obtain an abortion in a different state than their state of residence or when they move to a different state after giving birth) by design. The authors stated that they were looking to characterize the impact of a federal decision, and therefore used the US as a whole for their analysis. Prior research, however, has identified that the availability of abortion services has differed substantially by state (e.g., Donohue and Levitt 2001). Berk et al's (2003) use of a simple binary variable (i.e., 0 = before 1973, 1 = after 1973) to characterize the intervention of legalized abortion also does not seem to have been an optimal solution. Many studies (e.g., Joyce 2004; Lott and Whitley 2007) have demonstrated that abortions were taking place at high levels before legalization. The credibility of different estimates for rates of abortion prior to legalization remains debatable. The fact remains, however, that abortions were taking place at non-negligible levels before 1973. That five states were allowing abortions before 1973 and that other studies (namely Donohue & Levitt's paper that sparked this literature) used this very source of variation for quasi-experimental designs suggests that using 1973 as the year of intervention is untenable. Using a dichotomous indicator of abortion legislative status to represent changes in abortion rates is, therefore, an inappropriate proxy (Donohue and Levitt 2004). Time-series analyses that characterize abortion legislation changes in this manner are, therefore, not an appropriate method for testing the BMDL Hypothesis. Instead, abortion rates should be incorporated to directly capture the selection effect that the BMDL Hypothesis purports (Donohue and Levitt 2004).

Furthermore, it is reasonable to assume that abortions were not extensively available immediately after legalization and required a period of time for implementation. American abortion rates confirm that it took around seven years for abortion rates to stabilize (Figure 2.1). This variation in abortion around 1973 suggests that using a binary variable to categorize abortion legalization, while being appropriate for a legal impact study, is not a good test of the BMDL Hypothesis. This is the case particularly for a Canadian test as abortion rates increased in a much less dramatic and discrete manner after 1988 than in the US following 1973. Instead, the strategy employed to test the BMDL Hypothesis should incorporate actual abortion rates in order to be sensitive to gradual and progressive changes.

Secondly, Berk et al. (2003) used the number of homicides each year as their outcome variable to investigate the link between abortion legalization and crime. It was erroneous to use incidents rather than a rate as there is no control for potential cohort size changes. This was an odd oversight as they repeatedly emphasized the cohort size explanation of the BMDL Hypothesis (i.e., fewer potential victims and offenders). As Joyce (2010: 471) emphasized, the "outcome should be a rate of crime and not a level."

Finally, the use of homicides as the outcome variable of interest was argued thoroughly by Berk et al (2003), but not convincingly. Homicides were argued to be the most reliably recorded crime measure and thus a good variable to use for testing the BMDL Hypothesis. They used homicides by year of age of the *victim*, not the accused, based on the assumption that "people tend to kill others like themselves" (Berk et al. 2003:50). They wrote "[t]his assumption follows from the kinds of activities engaged in (e.g., gangs) and more generally, social and economic forces that tend to foster interaction among people who are alike. One key instance is the ways in which income often produces class and racial homogeneity within neighborhoods" (Berk et al. 2003:50). As consistent with past research this assumption may be, it was inappropriate to test the BMDL Hypothesis by using measures of the victims of crime, individuals who may or may not have been part of the treatment group at all, and therefore theoretically irrelevant. A more appropriate test of the hypothesis would have been to isolate and analyze the behaviours of those individuals who were and were not in the "treatment" group. That is, the criminality of individuals born after the legalization of abortion in comparison to those born before.

Joyce (2010) has noted that this literature has resulted in more confusion than consensus. He therefore summarized eight features of an empirical test of the BMDL hypothesis on which all parties may be able to agree (Joyce 2010:471-472):

1. The crime measure must be age-specific in order to identify cohorts.
2. The outcome should be a rate of crime and not a level.
3. The hypothesis should be consistent with the timing of abortion legalization and should be evident or not contradicted by basic time series plots.
4. The abortion rate should be measured by state of residence.
5. The abortion rate should be inversely related to fertility rates.[18]
6. Regressions of age-state-year crime rates should include state-year fixed effects.
7. The number of observations with no measure of abortion should be minimized.
8. Statistical tests should take account of the autocorrelation in crime rate residuals.

With these features in mind, this study proposes an improved empirical test of the BMDL Hypothesis by including critical developments and avoiding major controversies by design rather than through statistical controls.

The major improvement that this study proposes is to take advantage of the 1988 liberalization of abortion in Canada as the focal intervention for three reasons. First, the reliability of abortion data before and after this intervention is far superior to the data available before the 1969 legalization in Canada and the 1970-73 legalization in the US. Canadian abortion data are available from 1970 onward and report the province of residence of the woman. Second, the use of Canadian data avoids the biases created by the crack-cocaine epidemic that has plagued the American studies as this history effect was not a major factor in Canada (Joyce 2010). Finally, to properly test the BMDL Hypothesis and to avoid the circular reasoning of previous studies, selecting a different, yet theoretically parallel, intervention and then examining its impact on future crime rates

[18] Several scholars (e.g. Sen 2007; Joyce 2010) have argued that increases in abortions must be associated with declines in fertility for the BMDL Hypothesis to have plausibly had an impact on unwanted births. Fertility rates, however, are not theoretically critical to the BMDL Hypothesis. Donohue and Levitt (2004:33) asserted that "[a]s long as the number of unwanted births falls [evidenced by increases in abortions], even if total births do not decline at all, one would expect to see better life outcomes on average for the resulting cohorts." This is the core assumption of the BMDL Hypothesis' logic of the selection effect. Tests of the BMDL Hypothesis must, therefore, focus on the impact of increases in abortions irrespective of changes in total or teen fertility rates.

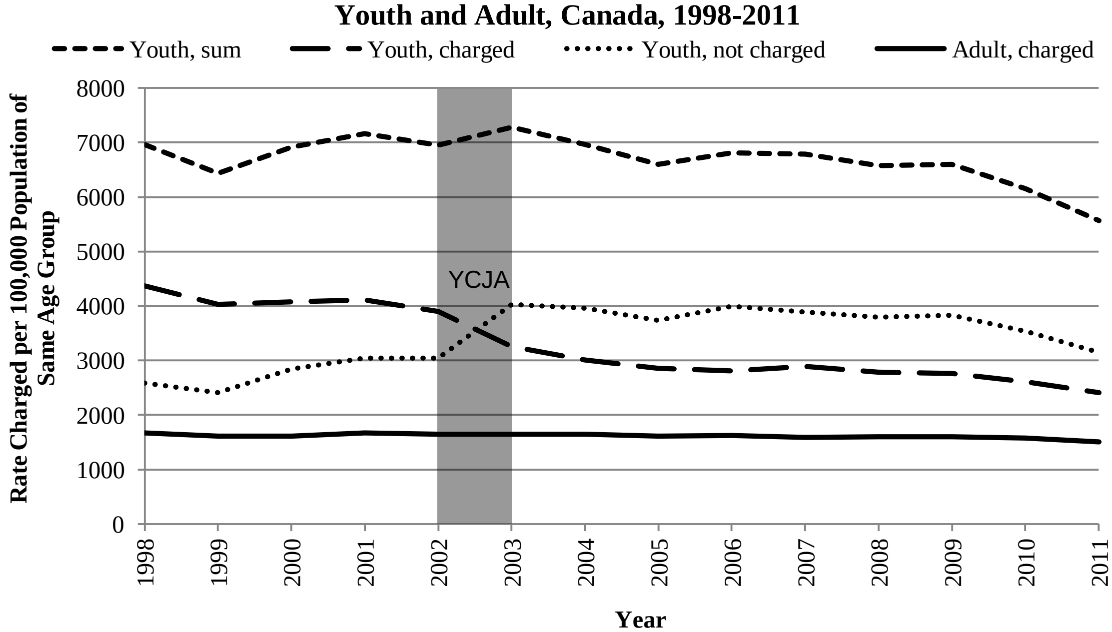
is a far more attractive and tenable method. In accordance with Joyce's (2010) suggestions, Canadian crime data will be used that measure crime as a rate. Donohue and Levitt (2004) argued that linking the crime rate of cohorts with the abortion rate that they experienced *in utero* is a better proxy for unwantedness than a binary legalization indicator. In order to investigate the selection effect of the BMDL Hypothesis, therefore, regressions must use crime rates and not a number crimes (Joyce 2009).

Since this study will focus on the 1988 liberalization of abortion, the crime rates from the late 1990s to 2011 will be examined. The focus will, therefore, remain on the criminality of relatively younger cohorts. In Canada, however, an important potential threat to internal validity must be addressed. The *Juvenile Delinquents Act* was the legislation governing youth crime in Canada until 1984 when the *Young Offenders Act* (*YOA*) came into effect. The *YOA* had a profound influence on the practices of charging young people, creating a substantial increase in the youth crime rate and a substantial decrease in the use of extrajudicial measures to manage young offenders (Carrington & Schulenberg 2005). The use of custody and courts in Canada under the *YOA* was much higher than other comparable western nations (Bala, Carrington & Roberts 2009). The large and immediate increase in the rate of youth being charged for crimes without a similar pattern in adult rates indicates that the changes in the youth crime rate following 1984 were the effect of the *YOA* and not of changing rates of youth crime.

The *Youth Criminal Justice Act* (*YCJA*) came into effect on April 1, 2003. The intent of this legislation was to encourage the use of extrajudicial measures instead of heavily relying on the formal judicial system for managing youth crime and to more effectively respond to the relatively small number of serious crimes of violence. The *YCJA* has been successful in reducing the use of the formal court system for less serious crimes, while maintaining similar rates for more serious offences. Further, the use of extrajudicial measures has increased in complement to the decline in the charging of youth, indicating that charging practices have changed, not youth crime (Carrington & Schulenberg 2005; Bala, Carrington & Roberts 2009).

Figure 2.5 illustrates the outcomes of criminal incidents involving youth compared to adults over time. The distinct decline in 2003 of incidents that resulted in youth being charged was supplanted by a complementary increase in incidents where youth were not charged (i.e., dealt with using extrajudicial measures). The rate of total chargeable youth offences (i.e., the sum of youth who were charged and youth who were not charged) and the adult rate show little variation around 2003. These results suggest that changes in legislation have had significant effects on the charging practices of police, but have not gone so far as to significantly affect incidents of youth crime itself. This study will, therefore, use rates of youth accused of crime rather than rates of youth charged with crime to measure youth offending. Young offenders in this study were born before and after the liberalization of abortion in 1988 and were therefore equally affected by the YOA and YCJA. Significant differences in crime rates between affected and non-affected cohorts can, therefore, be attributed to other causal agents like the liberalization of abortion in 1988 and not to changes in the legislation governing youth charging practices.

Figure 2.5: Violent and Property Criminal Code Violations, Rate per 100 000 Population, Youth and Adult, Canada, 1998-2011



Source: Statistics Canada, Criminal Justice Statistics, Uniform Crime Reporting Survey
Note: Youth are ages 12-17, adults are 18 and over

2.4 The Current Study

The current study aims to contribute to the literature by testing the BMDL Hypothesis on the liberalization of abortion in Canada that occurred in 1988. As discussed previously, this intervention offers an improved source of data by avoiding the issues surrounding measures of illegal abortions and the crack-cocaine epidemic. Taking the elements outlined by Joyce (2010) into consideration, this study will also employ abortion rates by area of residence of the women and age-specific crime rates. The time series of relevant crime rates will also be plotted to assess the plausibility of the BMDL Hypothesis. Many of the elements outlined by Joyce (2010) are relatively simple and largely accomplished by using the 1988 liberalization of abortion as the focal intervention. The selection of a credible and tenable analytic strategy is a more difficult task.

Four main analytic strategies have emerged from the econometric debate that has surrounded the BMDL Hypothesis. The DDD strategy (i.e., Joyce 2009) and time series analyses (i.e., Berk et al 2003) both relied on the use of dichotomous characterizations of the legal status of abortion. In essence, they were similar to a legal impact study, examining the impact of a change in legislation by characterizing it using a pre/post intervention strategy. These strategies, therefore, assumed that the legalization of abortion had an impact immediately after changes in legislation had been made. Further, these strategies did not incorporate the extent or rate of change in abortion rates that occurred after the legalization of abortion. That is, if abortion rates substantially increased immediately following the legalization of abortion and maintained a uniform rate hence, they would be modelled appropriately using a dichotomous categorization. If, however, abortion rates deviated from this ideal situation (e.g., if abortion rates were relatively similar in magnitude before and after the legalization of abortion), the two time periods would be attributed with an inappropriately dichotomous distinction. In opposition to this ideal situation, the rates of abortion that followed 1988 increased gradually (Figure 2.1 and 2.2). To test the BMDL Hypothesis, therefore, models must take into account actual changes in abortion rates to accurately identify associations between increases in abortions rates and declines in crime rates.

The construction of an "effective abortion rate" (EAR) (i.e., Sen 2007) and the use of age-specific crime rates (i.e., Donohue and Levitt 2001) were the other two strategies that were featured prominently in the debate. These two strategies incorporated data on rates of abortion into their models, allowing for more sensitivity and accuracy in estimating the association between changing abortion rates and crime rates. Unlike the DDD and the time series analysis strategies that investigated the impact of a change in legislation, the EAR and age-specific crime rate strategies examined the impact of a change in the rate of abortions. The EAR strategy was first employed by Donohue and Levitt (2001) and also by Sen (2007) in the sole Canadian analysis of the BMDL Hypothesis. The EAR strategy regresses the average weighted influence of abortion exposure that individuals experienced *in utero* on the aggregate youth crime rates of a given year. The second strategy regressed age-specific crime rates on the abortion rates of the year prior to a cohort's birth to approximate the exposure to abortion these cohorts experienced while *in utero*. As argued previously, the latter method was superior as it directly linked the *in utero* exposure to abortion that cohorts experienced with their crime outcomes while the EAR strategy relied on regressing aggregate crime rates on weighted and aggregated abortion rates. Although the EAR strategy relied on stronger assumptions regarding the influence of abortion rates on cohorts, it will be attempted again using the 1988 liberalization of abortion as a new exogenous source of abortion variation to test the plausibility of the BMDL Hypothesis. The age-specific strategy will also be used as it theoretically constitutes a more direct test of the BMDL Hypothesis. These two strategies have also been successfully used to find support for the BMDL Hypothesis in prior research. The EAR and age-specific strategies will, therefore, be employed to perform improved investigations of the BMDL Hypothesis. The current study will, therefore, investigate the impact of an increase in the abortion rate as opposed to a change in abortion legislation.

Contrary to prior studies, the current study will not include the various economic, socio-demographic, policing, and other control variables that have been common in order to examine the link between abortion and crime as clearly as possible and to avoid the issues of complexity and heavy parameterization (Berk et al 2003).[19] Furthermore, national analyses will be conducted to test the plausibility of the BMDL Hypothesis in Canada. These will benefit from avoiding the issue of interprovincial migration (Berk et al 2003). Due to differences in the provincial variation in the availability of abortion, the abortion and crime rates of the four focal provinces (i.e., BC, AB, ON, and QC) will also be analysed in a single model. These provincial data will constitute an improved analysis to supplement the national analyses by providing a larger sample size, more variation in the dependant and independent variables, and a more theoretically direct test of the BMDL Hypothesis.

[19] Please refer to footnote 27 and Appendix D for a discussion concerning covariates from prior research and their employment in the present study for robustness checks.

Chapter 3

3 METHODS

This study estimates two sets of fixed-effects models that resemble the design used by Sen (2007) and Donohue and Levitt (2001), in which crime rates were regressed on lagged abortion rates. The details of these two strategies will be further elaborated on in Section 3.2.

3.1 Data

The number of induced abortions per 100 live births in both hospitals and clinics is the focal independent variable used in this analysis, following the convention established in past research. The ratio of abortions per live births was the metric used as the explanatory variable when the EAR and age-specific crime rate strategies were employed in the past (i.e., Donohue and Levitt 2001; Sen 2007). This metric is theoretically important for the BMDL Hypothesis because it measures abortions as a proportion of completed pregnancies. It is, therefore, more parsimonious than other possible metrics, such as a measure of abortions per females of childbearing age while controlling for fertility. The ratio of abortions per 100 live births was, therefore, selected as it captures the proportion of pregnancies that were terminated versus the proportion that were brought to term. Statistics Canada reported this measure from 1970 to 2006 by the province of residence of the patient. This study employs Canadian abortion data from the *Therapeutic Abortion Survey* (TAS), CANSIM Table 106-9013, which was collected by Statistics Canada from 1969 to 1994 and by the Canadian Institute for Health Information from 1995 on. The TAS is a cross-sectional census conducted on an annual basis per calendar year. The province of residence for Canadian women who obtained abortions in the US was reported as well. Data for 1991 were not available provincially and were imputed by averaging the values for 1990 and 1992.

Some issues should be noted with the TAS data. The abortion legislation that decriminalized abortion services from 1969 to 1988 also included a clause that required the mandatory reporting of all induced abortions that were performed in Canada to the

Dominion Bureau of Statistics and later, to Statistics Canada. The coverage of the TAS was, therefore, considered to be 100 percent of all induced abortions from 1970 to 1987 (Statistics Canada 2009). The liberalization of abortion in 1988, however, also removed the clause for the mandatory reporting of all induced abortions to Statistics Canada. The reporting of induced abortion statistics is now voluntary, but the Canadian Institute of Health Information maintains that the TAS represents 90 percent of all induced abortions performed in Canada (Canadian Institute for Health Information 2003).[20] Despite its limitations, the present study uses the TAS as it is the only source for abortion statistics in Canada.

The violent and property crime rates per 100 000 population are the dependent variables of interest in this study. Canadian crime data were taken from the *Uniform Crime Reporting Incident-based Survey* (UCR2), which has been conducted by the Canadian Centre for Justice Statistics (CCJS) since 1988. The UCR2 is also a cross-sectional census conducted on an annual basis per calendar year. The first affected cohort (i.e., those born in 1989) had only reached 22 years of age by the end of the study time frame (i.e., 2011) because the liberalization of abortion in 1988 was selected as the focal year of intervention. An examination of youth crime rates, as opposed to total crime rates, was therefore attempted as a more proximal replication of Donohue and Levitt's (2001) strategy. The EAR analyses examined youth crime rates (i.e., 12-17 year old) between 1998-2011 for all ten Canadian provinces, which encompassed cohorts born between 1973 and 1999. Unlike prior studies, abortion rates were available for all years of birth included in this analysis. The present study analyzed rates of youth violent and property crime were separately using two sets of panel data.

This study, as with the prior research surrounding the BMDL Hypothesis, relied on official police statistics to capture incidents of crime. It is well recognized that biases are inherent in official crime statistics due to the under-reporting of incidents of crime to

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[20] Please refer to "Data Quality in the Therapeutic Abortion Survey" and "History of the Therapeutic Abortion Survey" for more detailed accounts of the methodological issues in the Therapeutic Abortion Survey. Available in the Documentation section of the Definitions, data sources, and methods of the Therapeutic Abortion Survey, available at http://www23.statcan.gc.ca/imdb/p2SV.pl?Function=getSurvey&lang=en&db=imdb&adm=8&dis=2&SDDS=3209#a3

the police, as well as justice officials' discretion used in the handling of incidents of crime reported to them (Gabor 1999). The biases involved in youth crime rates are of particular concern because they are the focus of the analyses of the present study. Specifically, it may be more likely that the criminality of younger aged youth (i.e., 12-15 year olds) is more easily "forgiven," than for slightly older youth (i.e., 16-17 year olds), who may be treated more harshly (Gabor 1999). Youth crime rate statistics in the UCR2, moreover, combine the rates of all youth incidents of crime that have been charged as well as those that were managed using extrajudicial measures. The proportion of "chargeable" youth who were not charged suffers from the bias of individual interpretation as it is under the discretion of individual police officers to decide whether or not to lay a charge for incidents of crime and, if not laying a charge, decide whether or not to record the incident at all (Carrington and Schulenberg 2005). These biases caused by the variation between individual officers as well as between police services in the reporting of criminal incidents involving youth who are not charged are stable enough over time to conduct time series analyses when the data are aggregated to the provincial level (Carrington and Schulenberg 2005). The present study, therefore, utilized crime data that were, at minimum, aggregated at the provincial level to overcome these potential limitations.

To obtain age-specific data for the age-specific crime rate analyses custom tabulations were ordered directly from the CCJS. Unfortunately, reliable measures for the age of accused could only be obtained from 2006 to 2011.[21] This study used national crime rates by single year of age of accused, and provincial crime rates in two-year age groupings due to censorship demands. Crime data were received as incident counts rather than rates and were subsequently converted to rates per 100 000 population using population estimates by single year of age from Statistics Canada, CANSIM Table 051-0001. The influence of legislative changes that created discontinuity in offence

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[21] Please refer to Summary of changes over time – Uniform Crime Reporting Survey in the "Definitions, data sources and methods" section of the Uniform Crime Reporting Survey for changes in coverage of the survey, available at http://www23.statcan.gc.ca/imdb/p2SV.pl?Function=getMainChange&SurvId=3302&SurvVer=0&InstaId=15093&SDDS=3302&lang=en&db=imdb&adm=8&dis=2.

classification and recording in the UCR2 was counteracted thorough data management.[22] Crime categories that were removed, added, or dramatically altered between 2006-11 were removed from analyses to maintain internal consistency.[23]

Youth violent and property crime rates from 1998 to 2011 were analysed separately using the EAR strategy. The annual violent and property crime rates of youth were linked with the "effective abortion rate" calculated for each year, which required abortion data from 1980 to 1998. Abortion data were available for all of these years, unlike prior application of the EAR strategy.

