

# The Abortion–Crime Link: Evidence from England and Wales

By LEO H. KAHANE<sup>†</sup>, DAVID PATON<sup>‡</sup> and ROB SIMMONS<sup>††</sup>

<sup>†</sup>*California State University*    <sup>‡</sup>*Nottingham University Business School*

<sup>††</sup>*Lancaster University Management School*

Final version received 23 May 2007.

Using data from England and Wales, we test the hypothesis that legalizing abortion reduces crime. The timing of changes in crime rates in aggregate data is generally inconsistent with this hypothesis. Using panel data on recorded crime from 1983 to 2001, we are able to replicate the negative association between abortion rates and reported crime that J. J. Donohue and S. D. Levitt found for the United States. However, this association breaks down under the scrutiny of robustness checks and is not present when we examine data on convictions broken down by age. Overall, we find no clear, consistent relationship between abortion and crime in England and Wales.

## INTRODUCTION

During the 1990s the United States witnessed a large, abrupt drop in crime rates across virtually all categories and in all regions. Between 1991 and 1999 murder rates fell by approximately 40% with violent and property crime rates falling by about 30%.<sup>1</sup> This dramatic and unexpected fall in crime led to a scramble by researchers for an explanation, and, while several factors may have played a role in the reduction of crime (e.g. increased police presence, strong economic growth, reduction in crack cocaine), none of them appear capable of explaining the magnitude and timing of the decline (Levitt 2004).

Donohue and Levitt (2001) (henceforth D&L) offer an explanation. The authors argue that the legalization of abortion some fifteen to twenty years earlier may be a large part of the answer. Their hypothesis (discussed in greater detail below) is that legalized abortion ultimately reduced the birth of children who, had they have been born, would have been at greater risk of committing crimes when they reached their teenage years. Several authors (Joyce 2004a, b, 2006; Foote and Goetz 2006; Lott and Whitley 2007) have subjected the D&L research to re-analysis, focusing on issues concerning both data and empirical techniques and casting some doubt on the robustness of the original D&L findings. In turn, Donohue and Levitt have responded (2004, 2006), claiming that the results of the re-analyses continue to support their hypothesis of a link between abortion and crime.

The vast majority of work subsequent to D&L (2004) used the United States as a setting. Given the current state of disagreement about what inferences can be drawn from the US data, an obvious way forward is to try to test the D&L hypothesis in other settings. To date, there have been very few attempts at such an exercise. Pop-Eleches (2006) finds that cohorts born after the abortion restrictions introduced in Romania in 1970 were more likely to commit crimes than those born before the restrictions. However, he is unable to identify a causal relationship from abortion restrictions, owing to the possible influence of the transition to capitalism on crime among the affected cohorts. Sen (2002) examines data from Canada and finds support for the D&L hypothesis with regard to violent crime for the period 1983–98, but not property crime. One problem with

Sen's paper, however, is that much of the variation in abortion access comes after 1988, when, following a Canadian Supreme Court decision, abortion was made widely available to women in Canada.<sup>2</sup> This being the case, the 1988 law change occurred too late for it to have an effect on crime in the 1983–98 period he examines. Moreover, Sen's paper does not present estimates by single-year-of-age, as found in D&L. Finally, Leigh and Wolfers (2000), discussing federal data from Australia, noted that a satisfactory analysis of the question in the Australian context is limited by the apparent lack of availability of useable crime and abortion data at a more geographically disaggregated level.

The goal of this study is to test the D&L hypothesis with data from the United Kingdom. The UK is a particularly useful context in which to examine this issue for several reasons. In the first place, the United Kingdom legalized abortion in 1968, some years before most other Western countries; as a result, we have a longer time period over which to examine any possible impacts on crime. More importantly, a particular feature of the UK legislation is that there is a statutory requirement for every abortion to be reported. A consequence of this is that abortion data are of very high quality. In contrast, the analyses of abortion and crime in the United States have had to rely on survey data that are incomplete and, in some cases, of questionable quality. In addition to the abortion data in the UK being complete, much more detail is available than from most other countries. Abortion data are available broken down by age of mother, marital status and whether or not the mother had to travel outside her area of residence to obtain the abortion. This level of detail allows us to test whether any relationship between abortion and crime is stronger for particular groups, e.g. abortions performed in local area of residence, teenagers and so on. The final advantage of the UK context is that institutional factors facilitating abortion have been subject to change both across regions and over time. This provides a useful source of exogenous variation in abortion rates with which to identify any abortion–crime relationship.

The timing of changes in crime rates in aggregate data is generally inconsistent with this hypothesis, as are comparisons with areas close to England and Wales in which abortion remained illegal. On the other hand, using panel data on recorded crime in 42 police force areas in England and Wales from 1983 to 2001, and estimating a model quite similar to D&L's US model of arrest rates, we do find a negative association between recorded crime and effective abortion rates. However, this association largely breaks down under the scrutiny of various robustness checks. Furthermore, we are not able to find any crime-reducing effects from legal abortion when using convictions data broken down by age for police force areas over the period 1984–2004, using a similar model to that of D&L for US conviction rates. These results generally do not provide robust, statistically significant evidence that legalized abortion has led to reduced crime in the United Kingdom, a result that stands in contrast to D&L, and which provides the first reliable evidence on the topic from outside the United States.

The remainder of the paper is organized as follows. Section I provides greater detail of the D&L hypothesis. Section II gives a brief history of abortion policy in the United Kingdom; it also presents a set of time-series graphs which allow us to offer a visual display of the timing of abortion legalization and its potential effects on crime some twenty years hence. Section III follows with our model of crime and discusses the covariates included and their expected effects on crime rates. Section IV discusses the estimation approach, describes the data-set and presents our empirical findings. Concluding thoughts are found in Section V.

## I. THE ABORTION–CRIME RATE LINK

The D&L hypothesis that changes in abortion rates may lead to changes in crime rates some years later encompasses two propositions: that changes in the abortion rate will change the *size* of birth cohorts, and that they will change the *composition* of the cohort. Only one of these propositions needs to be valid for changes in abortion rates to have an effect on crime rates. The first proposition straightforwardly states that if the abortion rate increases then, *ceteris paribus*, the birth cohort will be smaller, thereby leaving fewer individuals to commit crimes later. But it is well known that abortion in the United States probably had only minimal effects on the number of children born, calling this first proposition into question. Hence it is the second proposition that D&L stress in their paper, and this is clearly more controversial. A strong link between abortion and crime may be revealed if a *selection effect* is operational.