The age-specific analyses use four data sets consisting of age-specific crime rates of individuals aged 12-31. These individuals were born between 1973 and 1999. Abortion rate data were also available for all of these years. The first two data sets linked the national violent and property crime rates by single year of age with the national abortion rate of the year prior to each cohorts birth. The second data set included only British Columbia, Alberta, Ontario, and Quebec as they were the largest provinces by population and experienced large increases in abortion rates after 1988. As mentioned previously, the provincial crime data were obtained in two-year age groupings and so the abortion rates that corresponded to the years prior to the birth of these two-year cohorts were averaged. For instance, in 2004, the crime rates of sixteen and seventeen year olds were reported together. Since these individuals would have been born in 1987 and 1988, the abortion rates for 1986 and 1987 were averaged together to produce the abortion rate these cohorts experienced while *in utero*.

3.2 Analytic Strategy

Descriptive time-series plots of crime rates were produced to examine broad patterns and trends. Time-series plots should be consistent with, or at least not contradict, the BMDL Hypothesis, but they are not a conclusive test on their own. These plots will evidence the type of forces that were predominantly driving crime trends as period and selection

[22] Please refer to Legislative Influences (2011) in the "Documentation, data sources and methods" section of the Uniform Crime Reporting Survey, available at http://www23.statcan.gc.ca/imdb-bmdi/document/3306_D6_T9_V6-eng.htm.
[23] Please refer to the Appendix B for a complete list of violent and property crimes included in this analysis.

effects present in characteristic and distinct patterns (Joyce 2010). That is, the BMDL Hypothesis predicts declines in crime that should follow patterns consistent with selection effects. Declines in crime should be evident in younger cohorts followed by declines in successively older cohorts over time in an ordered manner to indicate the presence of selection effects. Period effects driving changes in crime rates rather than the selection effects proposed by the BMDL Hypothesis would be indicated by the crime rates of different cohorts changing in unison. Prior analyses that have attempted such time series plots have been biased by profound confounders, namely the crack-cocaine epidemic of the late 1980s and early 1990s. The time-series plots produced in this study have the benefit of avoiding such confounding period effects as they focus on the 1998-2011 and 2006-11 time frames. This should allow the selection effects of the BMDL Hypothesis to be more visually apparent.

The first statistical analysis involved regressing the constructed "effective abortion rate" (EAR) on annual youth crime rates based on the strategy used by Donohue and Levitt (2001). This strategy was employed here to assess the plausibility of the BMDL Hypothesis because it has been used in the past to produce supportive evidence (i.e., Donohue and Levitt 2001), particularly in the sole Canadian empirical investigation (Sen 2007). This analysis focused on the crime rates of youth that were 12-17 years of age because only 23 years have elapsed from 1988 to 2011. The *EAR* term was constructed following the method outlined by Sen (2007), but the weighting procedure was modified to be more theoretically parallel with Donohue and Levitt's (2001) original EAR weighting procedure. The EAR equation used in this study was

$$(1) \qquad EAR_t = \sum_a ABORTION_{t-a} * \left(\frac{POPULATION_a}{POPULATION_{total}}\right),$$

where the EAR in year t equals the sum of the weighted abortion rate of each age group, a. *ABORTION* is the ratio of induced abortions per 100 live births in the year prior to the birth of each age group. The weight in parentheses is the number of individuals of age group a in year t divided by the total number of individuals 12-17 years of age in year t, or the ratio of the 12-17 year old population that is in age group a in year t. To explain how Equation 1 was derived, it may be beneficial to review Donohue and Levitt's (2001) original calculation. Their equation was as follows,

(2) $$EAR_t = \sum_a ABORTION_{t-a} * \left(\frac{ARRESTS_a}{ARRESTS_{total}}\right),$$

where the weight in parentheses was the arrests in year *t* committed by age group *a* divided by the total arrests of year *t*, or the ratio of crimes committed by age group *a* in year *t*. Both Sen (2007) and the present study lacked age-specific arrest data and therefore were not able to perfectly replicate Donohue and Levitt's (2001) strategy. Sen (2007) used population composition ratios to weight abortion rates to overcome this limitation as follows,

(3) $$EAR_t = \sum_a ABORTION_{t-a} * \left(\frac{MALES\ 15\text{–}24_a}{TOTAL\ POPULATION_a}\right),$$

where the weight in parentheses was the proportion of the total population that were males 15-24 years of age. This weighting procedure was constructed under the assumption that the legalization of abortion reduced the cohort sizes of males who commit the majority of crime. Sen (2007) elected to use the ratio of the total population constituted by males 15-24 years of age in a given year as a proxy for the ratio of crimes committed by age group *a* in year *t*. It is unclear why Sen (2007) included females in the denominator but not the numerator in Equation 3. The population of males in cohort *a* divided by the total male population in year *t* is the theoretically driven weighting procedure that should have been used under Sen's (2007) assumptions. This procedure would still use the population of males as a proxy for the population of arrestees, but would include only males in the denominator of the weight. This would be more consistent with Donohue and Levitt's (2001) weighting procedure to capture the ratio of crimes committed by age group *a* in year *t* if males are assumed to be more representative of arrestees. The present study did not elect to make such assumptions about the gender differences in criminality because it is not an accurately measured control and the legalization of abortion should have influenced the cohort sizes of both sexes indiscriminately. This study therefore used the proportion of the population that was represented by cohort *a* in year *t* as the weight to calculate the EAR.[24]

This study offered the use of 1988 as the focal year of intervention as a second improvement. Donohue and Levitt's (2001) and Sen's (2007) lack of data on abortion rates prior to the legalization of abortion was a major criticism of both studies. This meant that Donohue and Levitt's (2001) EAR terms included zeros for abortion rates prior to 1973 and Sen's (2007) EAR terms included zeros for abortion rates prior to 1970. Prior EAR analyses conducted on American data attempted to overcome this limitation by using combined estimates from various sources (i.e., the AGI and CDC). The lack of reliability that is inherent in estimates of illegal behaviours were, however, unavoidable. The present study avoided this issue altogether by focussing on the crime rates of 12-17 year olds between 1998-2011. These cohorts were born in 1981 at the earliest. Actual abortion rates were therefore available for every cohort included in the EAR analyses.

The EAR regression equation estimated in this study was

$$(4) \qquad \ln(CRIME_{it}) = \beta_0 + \beta_1 EAR_{it} + \gamma_i + \lambda_t + \epsilon_{it},$$

where *CRIME* referred to the natural logarithm[25] of either violent or property crime rates per 100 000 population, *EAR* was the effective abortion rate, γ was the province fixed effect, λ was the year fixed effect, ϵ was the error term, and *i* and *t* indexed the province and year, respectively. The fixed effects are dichotomous dummy variables that were added for each province and year. These terms were included to control for persistent differences in crime between provinces and to control for time trends that were experienced by all provinces in a given year.

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[24] Sen (2007) argued that his weighting procedure was a valid method based on a Pearson correlation coefficient of 0.67. The Pearson coefficient between this study's weighting procedure and Sen's (2007) weights was 0.97. The Pearson coefficient between this study's weighting procedure and the theoretically more parallel method described above (i.e. male population in cohort a in year t / total male population in year t) was 0.98. Based on this evidence, the use of any of the three weighting procedures would be appropriate. Sen's (2007) weights, however, were not theoretically parallel to Donohue and Levitt's (2001) method, assuming male over-representation in criminality. This study did not elect to make such assumptions and instead based EAR weights on population proportions that included both sexes as crime and abortion rates also included men and women.

[25] The models used the natural logarithm of crime rates to maintain consistency with the prior research central to the econometric debate.

Ideally, the EAR analyses should only be conducted on theoretically relevant provinces. That is, the only provinces that should be included in the analyses are those that experienced improved access to abortion service after 1988 as this is a necessary precondition for the BMDL Hypothesis. Unfortunately, there were insufficient observations to analyse provinces separately. This limitation of insufficient observations is not a major set-back since the EAR analyses were conducted to test the plausibility of the BMDL Hypothesis in Canada. Prior studies that have used the EAR strategy and have found support for the BMDL Hypothesis based on analyses of the US or Canada as a whole without the selection of only theoretically relevant states or provinces (Donohue and Levitt 2001, 2008; Sen 2007). This study, therefore, conducted the EAR analyses on all ten Canadian provinces.

The second set of analyses in this study were based on regressing age-specific crime rates on lagged abortion rates. Two sources of data were used: national age-specific crime rates and provincial age-specific crime rates. These data were directly linked with national and provincial abortion ratios (i.e., abortions per 100 live births), respectively. As with the EAR analyses, these age-specific analyses also benefitted from focusing on the 1988 change in abortion legislation. The analysis included 12-31 year olds in all available years of age-specific data (i.e., from 2006-11). These individuals were born in 1975 at the earliest. Actual abortion data were therefore available for all included individuals.

To perform a more direct test of the BMDL Hypothesis, four focal provinces were selected that demonstrated the largest increases in access to abortion services as well as the largest increases in abortions shortly after 1988. These provinces were British Columbia, Alberta, Ontario and Quebec. Equation 5 was estimated using only these four provinces to remove the suppressing effects that theoretically inappropriate provinces could add. According to the BMDL Hypothesis, provinces that did not experience large increases in abortions after 1988 could not be expected to also experience a decline in future crime rates from the influence of abortion. Their inclusion would dampen the impact an increase in abortions could have had on crime rates, which could potentially lead to false null findings. The four provinces were, therefore, aggregated together to perform the age-specific analyses on an improved, more theoretically informed, set of observations to supplement the national age-specific analyses. An additional advantage of analysing the four focal provinces was that more observations were included in the sample. The national rates included only one observation per age per year. The provincial data, however, included one observation per two-year age group per year per province. This increased the sample size, which assisted in detecting statistical patterns using more robust analyses.

Three models were estimated for age-specific crime rates for the national and provincial data sets. The equations for the first model included age and year fixed effects in the national analysis (5a) and also included a province fixed effect in the provincial analysis (5b) as follows,

(5a) $\ln(CRIME_{at}) = \beta_0 + \beta_1 ABORT_{t-a-1} + \gamma_a + \lambda_t + \epsilon_{ta},$

(5b) $\ln(CRIME_{at}) = \beta_0 + \beta_1 ABORT_{t-a-1} + \gamma_a + \lambda_t + \theta_i + \epsilon_{ta},$

where *CRIME* referred to the natural logarithm of either violent or property crime rates per 100 000 population of age group *a* in year *t*. *ABORT* was the abortion rate in the year prior to the birth of age group *a*. γ was the age fixed effect, λ was the year fixed effect, θ was the province fixed effect, and ϵ was the error term. *a*, *t*, and *i* indexed age group, year, and province respectively.

A one-year lagged dependant variable was added in the second model to correct for serial correlation in the crime data (Vieraitis, Kovandzic, and Marvell 2007). The equations for the national (6a) and provincial (6b) models were,

(6a) $\ln(CRIME_{at}) = \beta_0 + \beta_1 ABORT_{t-a-1} + \ln(CRIME_{at-1}) + \gamma_a + \lambda_t + \epsilon_{ta},$

(6b) $\ln(CRIME_{at}) = \beta_0 + \beta_1 ABORT_{t-a-1} + \ln(CRIME_{at-1}) + \gamma_a + \lambda_t + \theta_i + \epsilon_{ta},$

where *CRIME* referred to the natural logarithm of either violent or property crime rates per 100 000 population of age group *a* in year *t*. *ABORT* was the abortion rate in the year prior to the birth of age group *a*. γ was the age fixed effect, λ was the year fixed effect, θ was the province fixed effect, and ϵ was the error term. *a*, *t*, and *i*, indexed age group, year, and province, respectively. The lagged dependant variable term (i.e., $\ln CRIME_{at-1}$) improved the ability of the *ABORT* coefficient to estimate the specific impact of abortion rates on crime rates by removing the correlation between the crime rate of each year with the crime rate of the previous year. The age-specific national violent and property crime rates as well as the provincial violent and property crime rates exhibited high auto-correlation with their one-year lags.[26] The *ABORT* coefficient of Equation 6, therefore, offered an improved estimate of the association between abortion and crime rates in comparison to the *ABORT* coefficient estimates of Equation 5.

The effect of abortion liberalization was expected to affect different age groups in different years because the crime rates were age-specific. An age-year interaction term was added to Equation 6, following Donohue and Levitt (2001), to control for changes in age-specific trends in crime over time. This control allowed the *ABORT* coefficient to estimate the association between exposure to legalized abortion while *in utero* and criminality regardless of the age or year of observation by allowing the slopes of age-specific trends to vary individually over time. The year fixed effect dummy variables in Equation 6 were replaced with a single continuous year variable. This was necessary to prevent over-identification of the model as the national and provincial data sets included only 100 and 200 observations, respectively. The equations for the final national and provincial models were,

$$\text{(7a)} \quad \ln(CRIME_{at}) = \beta_0 + \beta_1 ABORT_{t-a-1} + \beta_2 \ln(CRIME_{at-1}) + \beta_3 YEAR_t + \beta_4(\gamma_a * YEAR_t) + \gamma_a + \epsilon_{ta},$$

$$\text{(7b)} \quad \ln(CRIME_{at}) = \beta_0 + \beta_1 ABORT_{t-a-1} + \beta_2 \ln(CRIME_{at-1}) + \beta_3 YEAR_t + \beta_4(\gamma_a * YEAR_t) + \gamma_a + \theta_i + \epsilon_{ta},$$

where *CRIME* referred to the natural logarithm of either violent or property crime rates per 100 000 population of age group *a* in year *t*. *ABORT* was the abortion rate in the year prior to the birth of age group *a*. $CRIME_{at-1}$ was the lagged dependant variable term for each age group *a* in year *t*. *YEAR* was a linear year trend term that replaced the year fixed effect in Equation 6 (i.e., λ). $\gamma * YEAR$ was the age-year interaction term. γ was the age fixed effect, θ was the province fixed effect, and ϵ was the error term. *a*, *t*, and *i*, indexed age group, year, and province, respectively.

[26] The Pearson correlation coefficients were 0.94, 0.98, 0.85, and 0.90, respectively.

Serial correlation and heteroskedasticity were expected to be issues in all of the data sets. The Durbin-Watson test statistic and the Breusch-Pagan test for heteroskedasticity revealed that first-order serial correlation and heteroskedasticity were indeed present. White robust standard errors were calculated for each model to correct for heteroskedasticity following Sen (2007). A one-year lagged dependant variable (i.e. $\ln CRIME_{it-1}$) was added to each model as an independent variable to correct for serial correlation (Vieraitis, Kovandzic, and Marvell 2007). The models that included lagged dependant variables were estimated to examine if the abortion coefficients (i.e. β_1) would change in response to this correction. Coefficient estimates from models with and without lagged dependant variables will be reported.

Other covariates were not added to the model for several reasons. Prior studies have not maintained a consistent set of control parameters and have, at times, used inappropriate proxies and measures (Moody and Marvell 2010). For instance, both Sen (2007) and Donohue and Levitt (2001) included beer consumption per capita in their models. The logic was never specified and presumably rested on the assumption that the presence of alcoholism in the home was a detrimental influence for children. There was no method for linking aggregate beer consumption per capita with the childhood environments of the cohorts included in the analyses, however, in either the construction of the beer consumption measure or in the specification of the model. The heavily controlled and parameterized models that have been characteristic of previous econometric analyses have also made regression models extremely burdened and complicated to interpret (Berk et al. 2003). This study did not include other covariates in any of the models to avoid these issues and to conduct a more direct test of the BMDL Hypothesis.[27]

[27] Other covariates that have been found to be statistically significant when included in similar analyses in prior research were added to the analyses conducted in the present study to check the robustness of the abortion coefficient estimates. When included, the abortion coefficient estimates did not change in direction and only slightly in magnitude. The results and substantive conclusions of the present study, therefore, remain robust to the inclusion or exclusion of other covariates. Please refer to Appendix D for a description of the covariates, a discussion of their derivation, and tables of complete results.

Chapter 4

4 RESULTS

4.1 Time Series Plots

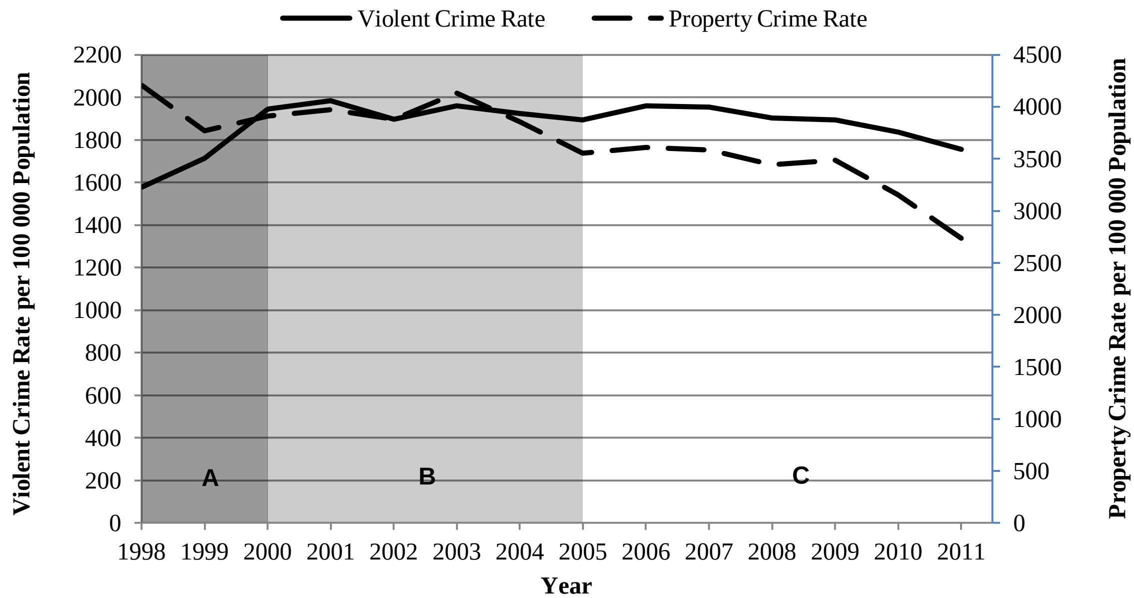
The consistency of the predictions of the BMDL Hypothesis with basic time series plots is an important prerequisite for the credibility of the BMDL Hypothesis (Joyce 2010). The first portion of this analysis, therefore, investigated the plausibility of the BMDL Hypothesis by plotting two types of crime rate measures: aggregate and age-specific crime rates.

4.1.1 *Plots of Aggregate Crime Rate*

Aggregate crime statistics are plotted first to assess the plausibility of the BMDL Hypothesis. These plots are similar to those used by Donohue and Levitt (2001, Figure II) to establish that the timing of the large declines in crime in the 1990s were consistent with the legalization of abortion in the early 1970s. In the US, crime rates began to decline in 1991, when the first cohort born after *Roe v. Wade* in 1973 would have been approximately 17 years of age. As affected cohorts (i.e., those born after 1973) progressed into their "high-crime" years, aggregate crime rates declined further. Since the 1988 liberalization of abortion in Canada, the first affected individuals would have only turned 23 years of age by 2011. The first set of time series plots will, therefore, present only youth crime rates (i.e., the crime rate of 12-17 year olds) to look for evidence for the BMDL Hypothesis. Although Donohue and Levitt (2001) focused on the crime rates of all ages, it is reasonable to assume that the reduced criminality of "wanted" children predicted by the BMDL Hypothesis should be evident first at younger ages.

Figure 4.1 presents the national violent and property crime rates of youth from 1998 to 2011 in Canada. These are the crime rates of individuals who were 12-17 years of age at the time the crime they were accused of was committed. The figures are shaded to represent the time periods when the 1988 liberalization of abortion could be expected to have an impact. Individuals who were 12-17 years of age between 1998 to 2000 (time period "A") were born between 1981-88 and, therefore, unaffected by the 1988 change in abortion legislation. Between 2001 and 2005, 12-17 year olds were born both before and after 1988, representing a “transition” period where exposure to liberalized abortion *in utero* was mixed in the 12-17 year old population (time period “B”). From 2006 on, 12-17 year olds were all born in 1989 or later, therefore having been exposed to liberalized abortion while *in utero* (time period “C”).

Figure 4.1: Youth Crime Rate per 100 000 Population, Age 12-17 Years, Canada, 1998-2011



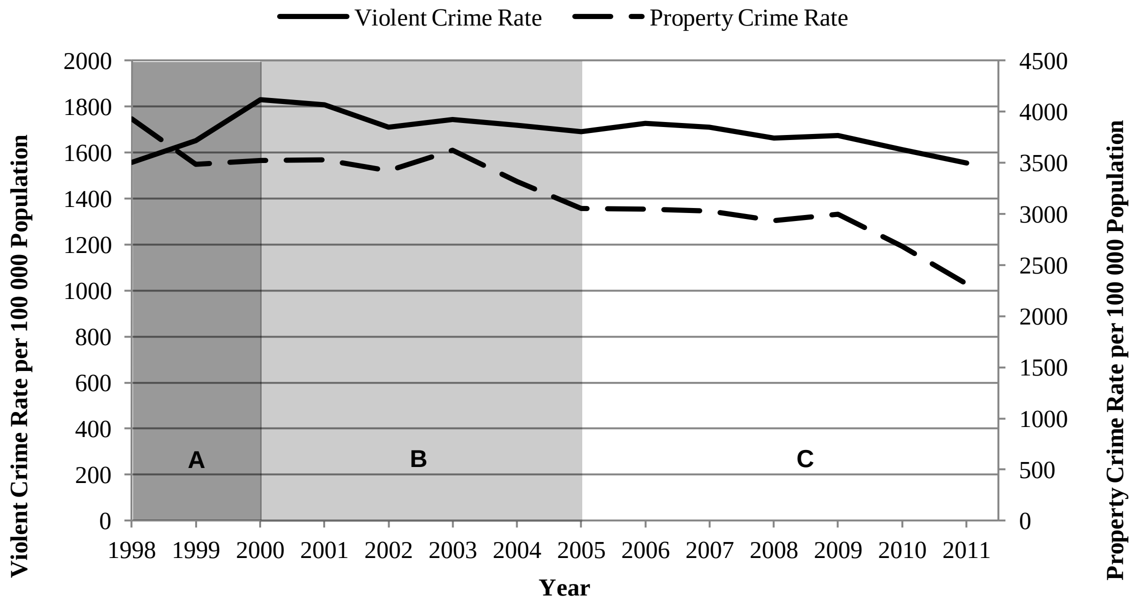
Source: Statistics Canada, Criminal Justice Statistics, Uniform Crime Reporting Survey

In theory, this time series plot would provide ideal evidence for the BMDL Hypothesis if crime rates were high in time period “A”, declined in “B”, and maintained a lower rate of crime in “C”. The national violent crime rates of youth did not change dramatically between the three time periods, increasing 0.8 percent in time period “B” (i.e., from 2000 to 2006) and declining 10.4 percent in time period “C” (i.e., from 2006 to 2011). The national property crime rates of youth did, however, decline 7.6 percent in time period “B” and declined 24.3 percent in time period “C”. Although youth property crime rates declined between 1998 and 2011, the start of major declines are slower than predicted by the BMDL Hypothesis. This pattern may, however, reflect lags in the implementation of the amendments to the *Canada Health Act* in 1988. It may have taken some time before abortion services were made available to women in medical facilities or for women to learn of the changes in the funding of abortions. Furthermore, abortion services were not equally accessible nationally. To investigate this possibility further, the crime rates of only those provinces that did experience large increases in abortions after 1988 are examined in Figure 4.2. Patterns should be clearer after removing the crime rates of provinces that did not experience large increases in abortions and are therefore not predicted by the BMDL Hypothesis to experience large declines in crime.

Figure 4.2 presents data similar to Figure 4.1, but for the four focal provinces of British Columbia (BC), Alberta (AB), Ontario (ON), and Quebec (QC) only. The increase in abortion rates around the 1988 liberalization of abortion were particularly pronounced in these four provinces (Figure 2.2) and are therefore expected to exhibit the largest declines in crime.

According to the BMDL Hypothesis, declines in youth crime rates should be more clearly evident in these focal provinces than for Canada as a whole. In these four provinces, however, trends in crime rates do not differ substantially from the declines in Figure 4.1. Property crime rates declined 13 percent in time period “B” and 24 percent in time period “C”. Violent crime rates declined 5.6 percent in time period “B” and 10.1 percent in time period “C”. Although declines in crime rates in the focal provinces are evident, they also begin later and are more gradually than the BMDL Hypothesis predicts. Barring minor fluctuations, the declines in crime rates in Figure 4.2 appear to be part of a larger trend of decline as opposed to a more sudden decline as predicted by the BMDL Hypothesis (Donohue 2008). Although these plots do not provide strong evidence to support the BMDL Hypothesis, they also do not conclusively contradict the theory.

Figure 4.2: Youth Crime Rate per 100 000 Population, Age 12-17 Years, BC AB ON QC, 1998-2011



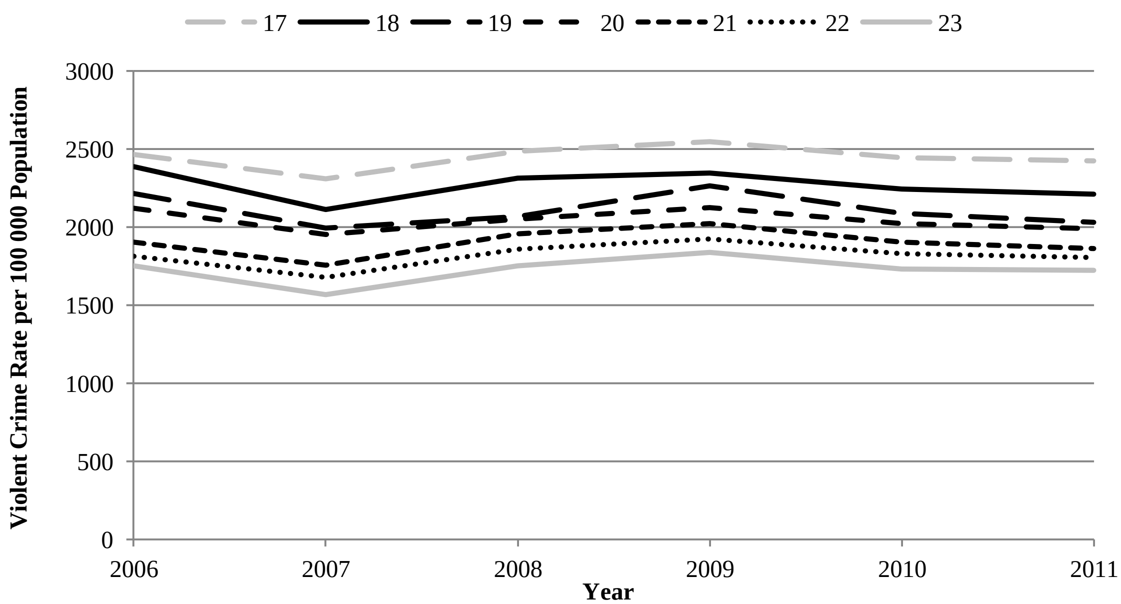
Source: Statistics Canada, Criminal Justice Statistics, Uniform Crime Reporting Survey

4.1.2 *Plots of Age-Specific Crime Rate*

To investigate the BMDL Hypothesis more effectively using time-series plots, crime rates by the age of the accused were plotted so that period and selection effects could be distinguished. The mechanism of influence purported by the BMDL Hypothesis predicts that declines in crime rates should occur in a very characteristic pattern. Declines in crime should be distinctly evident first in young age groups *before* they occur in successively older age groups to provide evidence of selection effects. This is because the exposure to legalized abortion while *in utero* follows affected cohorts as they age. The first affected cohort was born in 1989. Declines in crime should, therefore, be evident for 15 year olds in 2004, 16 year olds in 2005, 17 year olds in 2006, and so on. As the rates of abortion continued to increase after 1988, the crime rates of the aforementioned age groups should continue to decline as time passes. For instance, the crime rate of 15 year olds should be lower in 2005 than in 2004, the crime rate of 16 year olds should be lower in 2006 than in 2005, and so on. Investigating crime rates by age allows for the identification of the primary driving force behind changes in crime rates. That is, whether the trends in age-specific crime rates were experienced simultaneously by all age groups (i.e., period effects) or for particular age groups in a successive pattern (i.e., selection effects).

National age-specific crime rates (Figures 4.3 & 4.4) as well as the provincial crime rates in two-year age groupings for the focal provinces BC, AB, ON, and QC (Figures 4.5 & 4.6) are plotted. As discussed earlier, reliable age-specific data could only be obtained from 2006-11. Although this time frame is shorter than ideal, selection effects of the magnitude described by Donohue and Levitt (2001; 2008) should nonetheless be clearly evident in these time series plots.

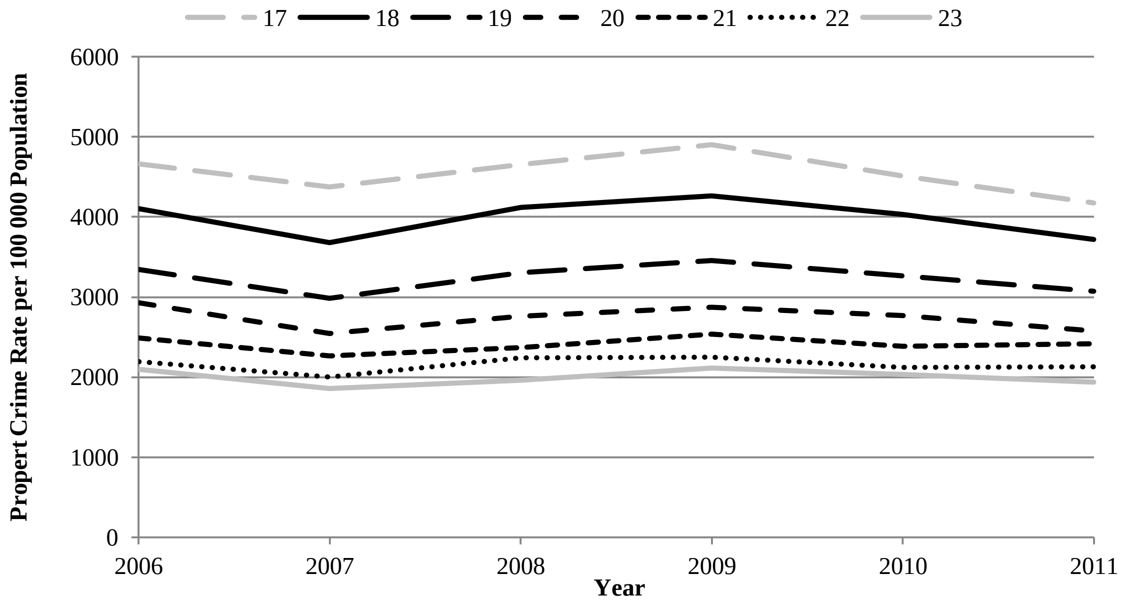
Figure 4.3: Violent Crime Rate per 100 000 Population, Age 17-23 by Single Year, Canada, 2006-2011



Source: Statistics Canada, Criminal Justice Statistics, Uniform Crime Reporting Survey, Custom Tabulation

In Figures 4.3 and 4.4, the crime rates of 17 and 23 year olds were included as comparison years of age to identify general period trends in crime. Between 2006-11, 23 year olds were not exposed to the 1988 liberalization of abortion while 17 year olds were exposed in all six years. Variation in the crime rates of 17 and 23 year olds are therefore not attributable to their *in utero* exposure to abortion. According to the BMDL Hypothesis, the crime rate of 18 year olds (i.e. born in 1989) should begin to decline in 2007, the crime rate of 19 year olds should begin to decline in 2008, the crime rate of 20 year olds should begin to decline in 2009, and so on. In both Figures 4.3 and 4.4, the age-specific crime rates do not deviate significantly from one another. Instead, the annual crime rates of 17-23 year olds generally vary in unison between 2006-11, which provides evidence of predominantly period rather than selection effects.

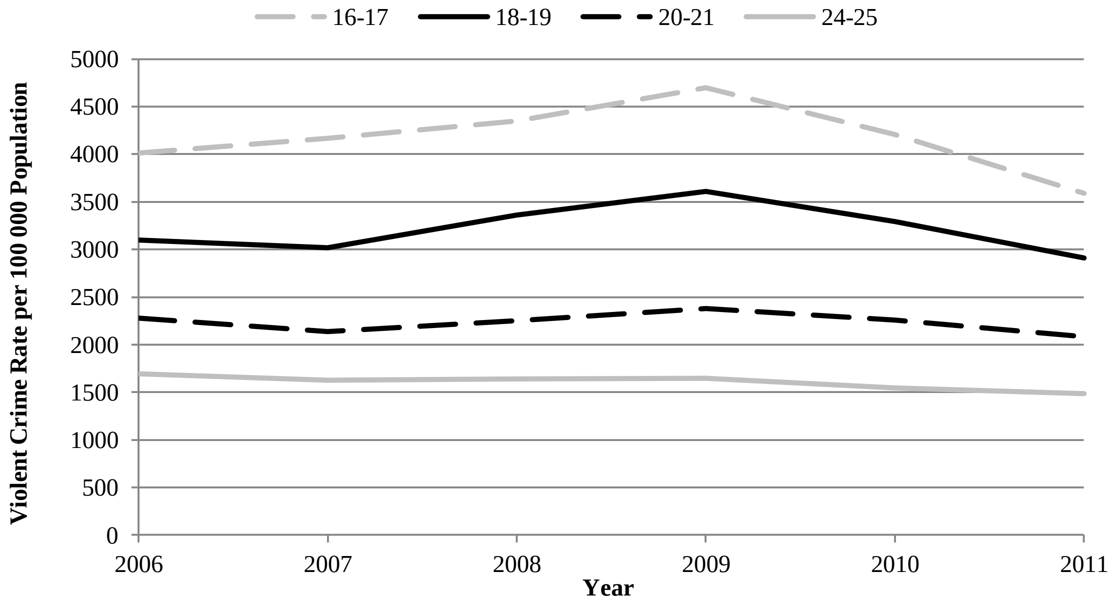
Figure 4.4: Property Crime Rate per 100 000 Population, Age 17-23 by Single Year, Canada, 2006-2011



Source: Statistics Canada, Criminal Justice Statistics, Uniform Crime Reporting Survey, Custom Tabulation

Figures 4.5 and 4.6 present similar data to Figures 4.3 and 4.4, but include the crime rates of BC, AB, ON, and QC only. Following the rationale for Figure 4.2, patterns of selection effect should be clearer in Figures 4.5 and 4.6 as the crime rates of the other six provinces and three territories of Canada were not included. Provincial data could only be obtained in two-year age groupings and, consequently, could not be plotted by single year of age as was done for the national crime rate plots. In Figures 4.5 and 4.6, the crime rates of 16-17 and 24-25 year olds were included as comparison ages as their exposure to liberalized abortion did not change between 2006-11. The crime rates of 22-23 year olds were not included as 22 year olds in 2011 would have been exposed to liberalized abortion while 23 year olds in 2011 would not have been exposed. According to the BMDL Hypothesis, the crime rate of 18-19 year olds should begin to decline beginning in 2007-08 and the crime rate of 20-21 year olds should begin to decline in 2009-10. As before, however, there is no evidence of distinct differences in the variation of crime rates between age groups, suggesting that selection effects are not influencing these crime rates. Instead, the year-to-year variation exhibits trends more consistent with period effects.

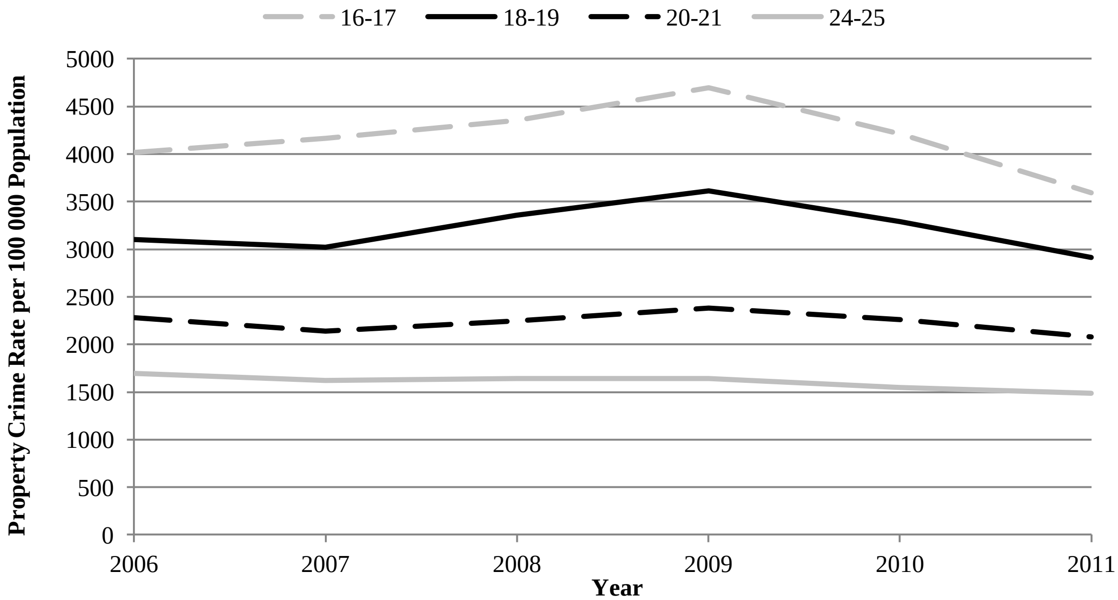
Figure 4.5: Violent Crime Rate per 100 000 Population, Age 16-25 Grouped, BC AB ON QC, 2006-2011



Source: Statistics Canada, Criminal Justice Statistics, Uniform Crime Reporting Survey, Custom Tabulation

Altogether, these time series plots do not provide compelling evidence for the BMDL Hypothesis. The trends in the plots of both aggregate and age-specific crime rates are more gradual than are predicted by the BMDL Hypothesis and instead appear to be driven primarily by period effects. Time series plots are not, however, conclusive tests by themselves. More sophisticated statistical analyses that are capable of directly linking crime rates to lagged abortion rates are therefore required for a more rigorous test of the BMDL Hypothesis.

Figure 4.6: Property Crime Rate per 100 000 Population, Age 16-25 Grouped, BC AB ON QC, 2006-2011



Source: Statistics Canada, Criminal Justice Statistics, Uniform Crime Reporting Survey, Custom Tabulation

4.2 Descriptive Statistics

Table 4.1 reports the means and standard deviations for the violent crime rates, property crime rates, and effective abortion rates employed in EAR analyses. The crime rates are reported in their original, unlogged units (i.e., rates per 100 000 population). The provincial values are the average of observations from each province across 14 years (i.e., 1998-2011). The yearly values are the average of observations from each year across all ten provinces. The overall means and standard deviations of violent crime rates, property crime rates, and effective abortion rates are reported in the final row of Table 4.1.

It is important to highlight the trends of the mean effective abortion rates in Table 4.1. In accordance with the reasons argued previously, the mean effective abortion rates for the four focal provinces are the highest values of all provinces. This provides further support for the use of BC, AB, ON, and QC to provide the most favorable source of data for testing the BMDL Hypothesis appropriately. A second trend worth noting is the increase of mean effective abortion rates of each year over time. As the proportion of 12-17 year olds who were born after the liberalization of abortion in 1988 increase, the EAR should also increase accordingly. By virtue of its construction (i.e., Equation 1), the EAR term should increase each year beginning in the year 2000. The values of the mean EAR by year reported in Table 4.2 evidences that over time, the changing magnitude of the influence of the liberalization of abortion is appropriately captured by the EAR term.

Table 4.1: Descriptive Statistics for Effective Abortion Rate Analyses

Province (N = 14)	**Youth Violent Crime Rate**		**Youth Property Crime Rate**		**Effective Abortion Rate**	
	Mean	**SD**	**Mean**	**SD**	**Mean**	**SD**
NL	2013.9	387.3	4454.6	621.9	9.1	3.9
PE	1463.0	450.2	3786.7	809.6	4.1	3.4
NS	2546.5	523.1	4836.4	513.4	15.9	2.1
NB	2258.1	338.2	4028.9	436.2	6.4	3.2
QC	1446.5	122.1	2271.6	370.8	22.9	5.9
ON	1714.5	91.2	2999.9	290.4	28.7	3.5
MB	2934.7	485.4	5287.5	683.6	16.8	3.4
SK	3552.4	679.1	9635.2	1053.4	9.6	2.5
AB	1974.9	223.5	5098.2	751.3	17.3	3.4
BC	1709.9	353.9	3797.6	975.7	28.5	2.0
Year (N = 10)	**Mean**	**SD**	**Mean**	**SD**	**Mean**	**SD**
1998	1532.6	408.5	4629.4	1551.4	12.3	8.5
1999	1693.5	469.8	4273.8	1440.7	12.4	8.6
2000	2040.3	565.4	4703.7	1895.8	12.6	8.7
2001	2203.0	694.0	4883.8	2095.0	13.0	8.8
2002	2159.2	713.1	4742.8	2089.5	13.4	9.0
2003	2287.2	763.7	5112.0	2288.6	14.1	9.3
2004	2129.3	839.1	4871.1	2333.2	14.9	9.4
2005	2205.2	776.4	4600.6	2210.0	15.8	9.3
2006	2417.9	848.6	5008.8	2386.3	16.8	9.2
2007	2450.6	936.4	4851.1	2392.5	17.6	9.1
2008	2436.7	856.2	4625.1	2111.2	18.7	8.7
2009	2323.5	796.5	4535.2	2085.4	19.7	8.5
2010	2268.6	839.8	4181.1	1984.2	20.6	8.5
2011	2112.6	737.3	3656.8	1991.0	21.3	8.6
Total (N = 140)	2161.4	755.6	4619.7	2012.9	15.9	9.0

Note: Crime rates are per 100 000 population (i.e., before logging). The "N" reported for the Province and Year are the number of observations for each Province and Year category listed below.

Table 4.2 reports the means and standard deviations of the national crime and abortion rates employed in the age-specific analyses. The crime rates are again reported in their original, unlogged units. The unit of the abortion ratio is the ratio of abortions per 100 live births. The means are reported by single year of age across all six year observations and by year across all 20 age observations. The overall means and standard deviations of violent crime rates, property crime rates, and abortion ratios for the entire sample are reported in the final row of Table 4.2.