D&L suggest that abortion may significantly alter the composition of birth cohorts in a way that leaves relatively fewer potential criminals than if abortion were not legally available. The reasoning behind this is twofold. First, legalized abortion allows women to better manage their fertility. That is, if a woman becomes pregnant at an inopportune time (e.g. during temporary financial difficulties, or while a student), she can terminate the pregnancy and perhaps bear children later when conditions are more favourable. The second (and somewhat related) reason why abortion may reduce crime has to do with the kind of environment into which children would be born if abortion were not available. Gruber *et al.* (1999) consider the kind of life that such a ‘marginal child’ might have experienced if abortion had not been the chosen course of action by the pregnant mother. Based on research using state-level data from the 1980 US census and US Vital Statistics, they note that the marginal child would likely have faced multiple hardships, such as being ‘60% more likely to live in a single parent home, 50% more likely to live in poverty, 45% more likely to be in a household collecting welfare, and 40% more likely to die during the first year of life’ (p. 265). D&L argue that legalized abortion may have reduced exposure to such hardships and ultimately caused a reduction in crime some fifteen to twenty years later. Or, as Levitt and Dubner summarize it in their best-selling book *Freakonomics*, ‘Legalized abortion leads to less unwantedness; unwantedness leads to high crime; legalized abortion, therefore, led to less crime’ (2005, p. 139).

## II. ABORTION AND CRIME IN THE UK

The 1967 Abortion Act decriminalized abortion in Great Britain under certain conditions, the most important being that two doctors had to state in good faith that continuation of the pregnancy would involve a risk, greater than if the pregnancy were terminated, of injury to the physical or mental health of the pregnant woman or to any existing children of the pregnant woman. Many doctors have interpreted this clause as permitting abortion in any case. Prior to 1967, and following a landmark Court Case in 1938, a small number of abortions were permitted on serious medical grounds.

The Abortion Act came into force in April 1968 and applied to England, Scotland and Wales, but not to Northern Ireland. The only major amendment to the Act came in 1991, when a formal time limit on abortions of 24 weeks was imposed, with the exception of certain cases, most particularly when the foetus was thought to be at risk of being born with physical or mental abnormalities, in which case abortion is permitted at any stage.

During 1969, the first full year in which the Abortion Act was in place, 49,829 abortions were carried out on residents in England and Wales. This increased rapidly to

over 100,000 by 1972 and then more gradually to about 170,000 in 1989. From this point onwards, the annual number of abortions on residents has remained relatively stable.

A natural question is whether the 1967 Act actually led to a significant increase in the total number of abortions. For obvious reasons, illegal abortions are not reported in official statistics. However, based on maternal deaths and morbidity statistics, Goodhart (1969, 1972) concludes that between 10,000 and 20,000 illegal abortions were taking place in the United Kingdom each year, although a proportion of these would have been on overseas residents.<sup>3</sup> Further, Cavadino (1976) presents strong evidence that significant numbers of illegal abortions continued to take place even after legalization in 1968. Taken together, the evidence suggests that the 1967 Abortion Act led to an increase in the order of magnitude of abortions being carried out on residents in England and Wales. Certainly, the likelihood of measurement error from illegal abortion needs to be borne in mind when interpreting our empirical estimates below. On the other hand, a particularly relevant feature of the 1967 Act for this paper is the dramatic increase in abortions provided free of charge on the National Health Service (NHS). Prior to the Abortion Act, around 3000 abortions were performed in NHS hospitals in the United Kingdom per year (Royal College of Obstetricians and Gynaecologists 1966), while by 1972 this figure had risen to around 57,000. Hence there was a major shift in the availability and monetary cost of abortions for women in socio-economic groups in which crime is most prevalent.

#### *Data description*

The panel data employed covers the 42 police force areas in England and Wales over the years 1983–2001 for recorded crime and from 1984–2004 for cautions plus guilty (C + G) data. ‘Recorded crime’ denotes offences that are officially recorded by police authorities in England and Wales. The Home Office defines a caution as follows: ‘If a person admits to committing an offence, he may as an alternative to court proceedings be given a formal police caution, by or on the instructions of a senior police officer.’ Informal or written warnings, often used in cases of traffic violations or shoplifting, are excluded. Only indictable offences are recorded. In 1985 the Home Office issued a directive urging greater use of cautions, and this may have resulted in a trend away from numbers found guilty towards cautions. The term ‘guilty’ refers to any conviction at a Magistrates’ Court or Crown Court. In England and Wales all but a few cases are initially heard at a Magistrates’ Court. If cases are deemed sufficiently severe, they are referred from the Magistrate’s Court to a higher-level Crown Court. Our data combine all guilty verdicts (convictions) from magistrates’ and Crown Courts. The cautions and guilty data are

TABLE 1(a)  
PERCENTAGE CHANGES IN RECORDED CRIME FOR ENGLAND AND WALES, BY CATEGORY AND OVER SUB-PERIODS, 1983–2001

	1983–87	1988–92	1993–97	1998–2001	1983–2001
Total	19.1	55.5	–21.8	0	28.9
Property	28.1	57.9	–24.3	–5.1	20.5
Robbery	38.6	98.7	7.2	72.7	484.8
Violent	28.2	24.3	20.1	28.3	99.5

TABLE 1(b)  
DESCRIPTIVE STATISTICS

	Mean	St. dev.	Minimum	Maximum
Recorded offence rates				
Total	78.58	27.86	28.49	160.87
Property	57.75	21.19	17.39	126.48
Robbery	0.58	0.78	0.03	7.21
Violent	3.63	1.61	1.18	13.03
<i>Cautions plus guilty rates</i>				
Ages 15–24	3.64	1.29	0.81	11.10
<i>Covariates</i>				
Real average wage (£1983)	195.1	25.38	148.3	327.6
Male unemployment (%)	8.00	3.97	0.81	22.50
Cars	374.55	70.34	206	673
Police	2.16	0.44	1.55	4.64
Children in care	4.98	1.41	2.10	9.90

*Note:* All reported values refer to annual observations at police force area level for 1983–2001 with the exception of cautions plus guilty, which are for 1984–2004. All variables are weighted by 1000 population in police force areas except for real average wage, male unemployment rate and children in care. All covariate values refer to 798 observations over the period 1983–2001.

TABLE 1(c)  
SUMMARY OF VARIATION IN KEY VARIABLES

Variable		Mean	St. dev.
Log of recorded crime	Overall	-2.836	0.358
	Between		0.303
	Within		0.197
	Diff.-in-diff.		0.099
EAR (effective abortion rate)	Overall	0.092	0.051
	Between		0.018
	Within		0.046
	Diff.-in-diff.		0.048

broken down by single year of age.<sup>4</sup> Scotland is excluded because of its separate judicial system, which generates crime statistics that are not compatible with England and Wales. The reporting rules for recorded crime changed considerably in 2002, and it is not possible to calculate a consistent series after this point.<sup>5</sup> Aggregate data for the whole of England and Wales are available from a slightly earlier time period for both data-sets. Table 1(a) shows a breakdown of recorded crime by type and period, and Table 1(b) provides summary statistics for recorded crime rates, C + G rates and the covariates; in Table 1(c) we report summary statistics for the log of total crime and for effective abortion rates, showing how each of these varies within regions over time, between regions and also between regions over time.