Table 4.2: Descriptive Statistics for National Age-Specific Crime Rate Analyses

Age (N = 6)	Violent Crime Rate		Property Crime Rate		Abortion Ratio	
	Mean	**SD**	**Mean**	**SD**	**Mean**	**SD**
12	728.2	48.3	1234.1	162.3	29.6	2.3
13	1224.0	67.6	2219.8	241.5	28.5	2.4
14	1714.6	79.8	2626.8	321.5	27.1	2.4
15	2159.6	77.5	4384.4	367.8	25.9	2.2
16	2368.5	102.5	4740.0	345.0	24.5	2.8
17	2445.0	79.0	4548.4	252.8	23.1	3.0
18	2268.8	100.2	3986.7	235.6	21.8	2.7
19	2109.6	106.2	3238.5	176.3	20.6	2.1
20	2043.7	69.5	2742.3	153.2	19.8	1.7
21	1900.5	89.3	2408.9	96.5	19.0	.7
22	1818.1	80.0	2155.5	92.5	18.7	.3
23	1727.4	88.6	1999.6	98.3	18.9	.6
24	1656.4	66.2	1890.1	99.7	18.9	.7
25	1653.5	82.4	1862.2	68.1	19.1	.7
26	1538.3	81.5	1754.5	55.8	19.2	.6
27	1506.7	78.0	1667.0	42.7	19.2	.6
28	1446.5	65.5	1611.1	46.6	18.9	1.3
29	1398.9	57.9	1514.2	42.1	18.2	1.5
30	1433.9	82.2	1540.1	80.5	17.5	1.8
31	1316.7	57.2	1376.5	54.3	16.8	1.7
Year (N = 20)	**Mean**	**SD**	**Mean**	**SD**	**Mean**	**SD**
2006	1762.6	445.6	2641.3	1193.3	19.6	3.1
2007	1616.7	410.4	2437.3	1113.4	20.2	3.4
2008	1756.6	439.8	2584.9	1209.7	20.9	3.7
2009	1795.0	445.4	2644.1	1247.8	21.6	4.1
2010	1708.7	421.6	2469.7	1123.3	22.4	4.5
2011	1698.0	417.0	2345.6	972.8	23.0	4.9
Total (N = 120)	1722.9	425.1	2520.5	1128.1	21.3	4.1

Note: Crime rates are per 100 000 population (i.e., before logging). Abortion ratios are per 100 live births. The "N" reported for the Age and Year are the number of observations for each Age or Year category that is listed below.

The mean abortion ratio of the year prior to the year of birth of each year of age is highest for 12 year olds and generally declines for each older year of age. Mean age-specific violent crime rates increase from 12 year olds to a maximum for 17 year olds, declining in each subsequent year of age until 31 year olds. Mean age-specific property crime rates are highest for 16 year olds, declining in both younger and older ages. Prior studies have maintained that the "high crime" years are between ages 15-25 (e.g., Donohue and Levitt 2001). These data suggest, however, that the highest mean crime rates are more tightly clustered around younger ages, specifically around ages 16-17.

Although there is some evidence of yearly variation in crime rates, reaching a high-point in 2009, they are relatively stable across all six years. The abortion ratio, however, steadily increases over time as expected as more of the individuals included in the sample are exposed to liberalized abortion over time.

Table 4.3 reports the means and standard deviations of crime and abortion rates from the provinces BC, AB, ON, and QC, which were employed in the provincial age-specific analyses. As in Table 4.2, the crime rates are reported in their original, unlogged units. The unit of the abortion ratio is the ratio of induced abortions per 100 live births. Means are reported by two-year age group across all six year observations and across all four provinces. The means of each year are also reported by year across all ten age group observations and across all four provinces. The means of each province are also reported across all ten age group observations across all six years. The overall means and standard deviations of violent crime rates, property crime rates, and abortion ratios are reported in the final row of Table 4.3.

The trends in Table 4.3 are very similar to the trends reviewed in Table 4.2. As in Table 4.2, the abortion ratio is highest in the youngest age group (i.e., 12-13 year olds) and generally declines for older age groups. Also, the mean violent and property crime rates peak in the 16-17 age group and decline in both younger and older ages as they did in Table 4.2. The yearly mean crime rates in Table 4.3 also reach a maximum in 2009, but again demonstrate relative stability across the six years. The abortion ratio also steadily increased from 2006 to 2011. It is important to note, however, the differences between Table 4.3 and Table 4.2 in abortion ratios. All of the mean abortion ratios reported in Table 4.3 are larger in magnitude than their corresponding values in Table 4.2. As argued previously, these four provinces were selected based on the increase in access to abortion services that they experienced after 1988. This is a critical improvement for the proper testing of the BMDL Hypothesis. Table 4.3 provides evidence that the use of the four focal provinces, namely BC, AB, ON, and QC, constitutes a more direct test by only including areas that did in fact experience increases in abortions following an increase in access to abortion services.

Table 4.3: Descriptive Statistics for Provincial Age-Specific Crime Rate Analyses

Age (N = 24)	Violent Crime Rate		Property Crime Rate		Abortion Ratio	
	Mean	SD	Mean	SD	Mean	SD
12-13	828.4	260.0	1469.1	615.6	30.0	4.7
14-15	1733.1	468.2	3560.7	1314.1	27.3	4.7
16-17	2150.9	585.3	4243.0	1607.6	24.7	5.1
18-19	1968.7	556.1	3338.0	1203.8	22.2	5.1
20-21	1773.0	489.9	2356.7	921.3	20.5	4.5
22-23	1603.6	446.5	1889.0	758.6	19.9	4.9
24-25	1502.8	407.9	1721.3	699.5	20.4	5.7
26-27	1393.4	379.0	1591.9	603.7	21.2	6.8
28-29	1313.9	360.9	1500.1	591.2	21.1	7.8
30-31	1285.8	338.8	1414.9	519.1	19.7	8.5
Year (N = 40)	**Violent Crime Rate**		**Property Crime Rate**		**Abortion Ratio**	
	Mean	**SD**	**Mean**	**SD**	**Mean**	**SD**
2006	1428.2	709.2	2196.3	1530.1	21.2	6.8
2007	1432.9	625.6	2234.4	1504.5	21.7	6.6
2008	1601.1	506.2	2439.5	1359.0	22.3	6.5
2009	1686.8	516.2	2530.1	1373.7	23.1	6.5
2010	1640.1	504.5	2341.0	1265.3	23.7	6.7
2011	1542.9	454.1	2109.6	1071.3	24.3	6.9
Province (N = 60)	**Violent Crime Rate**		**Property Crime Rate**		**Abortion Ratio**	
	Mean	**SD**	**Mean**	**SD**	**Mean**	**SD**
BC	1185.0	519.4	1648.8	853.0	29.3	2.6
Alberta	2091.5	504.7	3602.7	1460.6	17.1	3.3
Ontario	1501.6	437.3	2202.6	1166.1	24.9	3.8
Quebec	1443.3	342.2	1779.7	866.8	19.6	7.7
Total (N = 240)	1555.4	562.2	2308.5	1352.8	22.7	6.7

Note: Crime rates are per 100 000 population (i.e., before logging). The "N" reported for the Age, Year, and Province are the number of observations for each Age, Year, and Province category that is listed below.

In Table 4.3, it is interesting to note that BC experienced the lowest overall mean violent and property crime rate and the highest overall mean abortion ratio. Alberta, on the other hand, experienced the highest mean violent and property crime rate and the lowest mean abortion ratio. Ontario and Quebec experienced both crime rates and abortion ratios intermediate to BC and Alberta. The BMDL Hypothesis predicts that higher abortion ratios should be associated with lower crime rates while lower abortion ratios be associated with higher crime rates. These values reported in Table 4.3 suggest the plausibility of the BMDL Hypothesis in these four provinces. Further statistical analyses are, however, required to investigate this preliminary pattern.

4.3 Effective Abortion Rate (EAR) Analyses

The next two sections report the results of the statistical analyses that were conducted to investigate changes in crime rates while incorporating changes in abortion rates. These analyses constitute an improvement to the time-series plots presented above as the variation in abortion rates is used to predict the variation in crime rates.

Table 4.4 reports the estimates of the coefficient of the *EAR* term (i.e., β_1) in Equation 4. The model is estimated for the annual violent and property crime rates of youth offenders (i.e., 12-17 years of age) from all ten Canadian provinces. A one-year lagged dependant variable term (i.e., $\ln CRIME_{it-1}$) is added to the model to correct for serial correlation and the adjusted coefficient of the *EAR* term (i.e., β_1) is reported beneath the original *EAR* coefficients. Robust standard errors are reported in parentheses beneath coefficient estimates.

To review, these EAR analyses focus on annual youth crime rates and construct an "effective abortion rate" for each year of crime. The EAR is the average exposure to abortion that 12-17 year olds experienced in each year. This average is calculated by weighting the abortion rate that each cohort experienced while *in utero* by the proportion of the 12-17 year old population that each age cohort constitutes for each year.

The results of the EAR analyses provide little support for the BMDL Hypothesis. To provide evidence for the hypothesis, all of the *EAR* coefficient estimates in Table 4.4 should be negative. This would suggest that an increase in the effective abortion rate would be associated with a decline in crime rate. The EAR analyses, however, estimate *EAR* coefficients that are generally not of the expected negative sign. The sole coefficient that was negative (i.e., the property crime model without a lagged dependant variable) was not statistically significant at the $p \leq 0.05$ level. The only *EAR* coefficient that is statistically significant at the $p \leq 0.05$ level is from the violent crime model after a lagged dependant variable was added. This coefficient estimates that an increase in the *EAR* of one abortion per 100 live births is associated with a 1.4 percent *increase* in the youth crime rate, which directly contradicts the BMDL Hypothesis.

Table 4.4: Analyses of the Relationship Between the Effective Abortion Rate and Youth Crime Rates in Canada, 1998-2011

	ln Violent Crime	**ln Property Crime**
EAR	.014	-.003
(N = 140)	(.012)	(.010)
***EAR* + lagDV**	.014*	.008
(N = 130)	(.006)	(.004)

* $p \leq .05$; ** $p \leq .01$; *** $p \leq .001$
Note: Robust standard errors are reported in parentheses beneath their respective coefficients. Please refer to Appendix C for a complete set of model outputs.

Although the results Table 4.4 provide little support for the BMDL Hypothesis, the strategy relies on strong assumptions of the influence of abortion rates on cohorts due to the construction of the EAR term. The EAR strategy, therefore, lacks the ability to directly link the actual abortion rates experienced by cohorts with their crime rates because it relies on aggregate crime rates and averaged abortion rates. To perform a more direct test of the BMDL Hypothesis, age-specific crime rates are used to directly identify cohorts and link their criminality with the abortion rates that they experienced while *in utero*.

4.4 Age-Specific Crime Rate Analyses

Table 4.5 reports the regressions of lagged abortion rates on age-specific crime rates. The results of the three age-specific national and provincial crime rate models discussed in section 3.2 are reported in the six rows. Only the estimates of the coefficient of the *ABORT* term (i.e., β_1) from Equations 5-7 are reported. The *ABORT* coefficient estimates from the national models are reported in Panel A while the estimates from the provincial models that included only BC, AB, ON, and QC are reported in Panel B. Robust standard errors are reported in parentheses beneath their respective coefficients.

To review, Equation 5 linked the crime rate of cohorts with the abortion rate of the year prior to the birth of that cohort. Age, year, and provincial fixed effects were included to control for time invariant sources of heterogeneity. To improve the model estimated by Equation 5, a one-year lag of crime was included in Equation 6 to correct for autocorrelation in crime data. Equation 6 was further refined with the addition of an age-year interaction term to control for changes in patterns of age-specific crimes over time (Equation 7).

Table 4.5: Analyses of the Relationship Between Lagged Abortion Rates and Age-Specific Crime Rates of Individuals Aged 12-31 in Canada, 2006-2011

Panel A		**National Analyses**	
Model	**Coefficient** (N)	**ln Violent Crime**	**ln Property Crime**
Equation 5a	***ABORT***	-.012***	-.013**
	(N = 120)	(.002)	(.004)
Equation 6a	***ABORT***	-.009**	-.015***
(LDV)	(N = 100)	(.003)	(.003)
Equation 7a	***ABORT***	-.000	-.008
(LDV + Interaction)	(N = 100)	(.012)	(.013)
Panel B		**BC, AB, ON, and QC Analyses**	
Model	**Coefficient** (N)	**ln Violent Crime**	**ln Property Crime**
Equation 5b	***ABORT***	.006	.008
	(N = 240)	(.005)	(.005)
Equation 6b	***ABORT***	.007**	.003
(LDV)	(N = 200)	(.002)	(.002)
Equation 7b	***ABORT***	.009**	.005*
(LDV + Interaction)	(N = 200)	(.003)	(.003)

* $p \leq .05$; ** $p \leq .01$; *** $p \leq .001$
Note: Robust standard errors are reported in parentheses beneath their respective coefficients. Please refer to Appendix C for a complete set of model outputs.

For both national violent and property crime (Table 4.5, Panel A), the base model (i.e., Equation 5a) estimates *ABORT* coefficients that are negative and statistically significant. When the lagged dependant variables are added in Equation 6a, the coefficient estimates maintain similar magnitudes as well as similar statistical significance. These four estimates provide support for the BMDL Hypothesis, suggesting that exposure to higher abortion rates while *in utero* is associated with lower rates of violent and property crime. When the age-year interaction term is added in Equation 7a, however, both of the *ABORT* coefficients in the violent and property crime models maintain a negative sign, but decline in magnitude and lose statistical significance at the $p \leq 0.05$ level. The estimates of the impact of abortion rates on age-specific crime rates, therefore, decline in magnitude and statistical significance as the model is refined from Equation 5 to Equation 7.

Unlike the national models, the results of the provincial analyses (Table 4.5, Panel B) fail to provide evidence for the BMDL Hypothesis. The base model (i.e., Equation 5b) estimates *ABORT* coefficients that are positive but not statistically significant at the $p \leq 0.05$ level. When the lagged dependant variable is added to the model in Equation 6b to correct for serial correlation, the *ABORT* coefficient in the violent crime model reaches statistical significance at the $p \leq 0.01$ level, but remains positive. Finally, in Equation 7b, which adds age-specific trend terms to the model, both of the *ABORT* coefficients from the violent and property crime estimates are statistically significant and remain positive. These analyses rely on data from the four focal provinces that constitute the most theoretically-driven test of the BMDL Hypothesis. The national crime and abortion rates from Panel A include provinces that experienced little to no increases in access to abortion services and should not be relied on to test the BMDL Hypothesis effectively. The morels in Panel B, particularly from Equation 7b, should therefore be considered the more conclusive tests of the BMDL Hypothesis conducted in this study. The results of Equation 7b along with the sum of the results of Panel B suggest that increases in abortion rates are associated with an *increase* in crime, which directly contradict the predictions of the BMDL Hypothesis.

Altogether, the results of these analyses cast some doubt on the BMDL Hypothesis. The time-series plots found that trends in Canadian crime rates were predominantly driven by period effects rather than from the selection effects predicted by the BMDL Hypothesis. The EAR analyses, which have been used in the past to find support for the BMDL Hypothesis, failed to provide supporting evidence when the strategy was performed on a new, exogenous source of variation in abortion rates. The age-specific analyses, which are arguably the most cogent tests performed in this study, generally fail to provide convincing support the BMDL Hypothesis. While some support is observed for the BMDL Hypothesis in the national age-specific analyses, it is surprising that the provincial analyses, which benefitted from more theoretically direct data, a larger sample size, and greater variation in both the dependant and independent variables, failed to provide support for the BMDL Hypothesis. Although limitations in the execution of this study do exist, the findings overall provide little support for the BMDL Hypothesis and lead one to be skeptical of its validity. The increase in abortion rates that occurred after the 1988 liberalization of abortion in Canada does not, therefore, appear to be associated with a decline in either rates of violent or property crime.

Chapter 5

5 DISCUSSION AND CONCLUSION

5.1 Discussion

The purpose of this study was to test the BMDL Hypothesis on a new source of data by taking advantage of the liberalization of abortion that occurred in Canada in 1988. Thus far, the American literature that has surrounded the BMDL Hypothesis has been hampered by issues of circular logic. The BMDL Hypothesis was generated to explain the dramatic decline in crime that occurred in the US in 1990s and, therefore, should not be tested by investigating the same crime rates of the 1990s that motivated the development of the BMDL Hypothesis (Zimring 2007). The primary improvement that this study offered to the literature was to focus on a new intervention in abortion legislation, namely the 1988 liberalization of abortion services in Canada, and to then look for evidence of an impact in a different set of crime rates.

To situate the results of this analysis, it would be beneficial to review the main tenets of the BMDL Hypothesis. The BMDL Hypothesis argues that the legalization of abortion increases access to abortion services by lowering the social and financial costs of obtaining an abortion. This change in access is particularly important for women who are socioeconomically disadvantaged, allowing them the ability to more easily abort unwanted pregnancies. Had these unwanted pregnancies come to term, unwanted children would have been born into adverse environments influenced by both socioeconomic disadvantage and the effects of being an unwanted child. Children born into such environments are disproportionately more likely to be involved in criminal activity. The BMDL Hypothesis predicts, therefore, that individuals born after the legalization of abortion, or more accurately, after an increase in the availability of abortion services, will exhibit lower rates of criminality. More explicitly, the BMDL Hypothesis predicts that birth cohorts that experienced higher rates of abortions while *in utero* (i.e., the year prior to their birth), should be involved in less criminal activity, particularly during their high-crime years between 15-25 years of age. According to the BMDL Hypothesis, then, the increase in abortion rates that followed the liberalization of abortion services in Canada in 1988 should have caused a decline in crime rates in the 1990s and 2000s for those cohorts born after 1988. To investigate the plausibility of the BMDL Hypothesis, three of the best strategies that have emerged in the literature were selected and employed in this study: time series plots of crime rates, "effective abortion rate" analyses, and age-specific crime rate analyses.

5.1.1 *Time Series Plots*

Many critics have been skeptical of the credibility of the BMDL Hypothesis because it has been found to be inconsistent with time series plots of American crime rates (e.g., Lott and Whitley 2007; Zimring 2007; Joyce 2010). Donohue and Levitt (2004), on the other hand, have argued that the crack-cocaine epidemic that occurred during the late 1980s and early 1990s had a such a dramatic impact on crime trends that the time series patterns predicted by the BMDL Hypothesis have been hidden and obscured in simple plots of crime rates. Time series plots of crime rates from this period were, therefore, argued by Donohue and Levitt (2004) to be ineffective for visually investigating the BMDL Hypothesis.

To avoid this issue, this study examined youth crime rates from 1998 to 2011; a time frame that was not influenced by the crack-cocaine epidemic. Furthermore, the four provinces (BC, AB, ON, and QC) that experienced the largest increases in the availability of abortion services after 1988 were investigated separately. According to the BMDL Hypothesis, time series plots of youth crime rates including only these four provinces should display patterns that are consistent with the BMDL Hypothesis more clearly. The time series plots of aggregated Canadian youth crime rates (Figures 4.1 and 4.2) revealed declines in both violent and property crime rates. These trends were, however, slower and more gradual than predicted by the BMDL Hypothesis, even when the youth crime rates of only the four focal provinces were examined. Instead, the declines in Canadian youth crime rates appear to be part of a larger trend of decline that was not associated with a particular "shock" as predicted by the BMDL Hypothesis (Donohue 2008).

Time series plots of age-specific crime rates have also been critical in the academic debate surrounding the BMDL Hypothesis because it predicts a selection effect that presents in a very characteristic, cascading pattern in time series plots of age-specific crime rates (Joyce 2010). That is, the crime rates of age groups are predicted to decline in succession as each age group includes individuals born after 1988. The time series plots produced in this study (Figures 4.3 to 4.6) do not, however, provide evidence of such selection effects. The trend lines of age-specific crime rates in Figures 4.3 to 4.6 vary in unison, providing evidence of period effects rather than selection effects.

The results of the time series plots of Canadian crime rates produced in this study were similar to time series plots from prior American research (e.g., Lott and Whitley 2007; Joyce 2010) in that period effects seemed to be the predominant driving force behind trends in crime. In the US, these period influences have been identified as changes in policing and incarceration practices, demographics, the economy, and the decline of the crack-cocaine epidemic, with varying degrees of influence and contribution (Levitt 2004; Zimring 2007). Excluding the relatively acute influence of the crack-cocaine epidemic, these explanations were more gradual, long-term processes that influenced American crime trends in the 1990s (Donohue 2008). In Canada, similarly gradual explanations have been posited to influence crime rates, including the economy, alcohol consumption, policing legislation, and changing demographics (Bunge, Johnson, and Balde 2005). The aggregate time series plots suggested that the recent declines in crime have been part of a larger pattern of decline. The age-specific time series plots also suggest that more gradual period effects, rather than abrupt selection effects, have been predominantly influencing crime trends. The time series plots produced in this study generally support the explanation that broader and more gradual demographic, socioeconomic, and policing-related period effects have been predominantly influencing crime trends.

The time series plots produced in this study, combined with previous time series plots from past research, fail to provide convincing evidence to support the BMDL Hypothesis. To supplement the time series plots, statistical analyses were conducted to take into account the changing rate of abortions that occurred in Canada after 1988.

5.1.2 *Effective Abortion Rate (EAR) Analyses*

To review, the “effective abortion rate” (EAR) was a constructed variable that captured the average exposure to abortion rates that was experienced by each youth cohort for each year of crime data. As only 23 years had elapsed from 1988 to 2011, the EAR analyses focused on annual youth crime rates. This strategy was originally devised by Donohue and Levitt (2001) and was used by Sen (2007) in the sole Canadian examination of the BMDL Hypothesis. In both studies, the EAR strategy produced results in support the BMDL Hypothesis. The EAR strategy was, therefore, replicated to test the plausibility of the BMDL Hypothesis using the 1988 liberalization of abortion services in Canada as a new focal intervention. Although this study only examined youth crime rates while prior applications of the EAR strategy examined total population crime rates, differences in the criminality of those born before and after 1988 should still be evident in this younger age sample.