Figure 1 shows the time-series plot of total, violent and property crime, normalized to 1983, for the period covered in this study. Total and property crime follow almost exactly the same pattern as that reported for the United States by D&L (see their Figure II, p. 392). Both decreased slightly between 1986 and 1988, increased rapidly up to 1992 and

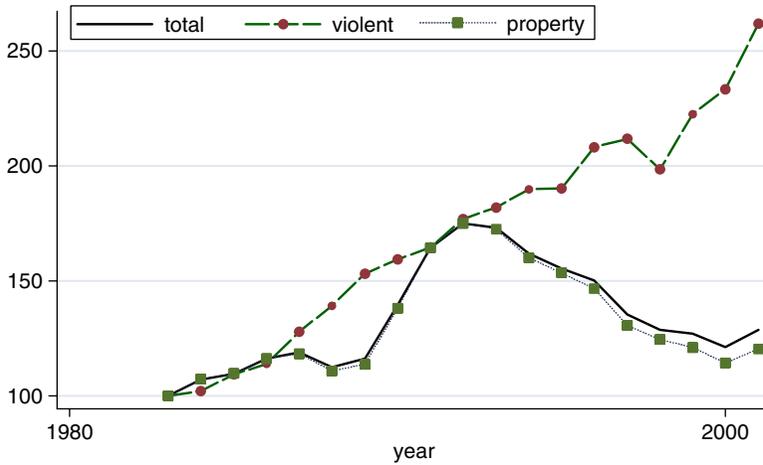


FIGURE 1. Recorded total, violent and property crime rates, England and Wales, 1983–2001

Note: ‘Violent’ refers violent crime and robbery combined; ‘property’ to burglary and theft combined.

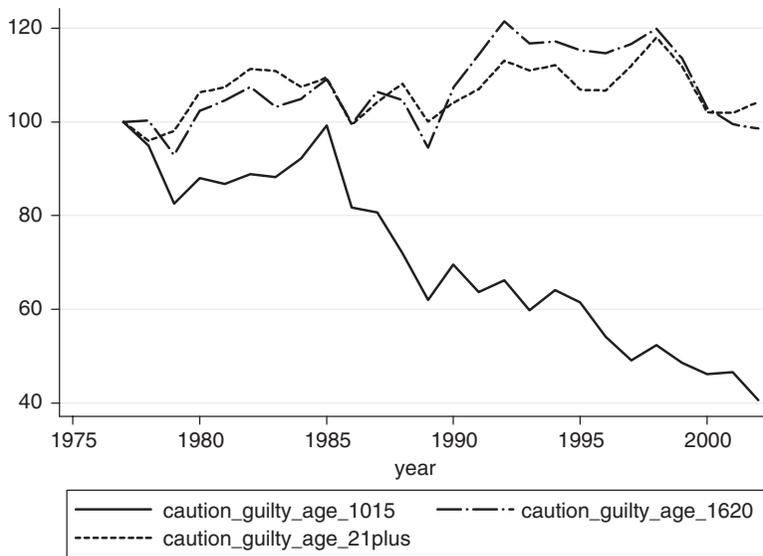


FIGURE 2. Total cautions and guilty by age in England and Wales.

then experienced a significant decline thereafter. Violent crime, on the other hand, increased more or less consistently over the whole period, a pattern that contrasts strongly with that reported for the United States. Figure 2 reports a similar time-series plot for total number of cautions plus the total number of people found guilty in the courts that year, expressed as rates per 100,000 population and then as index numbers with 1977 = 100. We show plots for three age groups: ages 10–15, 16–20, and 21 and over. These age bands are constructed on the basis of similarity in graph plots by particular year of age. The plots are not supportive of a link between abortion and crime. The decrease in C + G for the 10–15 age group seems unlikely to be due to abortion, as

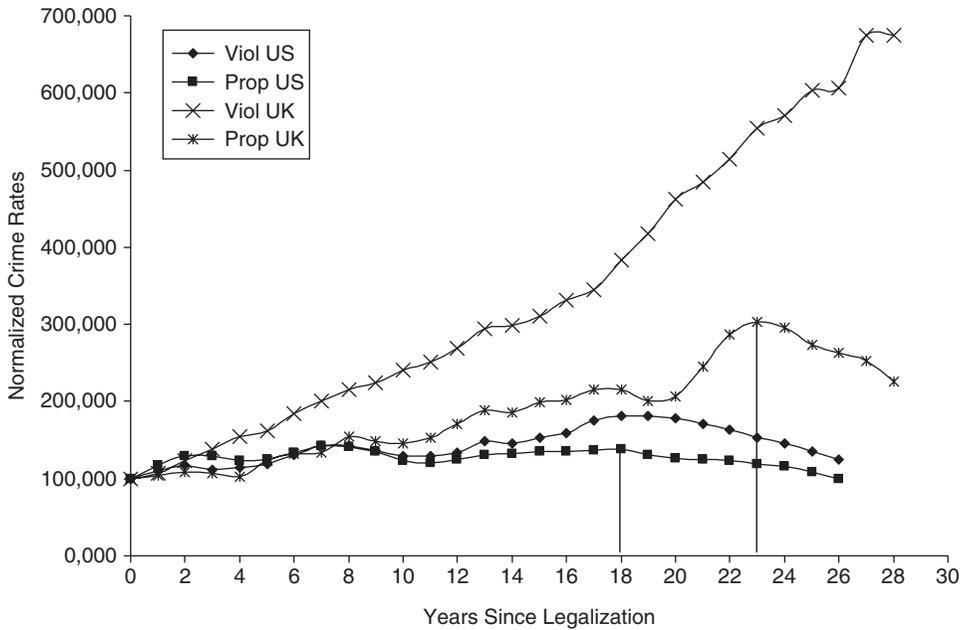


FIGURE 3. Violence and property crime rates for the United States and England and Wales since abortion legalization

*Note:* Data are indexed to be 100 for the first full year in which abortion was legalized (1973 for the USA, 1969 for England and Wales).

the plot decreases too late; if abortion plays a part, the decrease should start earlier, around 1980–2, unless there is a considerable delay in abortion impact. The plots for the 16–20 and 21 and over categories show no downward trend and appear to move up and down together. For abortion legalization to have an effect, we would predict that one plot would follow the other downwards.

The significant decreases in murder, violent and property crime in the United States starting in 1991—18 years after the end of the 1973 Supreme Court ruling abolishing restrictions on abortion—are a key part of the D&L case that abortion legalization reduced crime. As abortion in the United Kingdom was legalized five years earlier than in the United States, if legalization has the causal effect of reducing crime, we would expect the downward trend in crime to have occurred five years earlier in the former country than in the latter. The fact that, at least for total and property crime, we observe almost exactly the same timing in crime trends in the United Kingdom is somewhat problematic for an abortion–crime link. To illustrate this timing inconsistency, Figure 3 plots violent and property crime rates in the both countries, each normalized to be 100 in the first full year of abortion legalization (i.e. 1973 for the United States and 1969 for the United Kingdom). The horizontal axis shows the number of years since legalization. As seen in the US series, the turndown in both categories occurs at about the eighteenth or nineteenth year. The UK series behaves quite differently. Property crime rates show a slight dip in the nineteenth year, but then continue upward until the turning point in the twenty-third year. The vertical lines highlight the five-year difference in the local maximums for property crimes in the United States and the United Kingdom. Violent

crime behaves altogether differently, as the plot exhibits a more or less steady rise until the twenty-fifth year.