The results of the EAR analyses unanimously failed to support the BMDL Hypothesis. Only one of four estimated coefficient was found to be statistically significant, but was in the opposite direction from that predicted by the BMDL Hypothesis. This result suggests that increases in the effective abortion rate are associated with an increase in the youth violent crime rate. It is important to note that the modifications of the *EAR* term used in this study still produced values that increased from 1998 to 2011 (Table 4.1), which provides evidence that the construction of the *EAR* term correctly took into account the increase in abortion rates that followed the 1988 liberalization of abortion services. The lack of a negative association, therefore, should not be attributed to the modified construction of the *EAR* term.

The EAR strategy has been employed in prior research to find support for the BMDL Hypothesis in the US and Canada (e.g., Donohue and Levitt 2001; Sen 2007). Contrary to prior applications of this strategy, however, the results of the EAR analyses performed in the present study failed to provide support for the BMDL Hypothesis. The strong assumptions involved in the construction of the *EAR* term, however, make the EAR strategy as whole a weaker and less direct method for testing the BMDL

Hypothesis. Nevertheless, the effective abortion rate was not found to be negatively associated with crime rates.

5.1.3 *Age-Specific Crime Rate Analyses*

The second statistical strategy employed in this study was regression analyses of age-specific crime rates and lagged abortion rates. This strategy has been important in the literature as it was the main strategy that Donohue and Levitt (2001; 2008) relied on to find evidence to support the BMDL Hypothesis. To review, the crime rate of a cohort was linked with the abortion rate of the year prior to the birth of that cohort. Regression analyses were conducted on national crime and abortion rates and on provincial crime and abortion rates of the four focal provinces (BC, AB, ON, and QC). To provide evidence to support the BMDL Hypothesis, these analyses should have estimated negative and statistically significant coefficients.

Four of the six coefficient estimates of the national age-specific analyses were found to be both negative and statistically significant providing evidence in support of the BMDL Hypothesis. That is, exposure to higher abortion rates while *in utero* was associated with lower rates of both violent and property crime. In Equation 7a of the national age-specific analyses, however, an age-year interaction term was added to the model to control for changes in patterns of age-specific crimes over time. When this interaction term was added, the coefficient estimates for both violent and property crime lost statistical significance and declined in magnitude. It is interesting to note that as the model was progressively improved from Equation 5a to 7a, the coefficient estimates declined in magnitude and lost statistical significance.

Unlike the results of the national analyses, the coefficient estimates of the provincial age-specific analyses contradicted the predictions of the BMDL Hypothesis. Three of the six coefficient estimates from the provincial age-specific crime rate analyses were found to be statistically significant, but none were negative. These results suggested that exposure to higher abortion rates while *in utero* was associated with increases in both violent and property crime rates. The coefficient estimates also increased in statistical significance from Equation 5b to 7b, which was the opposite trend found in the national analyses. That is, as the provincial models were refined with the addition of a lagged dependant variable and an age-year interaction term, the coefficient estimates generally increased in positive magnitude and gained statistical significance. As the provincial analyses also benefitted from more theoretically direct data, a larger sample size, and greater variation in both the dependant and independent variables, it is surprising that these results directly contradicted the results of the national analyses and the BMDL Hypothesis.

To interpret these conflicting results, it is important to take stock of the theoretical rationale that motivated the age-specific crime rate analyses. The national measures of abortion and crime rates were highly aggregated and included data from all thirteen Canadian provinces and territories, most of which did not experience a large increase in access to abortion services after 1988. To improve the theoretical precision of the age-specific crime rate analyses, the models were refined (i.e., from Equation 5 to 7) and the provinces and territories that did not experience increases in access to abortion services or large increases in abortion rates following 1988 were removed. Four focal provinces were, therefore, selected on the basis that they constituted the most theoretically direct set of data to test the BMDL Hypothesis. The BMDL Hypothesis predicts that the coefficient estimates of the provincial analyses should be larger in magnitude than the estimates of the national analyses and that all coefficient estimates should be negative.

The results of the age-specific analyses contradict this theoretical rationale; the coefficient estimates from the national analyses were negative and the coefficients from the provincial analyses were positive. Under the theoretical assumption that the modeling procedure was improved from Equation 5 to 7 and from national to provincial analyses, one interpretation of these conflicting results is that increases in abortion rates were actually associated with *increases* in crime rates. The national analyses included all Canadian provinces, including those that did not experience an increase in access to abortion services. Since the availability of abortion services did not increase in provinces other than BC, AB, ON, and QC, their influence may have dampened the overall positive association between abortion and crime. Consequently, the national analyses may have falsely estimated negative coefficients due to this "contamination" of the data. The provincial analyses, which included only provinces that experienced increases in access to abortion services, estimated coefficients that were generally positive. Since these analyses constituted a more direct test of the impact of abortion on crime, they were able to reveal the "true" positive association between abortion and crime. Contrary to the predictions of the BMDL Hypothesis, therefore, the results of these age-specific analyses suggest that increases in access to abortion services are associated with increases in crime rates.

This interpretation is consistent with the findings of Lott and Whitley (2007), in which increases in abortion rates were associated with increases in murder rates. Based on Akerlof et al (1996), Lott and Whitley (2007) argued that the legalization of abortion in the US increased out-of-wedlock childbearing due to a decline in "shotgun marriages." Joyce (2010:470) explains that, "The availability of safe, legal abortion allows men to insist that women terminate a pregnancy instead of offering marriage. Women unwilling to terminate their pregnancies are more likely to raise the child alone. The impoverishment of women reduces investment in their children's human capital, which leads to later increases in crime." If this theory is true, it may constitute the mechanism through which an increase in the availability of abortion services is associated with increases in crime rates.

Kahane et al (2008) replicated Donohue and Levitt's (2001) study in the UK and found evidence contrary to the predictions of the BMDL Hypothesis as well. Kahane et al (2008) performed EAR analyses that found that increases in the abortion rate were associated with declines in property crime rates, but *positively* associated with violent crime rates. They also performed age-specific crime rate analyses and found that increases in the abortion rate were positively associated with total crime rates. They rationalized their results using two possible explanations. First, they hypothesized that the legalization of abortion in the UK did, in fact, reduce crime rates, but that other unmeasured factors were obscuring their results. This would primarily be due to the difficulties of asserting a causal link between abortion rates and crime rates that occurred approximately 20 years apart with many other socioeconomic changes occurring concomitantly.

Second, they hypothesized that the legalization of abortion was not associated with crime rates at all, and the results of Donohue and Levitt's (2001) were spurious and attributable to omitted variable bias. For instance, they cited evidence from Finer and Henshaw (2006) that in 2001, the abortion ratio for women in poverty and for women with less than a high school education (or "at risk" women) were *lower* than for women with higher incomes and college degrees in the US. Income and education may have influenced both the propensity of women to terminate an unwanted pregnancy and the likelihood of their children to engage in criminal activity. The authors argued that other measures like income and education that influenced the variation in both abortion and crime rates must be identified and explored to satisfactorily investigate the BMDL Hypothesis.

Overall, however, Kahane et al (2008) concluded that the mixed results of their analyses highlighted the difficulties of testing a causal link between abortion rates and crime rates that occurred 15 to 25 years apart. They argued that to conclusively substantiate the BMDL Hypothesis, it would be necessary to identify omitted variables and to more closely examine the mechanisms of change that linked abortion and crime rates within changing macro-social contexts. The myriad of social factors that influenced both abortion and crime rates between the late 1960s to the early 2000s has made the BMDL Hypothesis both difficult to study and substantiate. Although the limitations in this line of research were identified, Kahane et al (2008) concluded that based on their mixed and conflicting results, there was no consistent relationship between abortion and crime in the UK.

The present study employed methods similar to Kahane et al (2008) and also found mixed results concerning the BMDL Hypothesis. The, arguably, most direct test of the BMDL Hypothesis (i.e., the provincial age-specific crime rate analyses) found a positive relationship between the liberalization of abortion in 1988 and crime rates in the 2000s. It would be irresponsible, however, to focus solely on this single result and claim that increasing abortion rates are conclusively associated with increasing crime rates. Taking into consideration the results of the time series plots, the EAR analyses, and the age-specific crime rate analyses together, this study instead concludes that there is no consistent support for the BMDL Hypothesis in Canada.

5.2 Limitations

The results of the analyses conducted in this study generally do not find convincing support for the BMDL Hypothesis. Limitations do exist, however, that must be noted and discussed.

5.2.1 *Sample Size, Methodological Constraints, and Modifications*

Prior research has focused on the legalization of abortion in the US, Canada, and the UK that occurred in the late 1960s and early 1970s and examined crime rates, in some cases, as recent as 2003. The length of time that has elapsed between the legalization of abortion and the crime rates of interest has allowed researchers to examine a large number of observations by year. This study, on the other hand, focused on the liberalization of abortion that occurred in Canada in 1988 and crime rates up to 2011. This shorter length of time allowed for substantially fewer observations of crime rates to be included in analyses. The US in particular affords even more variation as there are 50 states as opposed to only 13 Canadian provinces and territories. The American population is also substantially larger than that of Canada and, therefore, provides far more variation in abortions and crime to investigate.

The primary practical consequence of these differences was a smaller sample size available for analysis in this study. The EAR analyses were, therefore, restricted to investigating only youth crime rates from 1998 to 2011. Prior American analyses that used the EAR strategy had sample sizes of over 900 observations (e.g., Foote and Goetz 2008). As there are far fewer Canadian provinces than there are American states, both the present study as well as Sen's (2007) analysis included only 130 to 144 observations. The data used in the age-specific crime rate analyses were limited even further as reliable age-specific crime rate data were only available from 2006 on, leaving only six year of observations for analysis. Again, prior American analyses that have used this age-specific strategy included, in some cases, nearly 6000 observations while this study included a maximum of 240 observations. Although concerns about the robustness of findings based on these smaller samples may exist, the only way to improve this would be to allow more time to elapse.

The present study also required modified versions of the EAR and age-specific crime rate strategies due to various constraints. As discussed in Section 3.2, the construction of the *EAR* term was based on population proportions as opposed to Donohue and Levitt's (2001) original construction using age-specific arrest rates because such data were not available for Canada. Although this was a different procedure, it was similar to the method used by Sen (2007), who was able to find evidence to support the BMDL Hypothesis. The procedure employed in this study to construct the *EAR* term was, however, arguably more parallel to Donohue and Levitt's (2001) method and relied on fewer assumptions than Sen's (2007) procedure. The *EAR* term was also found to correctly capture the increases in abortion rates and, therefore, should be considered a valid implementation of the EAR strategy.

The age-specific analyses also required modifications, departing from the original strategy used by Donohue and Levitt (2001; 2008). While national age-specific crime rates were obtained by single year of age, provincial age-specific crime rates were obtained in two-year age groupings, reducing the ability to isolate single age cohorts. Furthermore, samples in prior age-specific analyses were large enough to include complete sets of age, year, and state fixed effects along with age-year, age-state, and state-year interaction terms in estimation models. This study, however, could only include age, year, and province fixed effect terms due to small sample sizes. The age-year interaction term in Equations 7a and 7b were the only interaction terms included in models and required the use of a linear time variable to avoid issues of over-identification. Although this study was not able to faithfully replicate prior analyses, the full range of interaction terms could only be included if the number of observations was substantially larger. Unfortunately, the only way to accomplish this would be to allow more time to elapse.

5.2.2 *Migration*

International and interstate/interprovincial migration pose an issue that has not received much attention in this study or in prior research. The threat that migration poses is the possibility that the measures of aggregate abortion rates and aggregate crime rates do not represent the same individuals. That is, individuals may be represented in the crime rates of a geography while not having experienced the abortion rate of that same geography. For instance, if an immigrant was born after 1988 in a country where access to abortion was highly restrictive and then subsequently moved to Canada and committed a crime, they would be represented in Canadian crime rates but would not have experienced Canadian abortion rates while *in utero*. International migration may, therefore, pose a threat to the internal validity of both this and prior studies. Research on immigrant criminality in the US suggests, however, that immigrant crime rates tend to be substantially lower than those of the native-born in most categories of crime (Martinez and Lee 2000). Research on major Canadian cities, moreover has found that at the neighbourhood level, higher concentrations of recent immigrants were either not associated or inversely associated with crime rates (Charron 2009). If international immigrants are not heavily represented in crime rates, then the issue of international immigration may be of little concern for this study.

Interstate and interprovincial migration, however, poses a potentially more serious concern. Similar to the issue of international migration, interstate and interprovincial migration threatens the ability of the aggregated abortion and crime rates of a geography to represent the same individuals. That is, the crime rates of a geographic area must also represent individuals who were born there and, therefore, experienced the abortion rates of that geographic area while *in utero*. This assumption is threatened if high levels of interstate or interprovincial migration occurred in the time between the measures of crime and abortion. Prior research has attempted to manage the issue of interstate migration in several ways. For instance, the literature has stressed the need to use measures of abortion by the state of residence of the women who obtained the abortion rather than by the state of occurrence of abortions to maintain the continuity between abortion and crime rates. Berk et al (2003) attempted to manage interstate migration by analyzing the US as a whole. Donohue and Levitt (2008) used a complex weighting procedure to calculate an abortion rate that accounted for the proportion of a state's population that was born in a different state. The national age-specific crime rate analyses employed in this study avoided the issue of interprovincial migration in the same way that Berk et al (2003) managed interstate migration by examining national measures. Since these measures were aggregated at the national level, interprovincial migration was a non-issue. The EAR analyses and the provincial age-specific analyses, however, employed abortion and crime rates aggregated at the provincial level. These analyses were susceptible to the threat of interprovincial migration. It may, therefore, be beneficial to investigate the level of interprovincial migration that occurred between 1988 and 2011 to assess how great of a threat it may have posed.

Table 5.1 reports average annual levels of interprovincial migration between 1988 to 2011. The average number of annual in- and out-migrants for each province and the percentage of the provincial population constituted by interprovincial migration was calculated. The number of interprovincial migrants under age 25 were calculated separately because this was the demographic primarily influenced by the liberalization of abortion in 1988. The calculations in Table 5.1 suggest that interprovincial migration has not comprised a large proportion of average annual provincial populations, constituting 0.9 percent of provincial populations. Interprovincial migrants under age 25 constituted an even smaller proportion, averaging 0.4 percent of annual provincial populations. Furthermore, average interprovincial migration was relatively low in the four focal provinces (BC, AB, ON, and QC). This suggests that interprovincial migration may not have posed a large threat to the abortion and crime measures used in this study.

Although these percentages were low, they suggest that on average, nearly 300 000 individuals were migrating within Canada every year and over 40 percent were under age 25. During the 23 years between 1988 and 2011, this translates to nearly seven million interprovincial migrants; a potentially non-trivial quantity of individuals. The impact of interprovincial migration on crime rates remains unclear, but is unfortunately beyond the scope of this study. Interprovincial migration does, however, speak to the issues of investigating aggregated variables separated by long periods of time. Many period and history effects have occurred that may have obscured the ability to directly link aggregate measures of abortion and crime. Without individual level data it is difficult to conclusively link these distal variables. Although interprovincial migration may have influenced the measures of abortion and crime that were used in this study, the primary focus was to test the BMDL Hypothesis using the strategies developed in prior research, which relied on assessing the impact of aggregate rates of abortion on aggregate rates of crime.

Table 5.1: Average Interprovincial Migration, Canadian Provinces, 1988-2011

Geography	In-Migrants	Population	%	Out-Migrants	Population	%
Newfoundland						
total	8455	540458	1.6	11766	540458	2.2
under 25	3714	540458	0.7	6090	540458	1.1
PEI						
total	2712	135974	2.0	2773	135974	2.0
under 25	1148	135974	0.8	1388	135974	1.0
Nova Scotia						
total	16255	929830	1.7	17389	929830	1.9
under 25	6912	929830	0.7	8003	929830	0.9
New Brunswick						
total	11482	747820	1.5	12539	747820	1.7
under 25	5004	747821	0.7	5968	747821	0.8
Quebec						
total	22852	7384229	0.3	31735	7384229	0.4
under 25	9091	7384229	0.1	12309	7384229	0.2
Ontario						
total	67925	11656746	0.6	71093	11656746	0.6
under 25	28158	11656746	0.2	28922	11656746	0.2
Manitoba						
total	13973	1154817	1.2	19000	1154817	1.6
under 25	6475	1154817	0.6	8495	1154817	0.7
Saskatchewan						
total	16497	1012138	1.6	21539	1012138	2.1
under 25	7861	1012138	0.8	10486	1012138	1.0
Alberta						
total	69432	3038475	2.3	53489	3038475	1.8
under 25	32383	3038475	1.1	22931	3038475	0.8
British Columbia						
total	58445	3936510	1.5	46135	3936510	1.2
under 25	23609	3936510	0.6	19601	3936510	0.5
Total						
(all provinces)	288029	30536999	0.9	287458	30536999	0.9
Total under 25						
(all provinces)	124353	30536999	0.4	124195	30536999	0.4

Source: Interprovincial migration measures were taken from Statistics Canada, CANSIM Table 051-0012. Provincial population estimates were taken from Statistics Canada, CANSIM Table 051-0001.

5.2.3 *Issues related to Aggregate Measures of Abortion and Crime*

This study, as well as prior research, has focused on examining aggregate abortion and crime rates that occurred decades apart without investigating the actual causal link between these two distal variables. This line of analysis has posed two main problems: the difficulty of reliably linking abortion and crime rates and the lack of investigation into the mechanisms of change. Just as with the issue of migration discussed above, prior research has found it difficult to reliably link crime rates with the appropriate abortion rate that was experienced by cohorts while *in utero*. By convention, the abortion rate of the year prior to a cohort's birth has been linked with the crime rates of that cohort. Terms of pregnancy, however, generally last for nine months. This discrepancy leads to the distinct possibility that the abortion rate of the year prior to a cohort's birth does not accurately capture the exposure to abortion while *in utero*. Individuals born late in a year, for instance, may have been conceived in that same year. Linking their crime rates with the abortion rate of the prior year would, therefore, not be accurate. Overall, it is very difficult to reliably link individual exposure to abortion with rates of criminality when employing aggregate and distal measures of abortion and crime.

The possibility of committing an ecological fallacy is always present when examining aggregate data to make conclusions about individual behaviour and becomes greater when aggregate measures are separated by long periods of time (Babbie 2004). In terms of the BMDL Hypothesis, it is possible that such a fallacy may be committed because the measure used as a proxy for the "wantedness" of cohorts has been rates of abortions and the outcome measure has been incidents of crime aggregated, at minimum, at the provincial level. For instance, "at risk" women may not have been obtaining abortions at high rates and instead, non-"at risk" women may have been predominantly driving the increase in abortion rates shortly after 1988. Evidence suggests that older women, women with higher education, and women with higher incomes may have been the ones more likely to take advantage of the increase in access to abortion services (Finer and Henshaw 2006; Goldin and Katz 2002). An ecological fallacy may be committed, therefore, if "at risk" women were producing more criminal children while non-"at risk" women were increasing the rates of abortion.

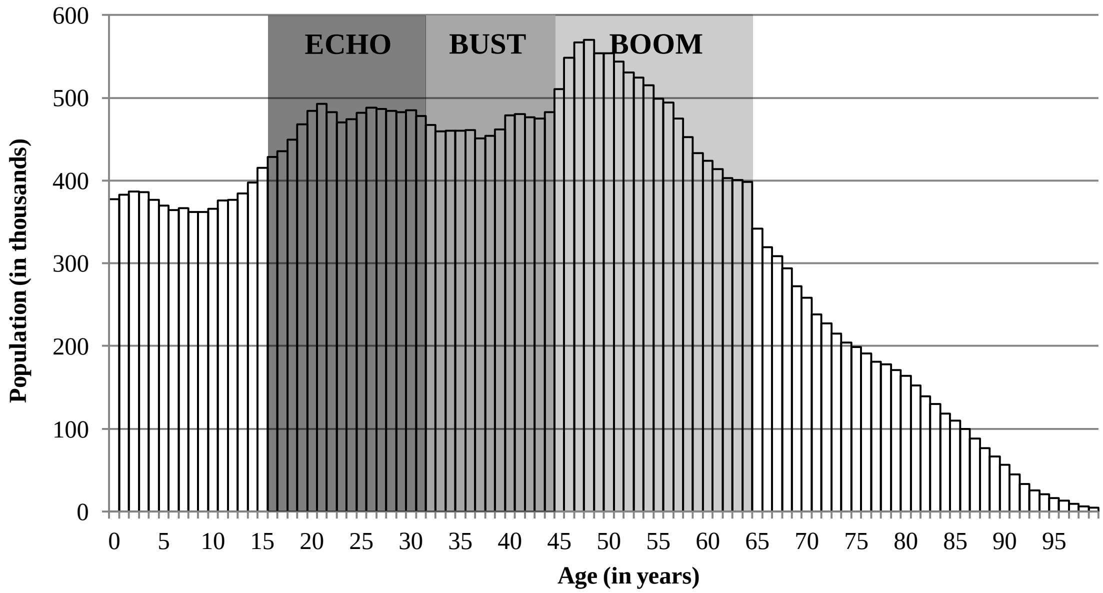
The underlying assumptions for the use of abortion rates as a proxy for the "wantedness" of cohorts are also limitations of this and prior studies. The use of this proxy was based on the assumption that all pregnant women consider aborting an unwanted pregnancy a real possibility. Considering the controversies over abortion, however, this assumption is not entirely tenable. It was also assumed that after legalization, all pregnant women had access to abortion services to the extent that they could easily elect to abort unwanted pregnancies. The prior discussion concerning the differential access to abortion services that women faced based on geography, however, also demonstrates that this is a faulty assumption. Finally, it is assumed that the decline in cost of abortion services did not affect the overall likelihood of conception. It is a real possibility, however, that an increase in the availability of abortion services may have influenced the likelihood of women to engage in risky sexual behaviour, consequently increasing both pregnancy and abortion rates. Joyce (2010) has stressed that abortion is endogenous with many other factors influencing fertility, including contraception, mores of sexual activity, marriage, and labour supply. He argued that the factors that influence the likelihood of unintended or unwanted pregnancy and childbearing represent a complicated structural model.

Other critically important changes that were occurring around the same time as the legalization of abortion may, therefore, have heavily influenced fertility. For instance, the increase in the availability of birth control pills has been found to have decreased the cost of investment into careers for women and increased the age at marriage for women in the 1970s by providing greater certainty regarding the pregnancy outcomes of sexual activity (Goldin and Katz 2002). Changes in the 1970s regarding the costs and attitudes toward sexual activity, marriage, labour force participation, and childbearing coupled with changes in the costs of contraception and abortion may have influenced the likelihood of unintended pregnancy. If any of these aforementioned factors changed the likelihood of sexual behaviour resulting in pregnancy, this may have also artificially changed rates of abortion. In economic terms, this would represent an increase in the demand curve of abortions while the BMDL Hypothesis argues primarily for a change in the supply curve. Instead, Joyce (2010) argues that models that attempt to link abortion and crime must find a method for identifying changes in abortion rates that are induced only by changes in the supply curve of abortion services and not by changes in the incidence of unwanted pregnancies. These aforementioned alternative influences may have had dramatic effects of the likelihood of unwanted pregnancies, while the effect of abortion primarily concerns the prevention of unwanted births. To return to the use of abortion rates as a proxy for "unwantedness," incorporating other endogenous factors (e.g., levels of sexual activity or use of contraceptives) may be a more appropriate way to investigate the BMDL Hypothesis by isolating the impact of abortion in preventing unwanted births from the prevention of unwanted pregnancies through contraceptives.

In addition to these issues surrounding the use of abortion rates, crime rates have also been influenced by a myriad of other socioeconomic and demographic factors. Demographic changes, particularly the effects of relative cohort size, may represent a potentially important influence on crime for this study. Easterlin (1987) has hypothesized that relatively larger cohorts exhibit higher crime propensities as sources of socialization and social control, which come primarily from older age groups, are spread thinner and become strained. This line of research has generally attributed the rise in crime rates of the 1960-70s to the increase in relative size of the "baby-boom" generation (South and Messner 2000). Furthermore, as the relative size of cohorts began to decline after the "baby-boomers," cohort-specific delinquency rates also began to decline (Maxim 1985).

In the context of the present study, the "baby-boom" generation (i.e., born 1947-1966) were the parents of the focal group of study, namely the "baby-boom echo" generation, who were born between 1980-95. In 2011, the "baby-boomers" were 45-65 years of age while the "echo" was 15-30 (Figure 5.1). Following the Easterlin hypothesis, the "echo" generation should have evidenced a lower rate of criminality because they were relatively smaller in size. It is important to note that the cohort that was in their "high crime" years (15-25) during the late 1980s and early 1990s, when crime rates reached an upper maximum, were born just after the "baby-boomers" and were approximately 35-45 years old in 2011. This cohort, or the "baby bust" generation (born in 1967-1979), is relatively smaller than the "boomers" and relatively similar in size to the "echo" cohort. The smaller relative size of these cohorts in comparison to the "boomers" should, therefore, predict declines in crime rates, at least at the national level of aggregation. The results of the national age-specific analyses conducted in this study may, therefore, have been a spurious relationship that could be better explained by the effect of relative cohort size on crime prevalence.

Figure 5.1: Population by Single Year of Age, Canada, 2011



Source: Statistics Canada, CANSIM Table 051-0001

The results of the provincial age-specific analyses, however, were of the opposite, positive direction. Given the increases in abortion rates that occurred around 1988 and the predictions of the Easterlin hypothesis, declines in crime should have been evident in the provincial analyses as well. A cohort explanation may, however, also be able to make sense of these results. The "baby-boomers" may have experienced fewer agents of social control due to the relatively smaller size of the cohorts that preceded them. The "baby-boom echo" cohort, however, was preceded by the larger "boomer" and "bust" cohorts. Although this may have afforded the "echo" cohort with more agents of social control, and consequently lower rates of crime, they were also entering a labour force saturated with the "baby-boomers" and the "baby bust" cohorts (Foot and Stoffman 1996). The lack of labour opportunities and the more volatile economy that the "echo" cohort faced may have increased their likelihood of criminality. This cohort effect may explain why a positive association between abortion and crime was found in the provincial age-specific crime rate analyses. The lack of economic opportunity that the "echo" cohort faced may have increased their criminality and the positive association between abortion and crime found in the provincial age-specific analyses may have been a spurious association.

This discussion on the demographic influences on crime is meant to highlight a larger limitation with both this and the prior analyses that have engaged with the BMDL Hypothesis. The social realities that have surrounded both abortion and crime rates at larger, macro-levels may have had profound influences on these measures. The social, economic, and demographic influences discussed here emphasize just some of the factors that have influenced both abortion and crime between 1988 and 2011. Analyses that aim to investigate the link between abortion and crime should, therefore, take these larger factors into consideration.

Finally, the actual causal mechanism purported by the BMDL Hypothesis has received little attention in both this and prior studies. Although no consistent relationship between abortion and crime was found in this study, confounding period effects or inaccuracies in linking abortion and crime rates may have diluted the effect. To satisfactorily dismiss the BMDL Hypothesis, the theoretical links between increases in access to abortion, increases in the "wantedness" of cohorts, and declines in crime rates should be investigated while accounting for the myriad of external influences described above. According to the BMDL Hypothesis, the legalization of abortion reduced the number of "unwanted" children born into environments of socioeconomic disadvantage, abuse, and neglect, which consequently reduced the criminality of these children. Children who experience environments characterized by abuse, neglect, and socioeconomic disadvantage have been repeatedly shown to have poorer cognitive, social, behavioural, and criminal outcomes (Currie and Tekin 2012; Hildyard and Wolfe 2002; Widom 1989). The "unwantedness" of children may indeed have criminogenic effects. Although the BMDL Hypothesis argues that less of these "unwanted" children have been born since the legalization of abortion, the use of rates of induced abortions as the primary explanatory variable may not be an accurate proxy for the "unwantedness" of cohorts. The association between unwanted pregnancies and unwanted children is a related, but theoretically separate empirical question. Future research should, therefore, follow the theoretical links more closely and look for declines in the rates of "unwanted" children born into adverse environments and declines in the likelihood of criminality for children born into “wanted” environments in direct response to increases in access to abortion services. This type of longitudinal investigation would require individual level data. The lack of support for the BMDL Hypothesis at the aggregate level, however, suggests that there is not much justification for investigating the link between abortion and crime further.

5.3 Conclusion

The aim of this study was to test the BMDL Hypothesis using the 1988 liberalization of abortion services in Canada. Based on a review of the literature, three main strategies were selected that were sensitive to the increases in abortion rates that followed the 1988 liberalization of abortion services. These were time series plots of crime rates, “effective abortion rate” analyses, and age-specific crime rate analyses. The time series plots suggested that period effects, rather than the selection effects purported by the BMDL Hypothesis, were the primary forces driving crime rates. The EAR analyses found no association between the effective abortion rate and annual youth crime rates. Finally, the age-specific crime rate analyses found mixed results that generally failed to support the BMDL Hypothesis. The results of these analyses taken together do not provide consistent support for the BMDL Hypothesis. Limitations of this study do exist, but are common to this line of research and speak to the difficulties of investigating the BMDL Hypothesis using these methods. Future research may benefit from either elucidating the causal links between abortion and crime or using a more encompassing proxy for the latent “unwantedness” variable. The methods and results of this study are not powerful enough to conclusively settle the current academic debate. Nevertheless, the sheer quantity of research that has failed to find evidence to support the BMDL Hypothesis leads one to be highly skeptical of an association between the legalization or liberalization of abortion and crime. Considering the controversy in the academic literature in the US, the lack of support from the UK, and the results of the present study, the BMDL Hypothesis cannot continue to be publically disseminated as an important influence on declines in crime rates.

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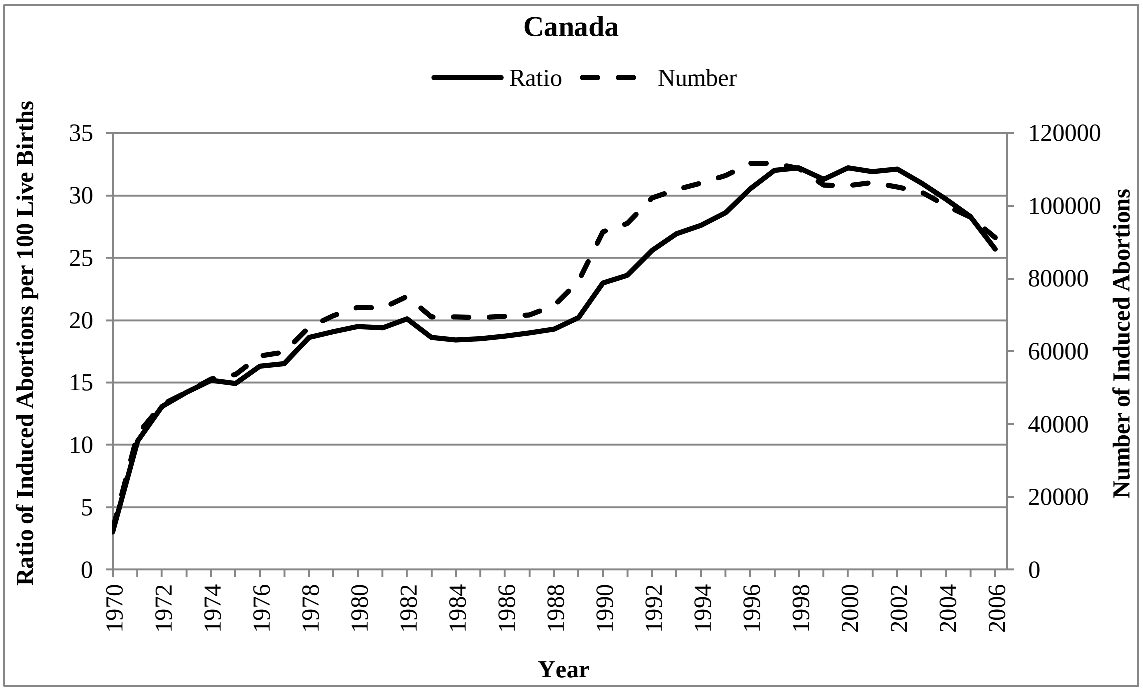
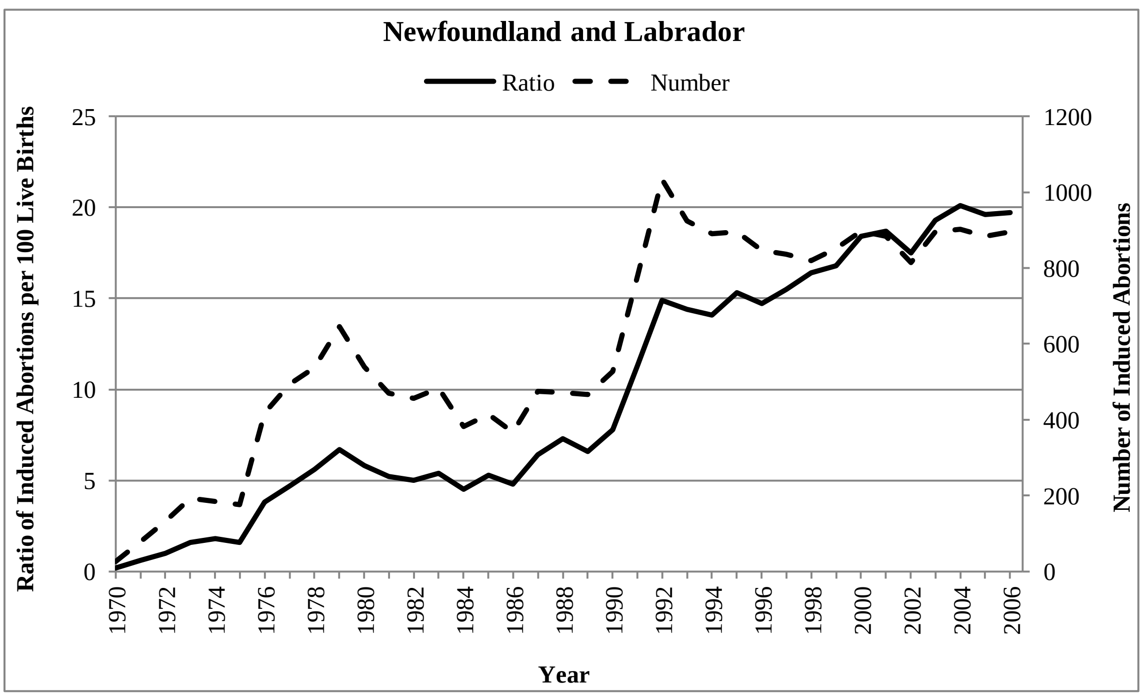
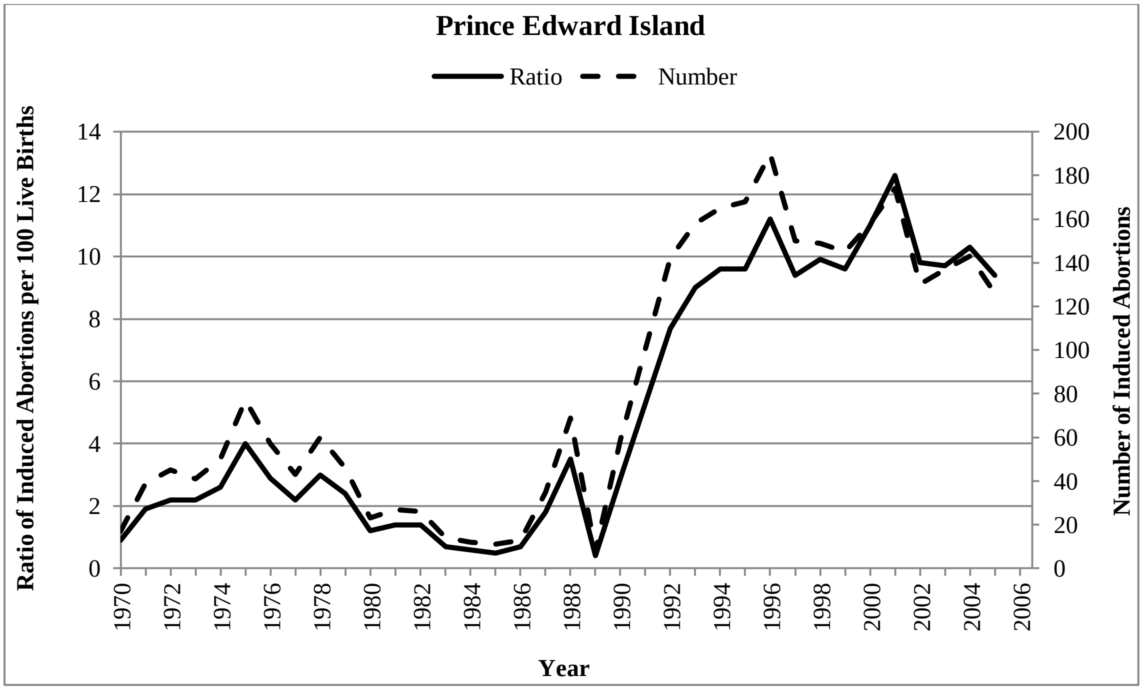
Appendices

Appendix A: Trend Graphs of Abortions and Abortion Ratios, Canada and Provinces, 1970-2006






Prince Edward Island
Ratio
Number
Ratio of Induced Abortions per 100 Live Births
Number of Induced Abortions
Year
0
2
4
6
8
10
12
14
0
20
40
60
80
100
120
140
160
180
200
1970
1972
1974
1976
1978
1980
1982
1984
1986
1988
1990
1992
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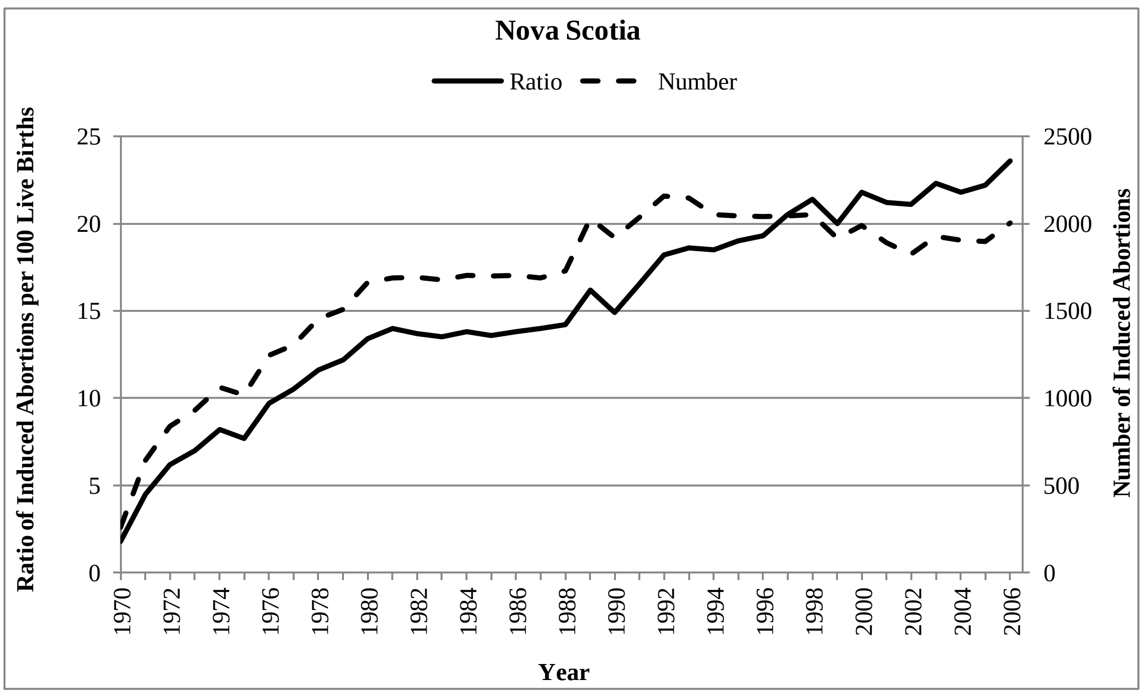
1998
2000
2002
2004
2006