There are (at least) three alternative explanations for the observed patterns in crime trends across the two countries:

- (i) Abortion legalization reduced crime in the United States but not the United Kingdom.
- (ii) Abortion did reduce crime in the United Kingdom, but other factors have swamped the time-series relationship.
- (iii) Abortion did not reduce crime either in the United Kingdom or the United States, and any negative correlation between abortion and crime is spurious.

In the regression analyses below we test the first hypothesis by attempting to correlate changes in abortion rates within regions of the United Kingdom with changes in subsequent crime rates. A significant negative correlation between abortion and crime rates (as found by D&L for the United States) would be evidence against a differential effect between the two countries.

The fact that abortion remained illegal in states that are geographically and culturally close to Great Britain provides another useful point of comparison for identifying the impact of abortion legalisation on crime. In particular, the 1967 Abortion Act did not apply in Northern Ireland. Similarly, no comparable legislation was passed in the Republic of Ireland. In Figure 4 we report total recorded crime in four regulatory regimes between 1983 and 1997, normalizing crime at the start of the period to 100. In two of these regimes (England/Wales and Scotland) abortion was effectively legalized in 1968, while in the other two (Northern Ireland and the Republic of Ireland) it has remained illegal throughout the sample period.<sup>6</sup> If legalisation of abortion reduces crime, we might expect that crime in England/Wales and in Scotland *decreases relative* to the other two areas from the mid-1980s onwards. In fact, if anything the graph suggests a relative increase in crime, at least in the start of the period, in the areas affected by abortion legalization.

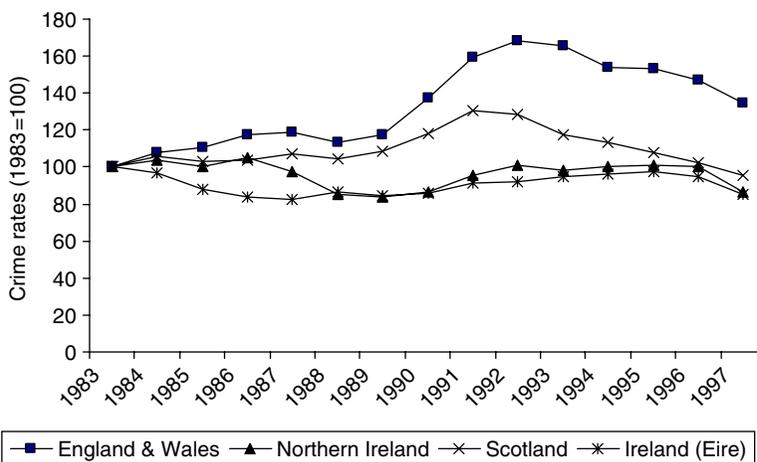


FIGURE 4. Recorded crime rates 1983–1997: England and Wales, Northern Ireland, Scotland and Ireland.

D&L support their hypothesis that legalization of abortion led to an eventual reduction in crime, in part by presenting time plots showing that the downturn in crime in the United States occurred approximately seventeen years after legalization, leading them to conclude that:

The timing of the break in national crime is consistent with a legalized abortion story. In 1991 the first cohort affected by *Roe v. Wade* would have been roughly seventeen years old, just beginning to enter the highest crime adolescent years. (D&L, p. 393)

Similar graphs for England and Wales, including those that draw comparisons with areas in which abortion has remained illegal, do not yield the same support for an abortion-crime link. The ability of time-series graphs to identify any crime-reducing effects of legalized abortion is limited, as other national effects may crowd out the effects of abortion on crime. Hence a multivariate econometric approach is needed to control for various changes that might impact on crime, including variations in indicators of economic performance (real earnings, unemployment), social deprivation and policing.

#### *Construction of effective abortion rates*

In contrast to many parts of the United States, health authorities in the United Kingdom have a statutory requirement to report every legal abortion. As a consequence, a complete set of data is available from the Department of Health on the number of abortions performed in each region, broken down by the age, marital status and place of residence of the mother.

We follow Donohue and Levitt in constructing measures of effective abortion rates, weighted by the age profile of the criminal population. Abortions can be expected to have an impact on crime rates only when the children who have been aborted would have been old enough to commit crimes. Hence we first calculate the annual abortion rate as a proportion of live births in each police force area. By using lagged values, this provides us with the abortion *ratio* in area  $i$  for any age  $j$  in the current year  $t$ . The potential impact of a high abortion rate for any particular cohort will depend on how prevalent crime is among that cohort. For this reason, we calculate the effective abortion rate ( $EAR_{it}$ ) as the weighted sum of the abortion rate of each age group:

$$(1) \quad EAR_{it} = \sum_{age} Abortion\ Ratio_{i,t-age-1} (Arrests_{age} / Arrests_{total}),$$

where  $(Arrests_{age} / Arrests_{total})$  is the proportion of crime that is committed by an *age* cohort in area  $i$ . The weights are derived from data published by the Home Office on numbers cautioned or found guilty for different crime categories for each area in each year. The weights we use vary for different crime categories and for each of the 42 police force areas, but are constant across years.<sup>7</sup> If abortion really does have an impact on crime, then the age distribution of crime will be endogenous to abortion. This would suggest that we construct the weights using data from a period before abortion could have had an impact, and this is the procedure followed by Donohue and Levitt. However, the age distribution of crime may also vary over time for other reasons. In that case, relying on pre-abortion weights may bias the results. We experiment with three different sets of crime weights. The first uses data from the early part of the estimation period, before abortion could have affected the age distribution of crime, to calculate the weights. The second uses data from the end of the sample, while the third is the mean of

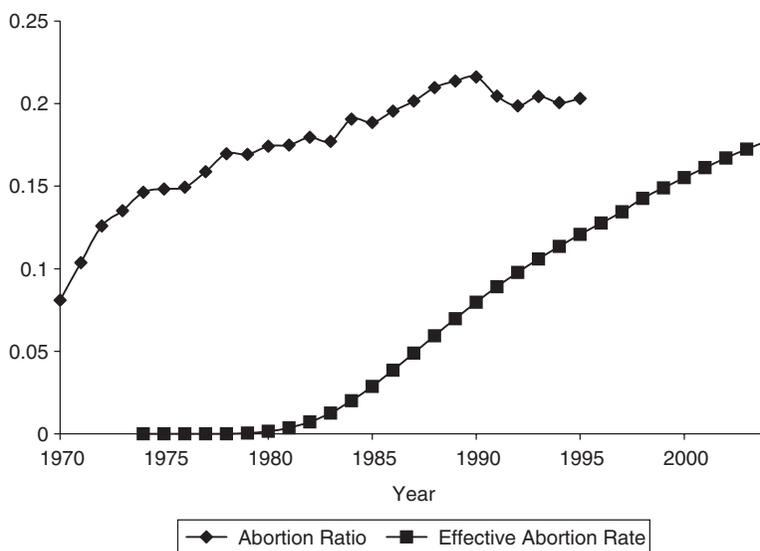


FIGURE 5. Abortion ratio and effective abortion rates across time.

the first two. In practice, we find that our results are not at all sensitive to the choice of weights, and all the results reported here are based on the first set, which allows for a more direct comparison to the D&L results. Figure 5 plots the abortion ratio (abortions to 1000 live births) from 1968 to 1995, and the effective abortion rate up to 2004.

In our econometric work below, we exploit the quality of the British abortion data by using alternative measures of effective abortion rates for each of (i) abortions on single women, (ii) abortions on teenagers and (iii) abortions performed in the health authority of residence. Regressions shown below use effective abortion rates and an appropriate set of control variables to identify abortion effects by cross-area variation in effective abortion rates. Temporal variations will then be captured by inclusion of year dummies. We can then separate out the effects of abortion from impacts on crime from other national effects, including cohort impacts on crime.

### III. THE MODEL OF CRIME

In order to investigate the possible linkage between recorded crime and abortion we consider the following model (which bears close resemblance to D&L):

$$(2) \quad \ln(\text{Crime}_{it}) = \beta_0 + \beta_1 \text{EAR}_{it} + X_{it}\Gamma + \gamma_i + \nu_t + \varepsilon_{it},$$

where  $\text{Crime}_{it}$  is the per capita crime rate (total and by sub-category) for police force area  $i$  in year  $t$ . The use of area per capita crime rates in all our regressions is appropriate for the testing of an abortion selection effect on crime. The measure  $\text{EAR}_{it}$  is the effective abortion rate, as described above. The vector  $X_{it}$  contains a number of covariates designed to control for various factors that may affect crime rates. The variables  $\gamma_i$  and  $\nu_t$  are included to control for police force area and year fixed-effects, respectively.

*Covariates*

The variables included in vector  $X_{it}$  are motivated by work on the economic analysis of crime pioneered by Becker (1968) and Ehrlich (1973) (see Freeman 1999 for a survey). An individual faces a choice between legal market work and crime. Crime is chosen when the difference between the expected return from crime and the expected return from market work exceeds an exogenous threshold.

In our data-set we have region-level statistics on two variables that capture labour market opportunities (log average weekly earnings and male unemployment rate) and a proxy for regional wealth given by the number of cars per capita.<sup>8</sup> The net effects of regional income and unemployment on crime are ambiguous, since increased expected earnings will raise the demand for crime but will reduce the supply of crime as expected returns from market work rise relative to expected returns to crime. The use of both average earnings and unemployment rate as covariates should in principle allow us to obtain an estimated effect of unemployment on crime through the supply of crime, rather than through the indirect negative effect via demand for crime (Edmark 2005). Hence we predict that male unemployment has a positive influence on crime rates.<sup>9</sup>

Increased wealth raises the demand for crime as more 'loot' is available. We have a useful proxy for regional wealth in the form of number of registered motor vehicles by private individuals per capita (Witt *et al.* 1999). An argument can be made for a negative effect of wealth on crime. As people accumulate more wealth, so they are also likely to spend more on measures to protect their assets, such as burglar alarms and car alarms. These devices raise the cost of committing crime and reduce the supply of crime. Hence the impact of wealth on crime does not occur solely through the demand for crime, and the sign of impact is an empirical issue.

The supply of crime is predicted to fall as the expected costs of punishment rise. We have just one measure of cost of punishment, which is the number of police in a police force area. Increased police strength is predicted to lower crime rates. We endeavour to reduce the endogeneity bias identified by Levitt (1997), i.e. that police strength may respond positively to increased crime rates as citizens demand greater police protection, by using the log of number of police per capita, lagged one period.

A further covariate that we use, not present in D&L, is a measure of social deprivation: the rate, per 1000 people under 18 years, of children in local authority care. In England and Wales, children whose parents are deemed unfit to provide for care (e.g. because of violence or drug abuse) can be placed under the care of local authorities, either in care homes or with foster parents. The proportion of children in such care is a proxy for poor social conditions which might be conducive to increased crime since the stocks of human and social capital will be relatively low in areas with greater social deprivation.<sup>10</sup> Finally, we include the percentage of the regional population aged 15–24 to control for age cohort effects on crime.

#### IV. ESTIMATION APPROACH AND EMPIRICAL FINDINGS

*Estimation approach*

We begin our estimation of equation (3) by utilizing a similar method employed in D&L. Regressions are performed for total recorded crime rates and separately for three sub-categories: violence, robbery and property crime.<sup>11,12</sup> Fixed-effects regressions are estimated with controls for year-specific effects, weights based on 1990 population data,

TABLE 2  
FIXED-EFFECTS REGRESSIONS FOR RECORDED CRIME, ENGLAND & WALES, FOR 42 Police  
Force Areas, 1983–2001

Variable	Crime category			
	Total	Violent	Robbery	Property
Effective abortion rate	– 1.515** [0.717]	1.469*** [0.493]	– 4.231*** [0.691]	– 1.305*** [0.368]
ln (average real wage)	0.121 [0.142]	– 0.015 [0.225]	0.712** [0.319]	0.217* [0.130]
Unemployment rate	0.015*** [0.005]	0.003 [0.009]	– 0.011 [0.007]	0.013*** [0.005]
ln (cars per 1000 people)	– 0.132*** [0.049]	0.087 [0.119]	– 0.069 [0.129]	– 0.166*** [0.053]
ln (police per capita ( $t - 1$ ))	– 0.430*** [0.145]	– 0.407* [0.218]	– 0.455** [0.197]	– 0.396*** [0.112]
Children in care (4 yr moving avg.)	0.016* [0.009]	0.012 [0.016]	– 0.047*** [0.017]	0.012 [0.010]
% Population aged 15–24	0.0186 [0.0254]	0.0112 [0.0289]	0.0368 [0.0358]	0.0338 [0.0219]
Constant	– 2.873*** [0.916]	– 6.409*** [1.583]	– 11.107*** [1.968]	– 3.603*** [0.811]
Observations	798	798	798	798
Number of areas	42	42	42	42
R-squared	0.995	0.997	0.998	0.995

Notes: Panel-corrected standard errors in brackets. Dependent variables are measured as the natural logs of the per capita rate. All regressions include year and area dummies and are estimated assuming a within-panel AR(1) error structure.

\*Significant at 10%; \*\*significant at 5%; \*\*\*significant at 1%.

and allowing for an AR(1) within-panel error structure. Given the possibility of contemporaneous correlation, panel-corrected standard errors are reported. We then proceed with various robustness checks; we include regressions excluding London (which is an outlier in several ways), experiments with various weighting schemes, estimation in first differences, and regressions using alternative abortion measures.