Nova Scotia
Ratio
Number
Ratio of Induced Abortions per 100 Live Births
Number of Induced Abortions
Year
0
5
10
15
20
25
0
500
1000
1500
2000
2500
1970
1972
1974
1976
1978
1980
1982
1984
1986
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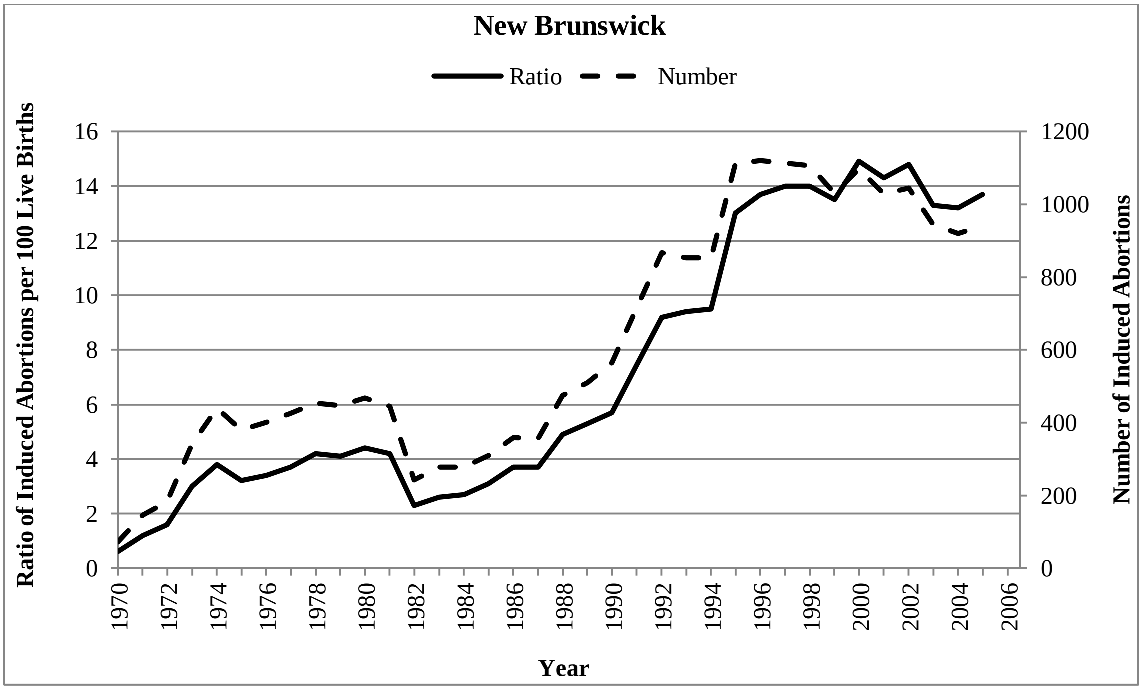
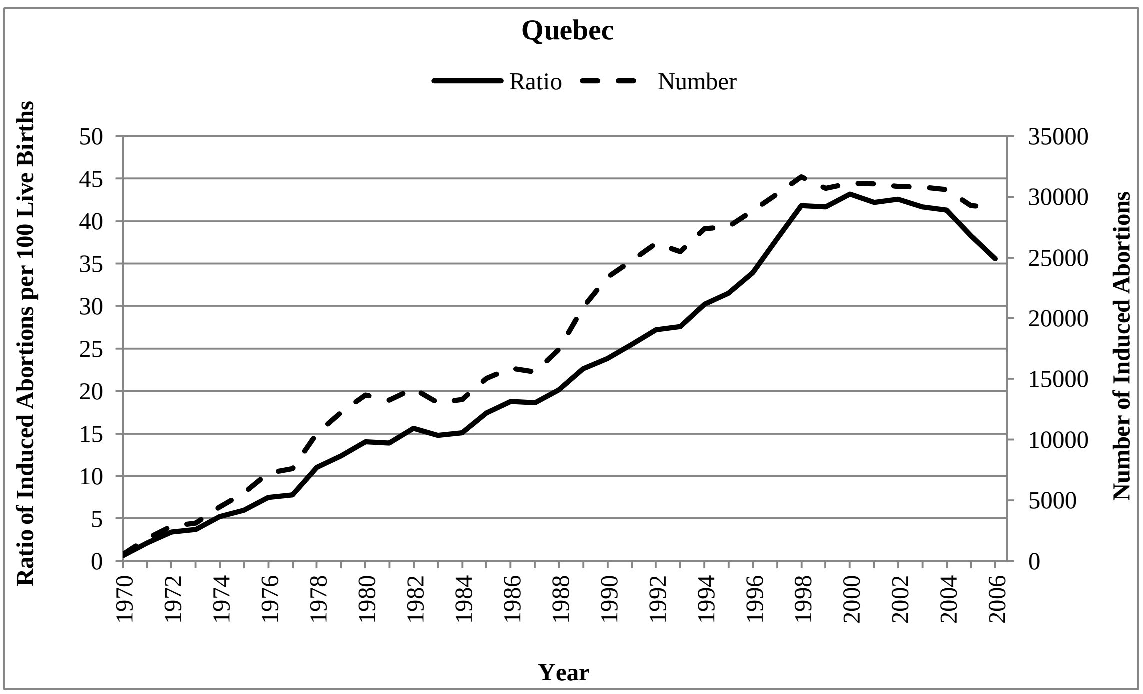
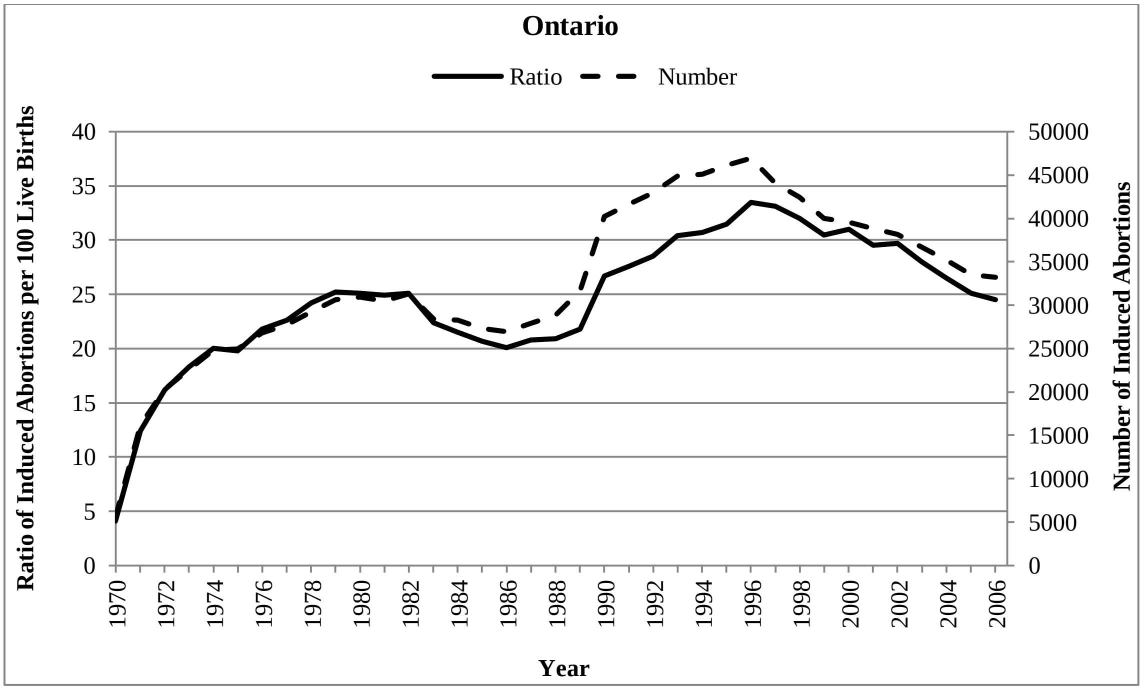
1996
1998
2000
2002
2004
2006


New Brunswick
Ratio
Number
Ratio of Induced Abortions per 100 Live Births
Number of Induced Abortions
0
2
4
6
8
10
12
14
16
0
200
400
600
800
1000
1200
1970
1972
1974
1976
1978
1980
1982
1984
1986
1988
1990
1992
1994
1996
1998
2000
2002
2004
2006
Year


Quebec
Ratio
Number
Ratio of Induced Abortions per 100 Live Births
Number of Induced Abortions
0
5
10
15
20
25
30
35
40
45
50
0
5000
10000
15000
20000
25000
30000
35000
1970
1972
1974
1976
1978
1980
1982
1984
1986
1988
1990
1992
1994
1996
1998
2000
2002
2004
2006
Year


Ontario
Ratio
Number
Ratio of Induced Abortions per 100 Live Births
Number of Induced Abortions
0
5
10
15
20
25
30
35
40
0
5000
10000
15000
20000
25000
30000
35000
40000
45000
50000
1970
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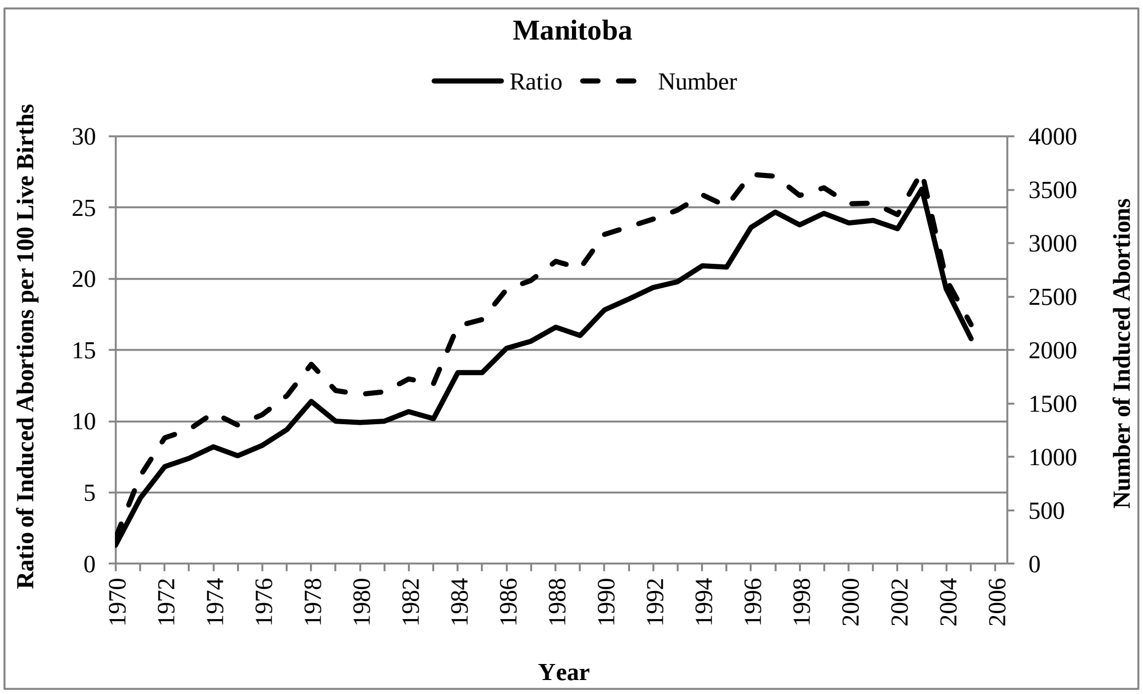
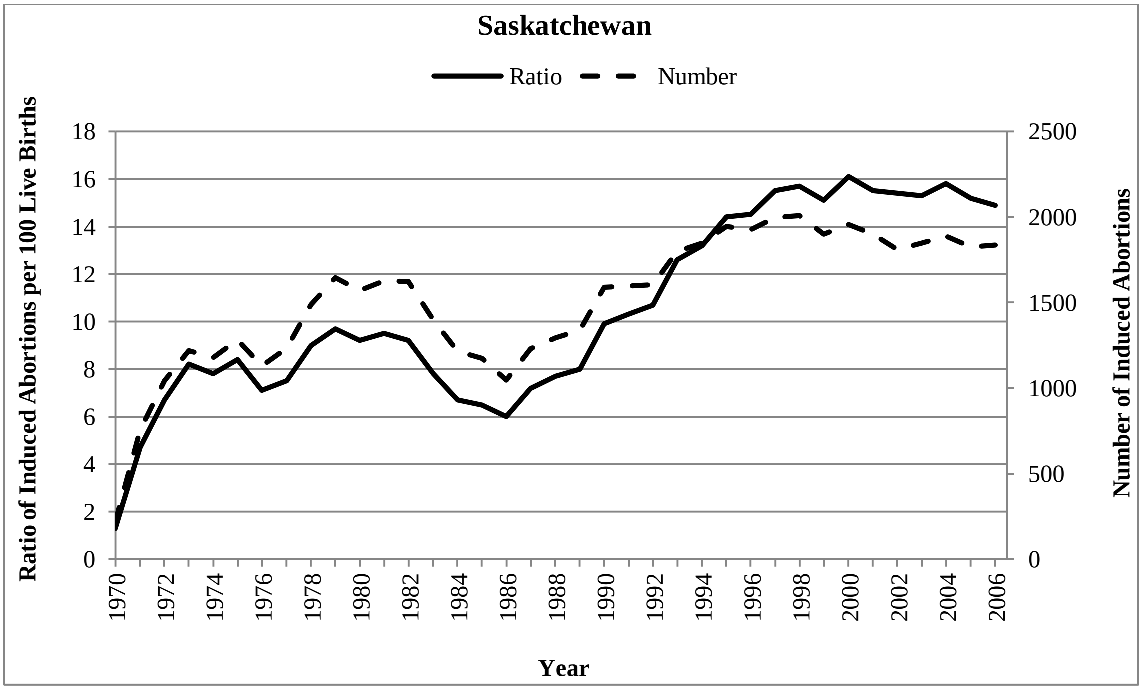
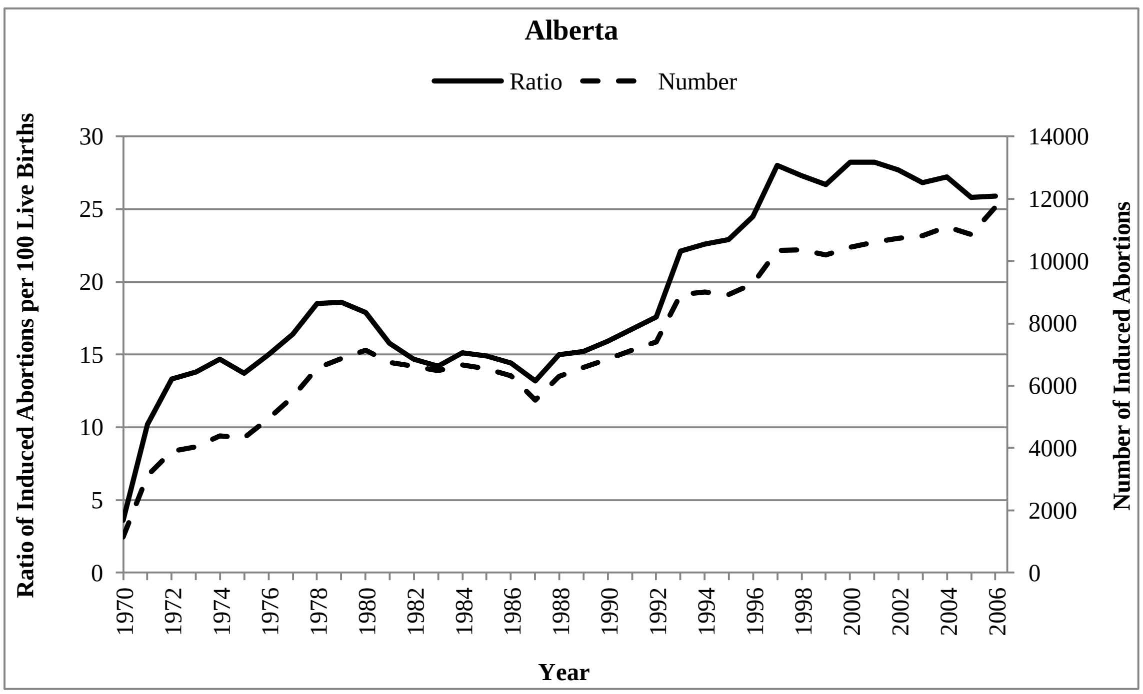
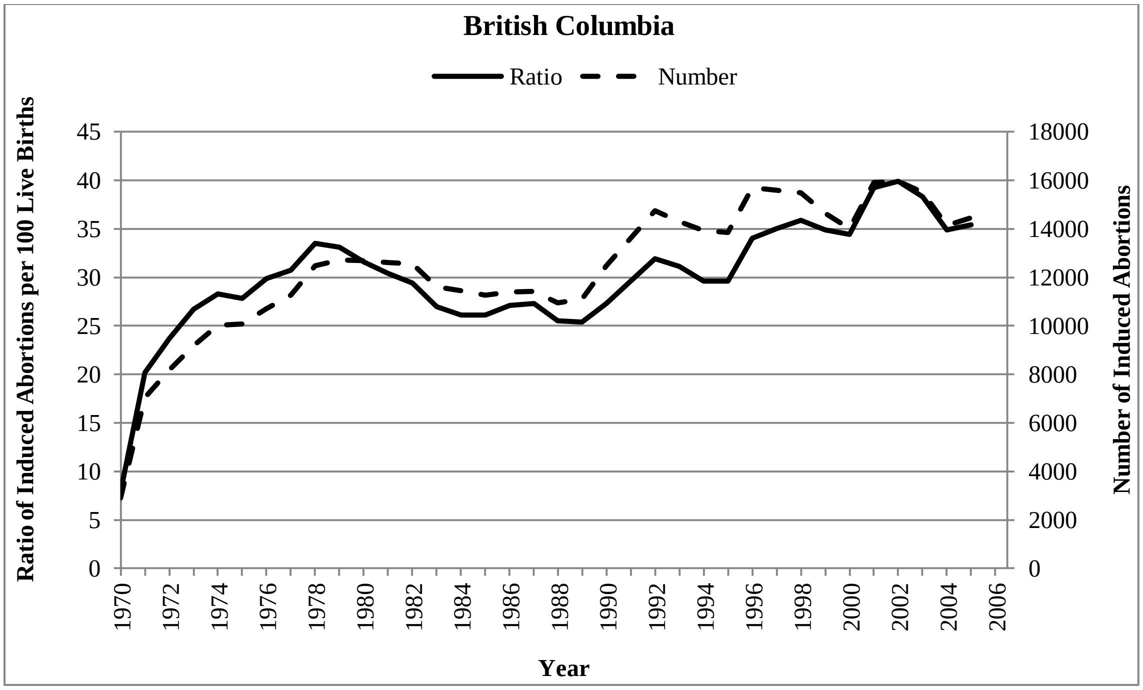
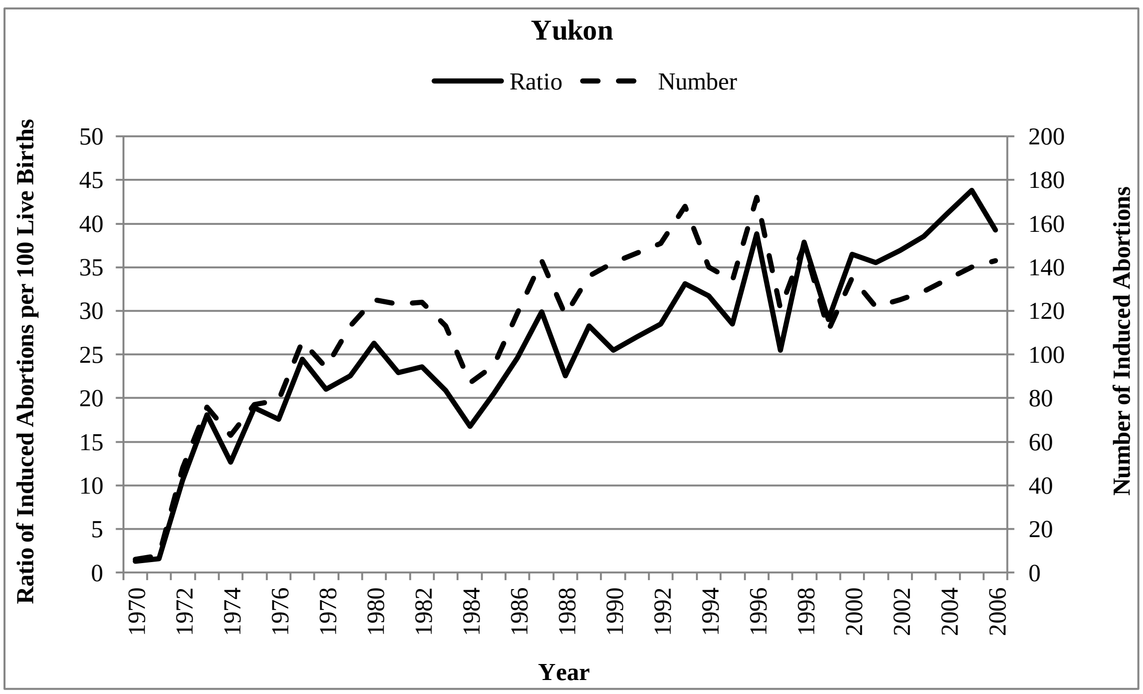
2000
2002
2004
2006
Year


Manitoba
Ratio
Number
Ratio of Induced Abortions per 100 Live Births
Number of Induced Abortions
0
5
10
15
20
25
30
0
500
1000
1500
2000
2500
3000
3500
4000
1970
1972
1974
1976
1978
1980
1982
1984
1986
1988
1990
1992
1994
1996
1998
2000
2002
2004
2006
Year


Saskatchewan
Ratio
Number
Ratio of Induced Abortions per 100 Live Births
Number of Induced Abortions
0
2
4
6
8
10
12
14
16
18
0
500
1000
1500
2000
2500
1970
1972
1974
1976
1978
1980
1982
1984
1986
1988
1990
1992
1994
1996
1998
2000
2002
2004
2006
Year


Alberta
Ratio
Number
Ratio of Induced Abortions per 100 Live Births
Number of Induced Abortions
0
5
10
15
20
25
30
0
2000
4000
6000
8000
10000
12000
14000
1970
1972
1974
1976
1978
1980
1982
1984
1986
1988
1990
1992
1994
1996
1998
2000
2002
2004
2006
Year





Source: Statistics Canada, Therapeutic Abortion Survey, CANSIM Table no. 106-9005
Note: Nunavut officially separated from the Northwest Territories in 1999, creating discontinuity in the abortion data and, therefore, were not included in this set of graphs.