#### *Empirical results of effective abortion ratio regressions*

Table 2 contains the estimation results for the recorded crime data (by total and sub-categories). Among the covariates, unemployment rates have a positive and significant effect on total and property crime. The log of police per capita (lagged one period) has a consistently negative and significant effect on crime in all the regressions. The estimated coefficient for the log of the average real wage is positive and significant for the robbery and property categories. The coefficient on children in care is positive and significant for total crimes—which is in line with our hypothesized relationship—but is negative and significant for robbery, which is contrary to our expectations. Our proxy for wealth, the log of cars per 1000 people, has a negative and significant coefficient for total and property crimes.

Effective abortion rates are associated with significantly lower recorded rates of total crime and of robberies and property crime. Abortion, however, is positively and significantly associated with the violent crime rate. Focusing on total recorded crimes, our estimates imply that the increase in effective abortion rates from 0 to the mean value at the end of the sample period ( $= 0.161$  for total crime) has led to a reduction in total crime of about 24.4%. This result appears to contradict the impression of a lack of correlation between abortion and crime given by the time-series plots for England and Wales in Figure 2. However, taken alone, these figures would suggest that the impact of abortion on crime in England and Wales is much greater than that found by D&L in the United States. Given that abortion rates in the England and Wales have historically been a little lower than in those for the United States, the magnitude of the coefficients is somewhat implausible. For this reason, we next investigate the sensitivity of this result to the various robustness checks.

Table 3 reports results from a series of robustness checks. We begin with two checks also employed by D&L. First, we estimate our total recorded crime regression without the covariates. The results, provided in the first column of Table 3, show that the estimated coefficient for the effective abortion rate is now smaller (in absolute terms) than before and is no longer statistically significant. Second, in an effort to reduce the possibility of omitted variables bias, we re-estimate our total crime regression and include regional dummies interacted with year dummies. The regression results, shown in column (2) of Table 3, provide a slightly larger coefficient for abortion (in absolute terms), but little change otherwise.

Our next checks attempt to examine the impact of migration and immigration. Some people who are exposed to abortion in one area may have moved to another area when crime rates were being measured. Similarly, some people committing crime will have been born in another country entirely. Inevitably this introduces measurement error into the regressions and may bias the results. Some data on national and international migration are published by the Office of National Statistics, although these are not available on a consistent basis throughout our sample. However, the data do make clear that the problem is much more significant for London, which is characterized by an extremely mobile population relative to other areas. Hence our robustness experiment is to exclude London. A further feature of London is that its population is approximately four times larger than the next most populous police force authority. In the context of regressions weighted by population, London may have an unrepresentative influence on regression results. For this reason, our next experiment was to conduct regressions on unweighted data.<sup>13</sup> The results of these tests are shown in columns (3)–(5) in Table 3. In each case, the coefficient for the effective abortion rate is not statistically significant while the coefficients for the covariates are not substantially different from those reported for total recorded crime in Table 2.

The next robustness checks are to estimate the model using abortions to single women and to teenagers. To the extent that crime is relatively more prevalent among offspring of these groups, one would expect any correlation between abortion and crime to be strongest in these regressions. These are followed by a regression that uses an effective abortion rate, computed for abortions performed in the health authority of the woman's residence. Lastly, as Wooldridge (2002) notes, the first-difference estimator will be more efficient than the fixed-effects estimator in the context of serial correlation; consequently our final robustness check reports estimates using first differences.<sup>14</sup>

The results of these robustness checks, presented in columns (6)–(9) of Table 3, are remarkably consistent. The covariates display a steady pattern, with coefficients

TABLE 3  
ROBUSTNESS CHECKS FOR TOTAL RECORDED CRIME, ENGLAND AND WALES, FOR 42 POLICE FORCE AREAS, 1983–2001

Variable	Specification									
	No covariates	With year–region interactions	No London	Unweighted	Unweighted, no London	Single women	Teenage	Home	First differences	
Effective abortion rate	- 1.13 [0.736]	- 1.635* [0.947]	1.034 [0.975]	- 0.571 [0.618]	0.697 [0.785]	- 0.704 [0.693]	- 5.934 [3.634]	- 0.816 [0.662]	- 1.578 [1.817]	
ln (average real wage)		0.234* [0.127]	0.156 [0.122]	0.122 [0.110]	0.131 [0.111]	0.026 [0.155]	0.019 [0.147]	- 0.013 [0.150]	0.039 [0.133]	
Unemployment rate		0.015** [0.006]	0.020*** [0.005]	0.013*** [0.004]	0.014*** [0.004]	0.014*** [0.005]	0.013*** [0.005]	0.012** [0.005]	0.014** [0.006]	
ln (cars per 1000 people)		- 0.113** [0.051]	- 0.123** [0.048]	- 0.112** [0.046]	- 0.116** [0.047]	- 0.104** [0.049]	- 0.105** [0.050]	- 0.095* [0.050]	- 0.110** [0.044]	
ln (police per capita ( $t - 1$ ))		- 0.064 [0.120]	- 0.272** [0.108]	- 0.273*** [0.101]	- 0.218** [0.108]	- 0.386** [0.152]	- 0.399*** [0.148]	- 0.389*** [0.150]	- 0.420** [0.169]	
Children in care (4 yr moving avg.)		0.009 [0.009]	0.01 [0.009]	0.013 [0.008]	0.009 [0.008]	0.023** [0.011]	0.023** [0.010]	0.025** [0.010]	0.01 [0.013]	
Percent population aged 15– 24		0.0343* [0.0192]	- 0.0036 [0.0194]	- 0.0201 [0.0166]	- 0.0275 [0.0171]	0.0161 [0.0267]	0.0134 [0.0254]	0.0139 [0.0262]	0.0178 [0.0374]	
Constant			- 2.910*** [0.784]	- 2.443*** [0.702]	- 2.397*** [0.718]	- 2.575*** [0.957]	- 2.462*** [0.917]	- 2.374** [0.926]	0.156*** [0.033]	
Observations	798	798	779	798	779	798	798	798	756	
No. of areas	42	42	41	42	41	42	42	42	42	
R-squared (overall)	0.994	0.997	0.995	0.973	0.974	0.994	0.995	0.994	0.601	

Notes: Panel-corrected standard errors in brackets. Dependent variables are measured as the natural logs of the per capita rate. All regressions include year and area dummies and are estimated assuming a within-panel AR(1) error structure. \*Significant at 10%; \*\*significant at 5%; \*\*\*significant at 1%.

on unemployment being positive and significant in all regressions, whereas coefficients on cars and police are negative and significant in virtually all regressions (though the coefficient on police loses significance in the regression with the area–year interactions). Children in care shows a positive and significant coefficient for the ‘single women’, ‘teenage’ and ‘home’ regressions which lends support to our hypothesis that, in areas where social conditions are worse, crime rates tend to be higher, all else equal.

Of greatest interest for all the robustness checks shown in Table 3, however, is that in all cases (other than the regression regional dummies–year interactions) the estimated coefficient on the effective abortion rate is generally lower in magnitude than before, varies in sign, and is never statistically significant.

### *Results from area–year–age regressions*

We now turn to results of crime rate regressions using the cautions-plus-guilty data. These data are organized by single year of age for the ages 15–24.<sup>15</sup> As in D&L, we use the actual abortion rate rather than the ‘effective’ abortion rate used for the recorded crimes. Fixed-effects regressions for total crimes are reported, followed by a series of robustness checks. The use of the age-related data is important for at least two reasons. First, these data allow nationwide variation in access to abortion (in particular the introduction of legal abortion in 1968) to identify the abortion–crime relationship, while still controlling for time and regional fixed effects. So, for example, if legalized abortion first affected births in 1969, we would expect crime among 20-year-olds to decrease in 1989 relative to older age groups. Second, the use of age-related data enables us to control for effects that are specific to a police force area in a particular year and for effects that are specific to particular age groups in each police force area. The advantage of using area–year regressions based upon data on cautions and convictions is that they allow for explicit controls for omitted area–year factors (the  $\theta_{it}$  terms) that seem to be biasing the effective abortion ratio regressions.

Two of the ongoing points of contention for the D&L by-age regressions have to do with appropriate controls for state–year effects and the appropriate computation of standard errors. Foote and Goetz (2006) note that, owing to a programming error, D&L mistakenly omitted controls for state–year effects and, as a result, ‘cross-state variation contaminates [D&L’s] final test’ (p. 16). Furthermore, D&L estimate their by-age regressions with a clustering by year-of birth and state. However, as pointed out in Bertrand *et al.* (2004) and emphasized in both the papers by Foote and Goetz (2006) and Joyce (2006), computing standard errors by clustering too narrowly places ‘unrealistic restrictions on the correlation or residuals within states and over time’ (Joyce 2006, p. 5). Both Foote and Goetz (2006) and Joyce (2006) advocate clustering at a broader level, namely by state. In order to deal with these issues in our by-age estimates for the C + G data, we included police force area–year dummies, and we compute robust standard errors by clustering on police force areas.

### *Empirical results for cautions plus guilty*

Using data on cautions and guilty verdicts, we estimate the following equation:

$$(3) \quad \ln(\text{Arrests}_{i,t,\text{age}}) = \beta_0 + \beta_1 \text{abortion ratio}_{i,\text{birthyear}} + X_{it}\Gamma + \gamma_{i,\text{age}} + \lambda_{\text{ageband},t} + \theta_{it} + \varepsilon_{it}.$$

TABLE 4  
FIXED-EFFECTS REGRESSIONS FOR TOTAL CAUTIONS PLUS GUILTY, ENGLAND AND WALES, FOR  
42 Police Force Areas, Ages 15–24, 1984–2004

Variable	Specification						
	Total C + G	No London	Unweighted	Unweighted, no London	Single women	Teenage	Home
Abortion rate	0.226 [0.193]	0.992*** [0.308]	0.425 [0.329]	0.781*** [0.257]	0.043 [0.279]	0.992 [1.015]	0.465 [0.364]
Observations	8820	8610	8820	8610	8820	8400	8820
No. of area–age groups	420	410	420	420	420	420	420
R-squared	0.927	0.935	0.919	0.921	0.927	0.929	0.928

Notes: Robust standard errors, clustered by area, in brackets. Dependent variables are measured as the natural logs of the per capita rate.

All regressions include year dummies, year–area interaction effects, area–age interaction effects, a complete set of covariates and interactions of covariates with age dummies. Regressions are weighted using 1990 population values.

\*Significant at 10%; \*\*significant at 5%; \*\*\*significant at 1%.

Table 4 presents the by-single-year-of-age regression results for the cautions-plus-guilty data, including robustness checks. Each regression includes the same set of covariates used in the recorded crime regressions, with the additions of age-covariate interactions to allow for differential impacts of the covariates for each age group. That is, we include the interaction of each covariate with the age dummy to allow for different impacts of covariates for different age groups. Hence these interaction terms are not conflated with the area–year effects. As noted earlier, these are fixed-effects regressions that include both area–year and area–age controls and compute robust standard errors by clustering on area. To control for national changes in the way that crime among different age groups is treated (as suggested, for example, by the series in Figure 3), we divide the sample into three age-bands (15–17, 18–20 and 21–24) and include interaction terms between these age bands and each year.<sup>16</sup> Including these controls still allows nationwide changes to abortion provision (most particularly legalization) that are year-specific to have a differential impact on crime *within* the three age bands. Note that, in the context of England and Wales, where legal abortion was allowed at the same time in every area, including a fixed effect for interactions between years and each single year of age (rather than age bands) would eliminate entirely the impact of abortion legalization on crime. In order to save space, only the coefficients for the abortion rate are reported.<sup>17</sup>

The results displayed in Table 4 are mixed. For total cautions plus guilty, and for four of the six robustness checks, the estimated coefficient to the abortion rate is not different from zero, implying no crime-reducing effect. However, the regressions that exclude London have *positive* and significant coefficients. These effects stand in stark contrast to D&L, and are similar to the results of Lott and Whitley (2007), who find a direct relationship between abortion and murder rates in the United States. These authors argue that their direct relationship may perhaps be explained by the work of Akerlof *et al.* (1996), who note that legalized abortion and improved access to

contraception may have led to greater out-of-wedlock births. If true, and if children of out-of-wedlock births have a greater propensity to a life of crime as they grow older, then a positive relationship between abortion and subsequent crime may be observed.

## V. DISCUSSION AND CONCLUSIONS

The claims in Donohue and Levitt (2001) that the legalization of abortion in the United States in the 1970s led to a subsequent reduction in crime in the 1990s caused a significant stir among economists, criminologists and others. The goal of the present paper was to put the D&L hypothesis to test in a different environment, namely the United Kingdom.

Based on the above analysis, we are unable to say that abortion legalization in the United Kingdom significantly reduced crime in England and Wales some twenty years thereafter. We come to this conclusion by first noting, as we did earlier, that total recorded crime in the United Kingdom began to decrease at about the same time as in the United States, despite the fact that abortion legalization occurred here about five years earlier. Thus, we have a discrepancy in the timing of the potential effect of abortion on crime between the United States and the United Kingdom. Further, crime in England and Wales did not decrease relative to areas in which abortion remained illegal throughout the time period. On the other hand, regression models linking effective abortion rates in the United Kingdom and subsequent recorded crime suggest the same negative and significant correlation between the two variables (at least for total recorded crime and some sub-categories) as that reported for the United States by D&L. However, this negative association breaks down when we consider various robustness checks. Furthermore, when using our C + G data, our results are mixed and indicate either no relationship between abortion and subsequent crime, or a positive one in cases where London is excluded from the analysis—these latter results being opposite to those results found in D&L.