Appendix B: Uniform Crime Reporting Incident-based Survey (UCR2) categories included in the EAR and age-specific analyses

Categories included in the violent crime rate

- Homicide
  - Murder, first degree
  - Murder, second degree
  - Manslaughter
  - Infanticide
- Other violations causing death
  - Criminal negligence causing death
  - Other related violations causing death
- Attempted murder
- Sexual assault, level 3, aggravated
- Sexual assault, level 2, weapon or bodily harm
- Sexual assault, level 1
- Sexual violations against children
  - Sexual interference
  - Invitation to sexual touching
  - Sexual exploitation
  - Luring a child via a computer
- Assault, level 3, aggravated
- Assault, level 2, weapon or bodily harm
- Assault, level 1
- Assaults against a peace officer
- Other assaults
  - Unlawfully causing bodily harm
  - Criminal negligence causing bodily harm
  - Other assaults
- Firearms; use of, discharge, pointing
  - Discharge firearm with intent
  - Using firearm in commission of offence
  - Pointing a firearm
- Robbery
  - Robbery
  - Robbery to steal a firearm
- Forcible confinement or kidnapping
  - Forcible confinement
  - Kidnapping
- Abduction
  - Abduction under the age 14, not parent or guardian
  - Abduction under the age 16
  - Removal of children from Canada
  - Abduction under the age 14 contravening a custody order
  - Abduction under the age 14, by parent or guardian

- Extortion
- Criminal harassment
- Uttering threats
- Threatening or harassing phone calls
- Other violent violations
  - Conspire to commit murder
  - Other sexual violations
  - Sexual exploitation of a person with a disability
  - Incest
  - Corrupting morals of a child
  - Anal intercourse
  - Bestiality, commit or compel person
  - Voyeurism
  - Trap likely to or causing bodily harm
  - Hostage-taking
  - Trafficking in persons
  - Intimidation of a justice system participant or a journalist
  - Intimidation of a non-justice participant
  - Explosives causing death or bodily harm
  - Arson, disregard for human life
  - Other violent violations

Categories included in the property crime rate

- Breaking and entering
  - Breaking and entering
  - Breaking and entering to steal a firearm
  - Break and enter to steal a firearm from a motor vehicle
- Possession of stolen property
  - Possess stolen property
  - Possession of stolen goods over $5000
  - Possession of stolen goods $5000 or under
- Trafficking in stolen goods
  - Traffic stolen goods over $5000 (including intent)
  - Traffic stolen goods under $5000 (including intent)
- Theft of motor vehicle
  - Theft of motor vehicle over $5000
  - Theft of motor vehicle $5000 or under
  - Motor vehicle theft
- Theft over $5000 (non-motor vehicle)
  - Theft over $5000
  - Theft over $5000 from a motor vehicle
  - Shoplifting over $5000
- Theft under $5000 (non-motor vehicle)
  - Theft $5000 or under
  - Theft $5000 or under from a motor vehicle
  - Shoplifting $5000 or under

- Fraud
- Identity theft
- Identity fraud
- Mischief
  - Mischief
  - Mischief to religious property motivated by hate
- Arson
- Altering/removing/destroying vehicle identification number (VIN)

Appendix C: Complete model outputs for the EAR and age-specific analyses

EAR Base Model Output

Variable	ln Violent Crime	ln Property Crime
EAR	.014 (.012)	-.003 (.010)
Province Fixed Effects		
NL	.419 (.262)	.324 (.207)
PE	.134 (.329)	.135 (.262)
NS	.552 (.185)**	.433 (.140)**
NB	.580 (.295)	.218 (.235)
QC	-.091 (.093)	-.304 (.061)***
MB	.691 (.165)***	.523 (.127)***
SK	.978 (.260)***	1.101 (.206)***
AB	.296 (.160)	.486 (.123)***
BC	-.019 (.086)	.208 (.065)**
Year Fixed Effects		
1999	.087 (.125)	-.083 (.092)
2000	.280 (.101)**	-.003 (.079)
2001	.348 (.094)***	.028 (.080)
2002	.315 (.096)**	-.007 (.082)
2003	.368 (.100)***	.075 (.077)
2004	.266 (.104)*	.021 (.079)
2005	.303 (.109)**	-.030 (.085)
2006	.379 (.120)**	.047 (.105)
2007	.370 (.125)**	.015 (.101)
2008	.355 (.138)*	-.017 (.109)
2009	.298 (.147)*	-.029 (.114)
2010	.254 (.155)	-.110 (.121)
2011	.179 (.161)	-.257 (.131)
Constant	6.771 (.327)***	8.124 (.265)***
N	140	140
R^2	.818	.899

* $p \leq .05$; ** $p \leq .01$; *** $p \leq .001$

Note: Robust standard errors are reported in parentheses next to their respective coefficients. Ontario was the reference group for the province fixed effects. 1998 was the reference group for the year fixed effects.

EAR with Lagged Dependant Variable Model Output

Variable	ln Violent Crime	ln Property Crime
EAR	.014(.006)*	.008(.004)
Province Fixed Effects		
NL	.362(.121)**	.275(.089)**
PE	.315(.146)*	.295(.102)**
NS	.364(.083)***	.276(.062)**
NB	.420(.135)**	.271(.096)***
QC	.041(.040)	-.050(.038)
MB	.381(.086)***	.256(.063)***
SK	.552(.130)***	.519(.110)***
AB	.227(.071)**	.241(.060)***
BC	-.024(.027)	.040(.024)
Year Fixed Effects		
1999	*omitted*	*omitted*
2000	.136(.056)*	.138(.034)***
2001	.075(.058)	.108(.033)**
2002	-.005(.056)	.046(.037)
2003	.065(.064)	.148(.035)***
2004	-.078(.070)	.027(.039)
2005	.020(.076)	.006(.046)
2006	.064(.066)	.111(.054)*
2007	-.004(.068)	.018(.043)
2008	-.021(.072)	-.002(.044)
2009	-.077(.071)	.001(.044)
2010	-.092(.075)	-.079(.048)
2011	-.147(.080)	-.174(.061)**
LDV	.656(.075)***	.714(.073)***
Constant	2.169(.558)***	2.009(.612)**
N	130	130
R^2	0.926	0.966

* $p \leq .05$; ** $p \leq .01$; *** $p \leq .001$

Note: Robust standard errors are reported in parentheses next to their respective coefficients. Ontario was the reference group for the province fixed effects. 1998 was the reference group for the year fixed effects. The year 1999 was omitted by STATA due to collinearity

National Age-Specific Violent Crime Rate Analysis Model Output

Variable	Equation 5a	Equation 6a	Equation 7a
ABORT	-.012(.002)***	-.009(.003)**	.000(.012)
Age Fixed Effects			
13	.507(.026)***	.410(.064)***	.823(.103)***
14	.828(.024)***	.669(.099)***	1.253(.122) ***
15	1.045(.020)***	.844(.123)***	1.498(.138) ***
16	1.121(.020)***	.905(.132)***	1.591(.153) ***
17	1.137(.023)***	.925(.135)***	1.581(.158) ***
18	1.046(.023)***	.842(.125)***	1.492(.172) ***
19	.960(.025)***	.772(.119)***	1.392(.167) ***
20	.919(.026)***	.740(.110)***	1.363(.159) ***
21	.837(.026)***	.681(.102)***	1.222(.153) ***
22	.790(.027)***	.644(.096)***	1.143(.147) ***
23	.740(.028)***	.599(.090)***	1.050(.141) ***
24	.699(.027)***	.571(.086)***	1.022(.125) ***
25	.698(.027)***	.572(.086)***	.980(.123) ***
26	.627(.028)***	.516(.077)***	.882(.125) ***
27	.607(.029)***	.501(.075)***	.817(.128) ***
28	.562(.028)***	.468(.070)***	.760(.128) ***
29	.521(.030)***	.438(.066)***	.761(.148) ***
30	.537(.029)***	.435(.069)***	.821(.173) ***
31	.444(.032)***	.371(.062)***	.663(.178) ***
Year Fixed Effects			
2007	-.081(.010)***	*omitted*	
2008	.012(.008)	.109(.012)***	
2009	.040(.008)***	.118(.010)***	
2010	.001(.008)	.073(.011)***	
2011	.002(.009)	.082(.010)***	
Year			-.016(.022)
13*Year			-.024(.019)
14* Year			-.014(.019)
15* Year			.005(.019)
16* Year			.013(.020)
17* Year			.028(.020)
18* Year			.022(.020)
19* Year			.022(.021)
20* Year			.018(.019)
21* Year			.030(.027)
22* Year			.034(.026)
23* Year			.039(.027)
24* Year			.032(.029)
25* Year			.042(.031)
26* Year			.041(.029)
27* Year			.051(.027)
28* Year			.050(.021)*
29* Year			.038(.019)*
30* Year			.029(.022)
31* Year			.041(.020)*
LDV		.205(.120)	-.388(.084) ***
Constant	6.937(.056)***	5.424(.836)***	9.207(.630) ***
N	120	100	100
R^2	.993	.993	.985

Note: Robust standard errors are reported in parentheses next to their respective coefficients. 12 year olds were the reference group for the age fixed effects. 2006 was the reference group for the year fixed effects. The year 2007 was omitted in Equation 6a by STATA due to collinearity. * $p \leq .05$; ** $p \leq .01$; *** $p \leq .001$.

National Age-specific Property Crime Rate Analysis Model Output

Variable	Equation 5a	Equation 6a	Equation 7a
ABORT	-.013(.004)**	-.015(.003)***	-.008(.013)
Age Fixed Effects			
13	.575(.054)***	.139(.080)	.626(.107)***
14	1.024(.050) ***	.233(.148)	1.086(.145)***
15	1.223(.050) ***	.265(.180)	1.292(.164)***
16	1.284(.051) ***	.280(.192)	1.311(.182)***
17	1.225(.051) ***	.255(.187)	1.177(.172)***
18	1.076(.054) ***	.202(.170)	.959(.190)***
19	.853(.057) ***	.136(.142)	.699(.172)***
20	.676(.057) ***	.074(.120)	.514(.151)**
21	.537(.059) ***	.046(.103)	.336(.127)*
22	.422(.059) ***	.015(.091)	.240(.139)
23	.349(.061) ***	-.013(.082)	.127(.121)
24	.293(.064) ***	-.033(.075)	.125(.095)
25	.281(.060) ***	-.027(.074)	.161(.092)
26	.222(.058) ***	-.022(.067)	.049(.102)
27	.172(.059)**	-.030(.062)	-.047(.101)
28	.133(.060)*	-.038(.058)	-.141(.111)
29	.063(.062)	-.063(.057)	-.215(.135)
30	.069(.064)	-.080(.064)	-.131(.169)
31	-.052(.067)	-.111(.056)	-.348(.182)
Year Fixed Effects			
2007	-.072(.016) ***	-.070(.016)***	
2008	-.009(.013)	.057(.013)***	
2009	.020(.014)	.046(.013)**	
2010	-.032(.016)*	-.019(.016)	
2011	-.062(.019)**	*omitted*	
Year			-.077(.025)**
13*Year			.013(.022)
14* Year			.032(.027)
15* Year			.040(.028)
16* Year			.059(.035)
17* Year			.077(.030)*
18* Year			.090(.032)**
19* Year			.093(.029)**
20* Year			.087(.026)**
21* Year			.094(.023)***
22* Year			.086(.029)**
23* Year			.091(.029)**
24* Year			.073(.028)*
25* Year			.062(.029)*
26* Year			.076(.031)*
27* Year			.086(.027)**
28* Year			.099(.023)***
29* Year			.097(.021)***
30* Year			.079(.023)**
31* Year			.101(.023)***
LDV		.767(.147)***	-.155(.113)
Constant	7.524(.113) ***	2.053(1.038)	8.727(.892)***
N	120	100	100
R^2	.990	.994	.991

Note: Robust standard errors are reported in parentheses next to their respective coefficients. 12 year olds were the reference group for the age fixed effects. 2006 was the reference group for the year fixed effects. The year 2011 was omitted in Equation 6a by STATA due to collinearity. * $p \leq .05$; ** $p \leq .01$; *** $p \leq .001$.

Provincial Age-specific Violent Crime Rate Analysis Model Output

Variable	Equation 5b	Equation 6b	Equation 7b
ABORT	.006(.005)	.007(.002)**	.009(.003)**
Age Fixed Effects			
14-15	.776(.085)***	.328(.038)***	.296(.106)**
16-17	1.011(.087)***	.436(.045) ***	.393(.127)**
18-19	.937(.097) ***	.416(.046) ***	.363(.129)**
20-21	.849(.101) ***	.386(.048) ***	.290(.126)*
22-23	.752(.106) ***	.357(.047) ***	.238(.124)
24-25	.687(.105) ***	.330(.045) ***	.192(.113)
26-27	.608(.103) ***	.293(.047) ***	.148(.120)
28-29	.546(.107) ***	.275(.044) ***	.145(.115)
30-31	.538(.110) ***	.261(.047) ***	.153(.138)
Year Fixed Effects			
2007	.058(.073)	.038(.020)	
2008	.220(.067)**	.162(.023) ***	
2009	.269(.070)***	.111(.016) ***	
2010	.237(.070)**	.046(.014)**	
2011	.175(.069)*	*omitted*	
Linear Year and Interaction			
Year			-.044(.020)*
14-15*Year			.008(.024)
16-17*Year			.011(.027)
18-19*Year			.014(.026)
20-21*Year			.026(.026)
22-23*Year			.033(.026)
24-25*Year			.037(.024)
26-27*Year			.039(.025)
28-29*Year			.035(.024)
30-31*Year			.030(.027)
Province Fixed Effect			
QC	.003(.041)	.051(.017)**	.060(.018)**
AL	.383(.051)***	.207(.025)***	.217(.029)***
BC	-.351(.062)***	.062(.019)**	.059(.021)**
LDV		.602(.030)***	.611(.044)***
Constant	6.297(.207)***	2.308(.215)***	2.441(.340)***
N	240	200	200
R^2	.721	.953	.936

* $p \leq .05$; ** $p \leq .01$; *** $p \leq .001$

Note: Robust standard errors are reported in parentheses next to their respective coefficients. 12-13 year olds were the reference group for the age fixed effects. 2006 was the reference group for the year fixed effects. Ontario was the reference group for the province fixed effects. The year 2011 was omitted in Equation 6b by STATA due to collinearity

Provincial Age-specific Property Crime Rate Analysis Model Output

Variable	Equation 5b	Equation 6b	Equation 7b
ABORT	.008(.005)	.003(.002)	.005(.003)*
Age Fixed Effects			
14-15	.926(.087)***	.385(.050)***	.308(.140)*
16-17	1.113(.089)***	.475(.056)***	.394(.142)**
18-19	.904(.100)***	.386(.057)***	.233(.166)
20-21	.567(.104)***	.242(.050)***	.025(.136)
22-23	.357(.101)**	.151(.047)**	-.069(.121)
24-25	.261(.101)*	.096(.049)	-.136(.122)
26-27	.185(.096)	.079(.046)	-.115(.111)
28-29	.122(.096)	.048(.045)	-.173(.116)
30-31	.081(.106)	.032(.050)	-.166(.138)
Year Fixed Effects			
2007	.058(.074)	.050(.019)**	
2008	.199(.066)**	.156(.021)***	
2009	.229(.068)**	.103(.014)***	
2010	.144(.070)*	*omitted*	
2011	.054(.068)	-.039(.017)*	
Linear Year and Interaction			
Year			-.078(.017)***
14-15*Year			.015(.026)
16-17*Year			.017(.027)
18-19*Year			.037(.030)
20-21*Year			.057(.025)*
22-23*Year			.059(.022)**
24-25*Year			.063(.021)**
26-27*Year			.053(.021)*
28-29*Year			.060(.020)**
30-31*Year			.055(.022)*
Province Fixed Effect			
QC	-.156(.038)***	-.057(.020)**	-.042(.024)
AL	.603(.049)***	.250(.030)***	.253(.036)***
BC	-.334(.064)***	.071(.021)**	.071(.021)**
LDV		.595(.044)***	.619(.057)***
Constant	6.806(.202)***	2.712(.353)***	2.836(.478)***
N	240	200	200
R^2	.836	.973	.967

* $p \leq .05$; ** $p \leq .01$; *** $p \leq .001$

Note: Robust standard errors are reported in parentheses next to their respective coefficients. 12-13 year olds were the reference group for the age fixed effects. 2006 was the reference group for the year fixed effects. Ontario was the reference group for the province fixed effects. The year 2010 was omitted in Equation 6b by STATA due to collinearity

Appendix D: EAR Analysis Robustness Checks

The present study relied on two statistical strategies that have emerged from the econometric debate to test the BMDL Hypothesis. Past research that has employed similar strategies have included an assortment of covariates, but consistency has not been maintained between each study (Berk et al. 2003; Moody and Marvell 2010). The present study, therefore, did not elect to include any covariates other than the fixed effects variables. Covariates that have been found in past research to predict crime rates at statistically significant levels were, however, employed here to check the robustness of the abortion coefficient estimates.

Two main sources were used to select potentially important covariates. Moody and Marvell (2010) conducted a case study using the BMDL Hypothesis to explain their method of selecting control variables, called the "general-to-specific (GETS) winnowing of controls." They found that three control variables were able to predict the US violent crime rate at a significance level of $p<0.05$. These were the property crime arrest rate, real welfare payments per capita lagged 15 years, and the three-strikes law. They found that five control variables could predict the US property crime rate at a significance level of $p<0.05$. These were the crack-cocaine index, the one-gun-per-month law, the percent of the population that was 5-14 years old, the prison population per capita, and the Saturday night special ban.

As Sen's (2007) study was the sole Canadian investigation of the BMDL Hypothesis, it was also selected to find potentially important control variables for this robustness check. In Sen's (2007) analysis, he found that the employment rate was able to predict the Canadian violent crime rate at a significance level of $p<0.05$. He also found that the employment rate, the average beer consumption, and the percent of low-income families were able to predict the Canadian property crime rate at a significance level of $p<0.05$.

These covariates that have been identified were added to the EAR analyses conducted in the present study to check the robustness of the abortion coefficient estimates because both prior studies (i.e., Moody and Marvell 2010 and Sen 2007) also employed the EAR strategy. Canadian data that best approximated the covariates used by Moody and Marvell (2010) were searched for and identified. For the violent crime rate, the youth property crime rate (*PROPERTY*) was used instead of the property crime arrest rate because the EAR analysis in the present study only focused on youth crime rates. The average annual government transfers to the lowest income quintile (lagged 15 years) (*TRANSFER*) was used as a proxy for real welfare payments per capita lagged 15 years. A dummy variable capturing the *Tackling Violent Crime Bill* (2008) (*TVCB*) was used as it was the Canadian change in law that was most similar to the American three-strike laws. For the property crime rate, the percent of the population 5-14 years of age was included. The youth incarceration rate per 10 000 young persons was used as a proxy for the prison population per capita because, as mentioned previously, the EAR analysis in the present study only focused on youth crime rates. A proxy for the crack-cocaine index was not sought in the present study because such Canadian data were unavailable and, as argued in the main text, the crack-cocaine epidemic was not a major influence in Canada during the study time frame, between 1998-2011. No proxies for the one-gun-per-month law or the Saturday night special ban were included because there was also no similar legislation during the study time frame, between 1998-2011.

Data that best approximated the covariates used by Sen (2007) were also identified. The teen employment rate (i.e., age 15-19) (*EMP15-19*) was used instead of the employment rate because the EAR analysis in the present study only focused on youth crime rates. The data for average beer consumption and the percent of low-income families were identical to that used by Sen (2007). The results of the EAR analyses after the aforementioned covariates were included are presented below for violent and property crime separately.

EAR Base Model Output

Variable	ln Violent Crime	ln Property Crime
EAR	.017 (.007)*	.014 (.011)
Province Fixed Effects		
NL	-.155 (.058)	.340 (.095)***
PE	-.011 (.149)	.872 (.186)***
NS	-.219 (.223)	1.038 (.232)***
NB	.305 (.146)*	.470 (.175)**
QC	.064 (.066)	.140 (.165)
MB	.437 (.180)*	.460 (.266)
SK	.226 (.123)	.632 (.176)***
AB	.066 (.210)	.549 (.276)*
BC	.134 (.141)	.848 (.291)**
Year Fixed Effects		
1999	.159 (.073)*	*omitted*
2000	.267 (.061)***	.067 (.071)
2001	.298 (.054)***	.072 (.083)
2002	.299 (.058)***	.046 (.095)
2003	.268 (.068)***	.160 (.089)
2004	.204 (.058)**	.130 (.105)
2005	.294 (.056)***	.114 (.125)
2006	.297 (.060)***	.148 (.150)
2007	.315 (.064)***	-.000 (.155)
2008	.001 (.063)	-.069 (.169)
2009	-.049 (.049)	-.066 (.168)
2010	-.023 (.045)	-.164 (.196)
2011	*omitted*	-.356 (.221)
PROPERTY	.000(.000)***	
TRANSFER	.000(.000)	
TVCB	.332(.077)***	
EMP15-19	-.002(.004)	.005(.005)

POP5-14		-6.655(7.035)
INCPOP		.023(.006)***
BEER		-.015(.006)*
LOWINC		-.025(.012)*
Constant	5.992 (.316)***	9.390 (.994)***
N	140	125
R^2	.924	.934

* p ≤ .05; ** p ≤ .01; *** p ≤ .001

Note: Robust standard errors are reported in parentheses next to their respective coefficients. Ontario was the reference group for the province fixed effects. 1998 was the reference group for the year fixed effects.

EAR with Lagged Dependant Variable Model Output

Variable	ln Violent Crime	ln Property Crime
EAR	.017(.005)***	.012(.005)
Province Fixed Effects		
NL	-.105(.033)**	.095(.064)
PE	.090(.087)	.451(.102)***
NS	.006(.154)	.612(.131)***
NB	.305(.090)***	.337(.103)**
QC	.102(.035)**	.099(.092)
MB	.430(.116)***	.326(.145)*
SK	.259(.077)***	.336(.105)**
AB	.254(.133)	.415(.134)**
BC	.224(.102)*	.449(.161)**
Year Fixed Effects		
1999	*omitted*	*omitted*
2000	.109(.049)*	.136(.037)***
2001	.075(.050)	.116(.044)*
2002	.038(.051)	.067(.057)
2003	.055(.058)	.186(.059)**
2004	-.051(.055)	.086(.077)
2005	.056(.058)	.090(.098)
2006	.065(.054)	.188(.114)
2007	.043(.055)	.071(.118)
2008	.058(.043)	.040(.130)
2009	-.006(.037)	.057(.145)
2010	.004(.028)	-.019(.152)
2011	*omitted*	-.137(.183)
LDV	.410(.076)***	.648(.083)***
PROPERTY	.000(000)***	
TRANSFER	.000(.000)	
TVCB	-.008(.069)	
EMP15-19	-.004(.003)	.000(.003)
POP5-14		-5.051(4.896)
INCPOP		.006(.005)
BEER		-.007(.003)*
LOWINC		-.013(.010)
Constant	3.629(.506)***	2.009(.612)**
N	130	125
R^2	0.951	0.967

* p ≤ .05; ** p ≤ .01; *** p ≤ .001

Note: Robust standard errors are reported in parentheses next to their respective coefficients. Ontario was the reference group for the province fixed effects. 1998 was the reference group for the year fixed effects. The year 1999 was omitted by STATA due to collinearity

After the aforementioned covariates were added to the EAR models, the re-estimated results of EAR analyses did not differ significantly from the original EAR estimates. Only one coefficient estimate changed signs (i.e., property crime base model) and the change was from a negative to a positive direction. The coefficient estimates did generally increased in magnitude, but it was only in the *positive* direction. Considering these results, the exclusion of these covariates may have actually suppressed the positive association between abortion and crime. This suggests that the original EAR analyses may represent a conservative estimate of the association between abortion and crime. The results of this robustness check further reinforce the finding in this study and further fail to support the BMDL Hypothesis.

Curriculum Vitae

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