We hypothesized above that this pattern of results might be explained in one of two ways: first, that abortion reduced crime in both the United Kingdom and the United States but that the timing of the relationship in the United Kingdom is obscured by other variables; or, second, that abortion did not reduce crime in either the United Kingdom or the United States and that the negative correlation observed in the regressions was a spurious one. It is this latter possibility that concerns us greatly. In particular, one of the key weaknesses that we believe exists not only in the D&L paper, but in other papers attempting to link abortion rates to crime rates some fifteen to twenty years hence is the distinct likelihood that abortion is endogenous to crime, owing to omitted variables bias. For example, we can consider factors such as education and income levels of women, which may affect the fertility decisions of women and, in particular, the likelihood that an unwanted pregnancy will be terminated. According to research by Finer and Henshaw (2006) on unintended pregnancy in the United States in 2001, women below the poverty level had an abortion ratio (relative to unintended births) of 0.93, whereas the figure for women with income more than twice the poverty level was 1.64. As for education, women with less than high school education had an abortion ratio of 0.90, while women who were college graduates had an abortion ratio of 1.60. To the extent that these same measures—income and education—would have an effect on the human capital endowment of their children, and ultimately the propensity for these children to engage in criminal activity, the exclusion of such measures would bias the estimated coefficient to

abortion. Future research might usefully explore this issue further by attempting to identify explicitly unobservable variables that may explain both variation in abortion rates and variation in subsequent crime.

In any case, puzzles remain that need explanation. In particular, to be able to discount the abortion–crime link satisfactorily, researchers would need to identify more fully the social phenomenon that led to reductions in at least some crime categories in the early 1990s both in the United Kingdom and the United States. The fragility of the results in this paper serve to emphasize the difficulty researchers have in identifying causal effects of social change such as abortion legalization on crime rates some years hence, particularly given the myriad of other social changes occurring over the same time, which may dilute any effect. In general, we believe that an examination of alternative regulatory environments is a fruitful direction for researchers who want to improve our understanding of the link between abortion and crime.

## APPENDIX: DATA SOURCES AND DEFINITIONS

### *Crime variables*

All crime data used were obtained from the Home Office as summarized annually in *Criminal Statistics, Supplementary Tables*, Volume 3, and refer to 42 police force areas in England and Wales. Figures for the two London police force areas, the City of London and Metropolitan Police, were combined.

### *Abortion data*

All data on abortion are taken from the Abortion Statistics annual reference volumes published by the Office for National Statistics.

### *Police strength*

Numbers of police officers in each police force area were obtained from *Police Officer Strength England and Wales*, House of Commons Library Research Paper 01/28, 2001.

### *Population*

Population numbers for police force areas were derived by adding county data taken from *Census of Population 2001*.

### *Real average earnings*

Average earnings are weekly earnings for all full-time adult male workers as given in New Earnings Survey, published annually. The reported employment weights from the survey were used to combine county data into police force areas. The price deflator used in all items is the retail price index, base year 1987, from Office of National Statistics.

### *Male unemployment rate*

This series was obtained from NOMIS, the National On-line Manpower Information Service, based at the University of Durham. The denominator is the male labour force actively seeking work. Labour force weights are used to aggregate counties into police force areas.

*Cars*

Numbers of car registrations are as collected by the UK Driver and Vehicle Licensing Agency and reported in annual editions of *Regional Trends*. Figures are weighted by 1000 population.

*Children in care*

This is the number of children under 18 in local authority care per 1000 resident population under 18. Figures were obtained from *Regional Trends*. Population weights were used to combine counties into police force areas.

## ACKNOWLEDGMENTS

We are grateful to two anonymous referees for many useful comments and suggestions that helped improve the paper. We are grateful to Rob Witt and to participants at seminars at Nottingham University Business School, Lancaster University Management School, University of Surrey Department of Economics, Warwick University and Aston University; to participants at the AEI meeting on abortion and crime, and also to participants at the annual conferences of the Western Economic Association in San Francisco and the European Society for Population Economics in Verona, for many useful comments and suggestions. We also thank Jonathan Klick, Stephen Levine, Ted Joyce and Steve Levitt for valuable comments on earlier drafts. We alone are responsible for the content of the paper.

## NOTES

1. Donohue and Levitt (2001, p.392).
2. According to Sen (2002, p. 5), abortion was technically legal as early as 1969, but women faced significant obstacles. He notes that abortion remained a crime, but could be allowed under special permission if a committee of three doctors at a hospital determined that by not having an abortion the pregnant mother's life or health would be in danger.
3. Other authors suggest that as many as 250,000 illegal abortions took place in the United Kingdom each year. However, higher estimates such as this were usually proposed by organizations campaigning for abortion legalization (e.g. the Birth Control Trust in 1988) and their basis is difficult to establish. As Greenwood and Young (1976) argue, 'Experience shows that wherever legislation has become more permissive there has been an initial & sustained rise in the number of legal abortions. Easier access and acceptance of abortion enables many women who would not have risked the back street market to terminate their pregnancies' (p. 31).
4. Some data on victim reports of crime are available from the British Crime Victim Survey, but these are not available either as a panel or on a consistent annual basis. Although there are concerns about the rigour and accuracy with which reported crime offences are compiled (Macdonald 2002) we have no other source of data appropriate to panel data modelling and close enough to the approach taken by D&L. We assume therefore that any errors in crime reporting do not produce biases in our estimated coefficients and standard errors. Any deviations in crime reporting across police forces or over time will then be picked up in area and year fixed effects. As the C + G data-set is broken down by single year of age, we are able to include fixed effects for each area–year combination in these regressions, and this provides an even more robust control for deviations in annual reporting.
5. The counting rules for recorded crime also changed in 1998. However, the Home Office provides information on how each crime category in each police force authority is affected by the change, and this enabled us to calculate consistent series up to 2001.
6. We are grateful to an anonymous referee for suggesting the comparison with Northern Ireland. Data limitations meant that a more formal difference-in-difference analysis of the control and intervention states was not possible. The graphs end in 1997, as the recording system for Northern Ireland changed considerably after that time.
7. We aggregate the abortion data up to the police force area, the only level at which crime data are available. The police force area boundaries have not changed since 1974. The abortion data are sufficiently disaggregated to allow us to match the 1974 boundaries. For five police force areas the matching is not perfect for data before 1974. In each of these cases only a small part of the police force area was affected. The results reported are robust to exclusion of these areas.
8. See the Appendix for detailed variable descriptions and sources.
9. The use of male unemployment rates is due partly to greater consistency of definition and coverage over time for men and to the fact that the overwhelming majority of crimes are committed by (young) men;

- crime rates are then likely to respond more to male unemployment rates than total unemployment rates. The unemployment rate is not logged in our estimations.
10. Two covariates were experimented with but dropped due to lack of significant coefficients. These were the ratio of 75th to 25th percentile weekly earnings (highly correlated with average earnings) and the proportion of school-leavers having no formal qualifications to proxy low levels of human capital.
  11. Violence includes sex crimes. Property is equal to theft plus burglary. Total crime includes the three sub-categories plus fraud and damage crimes.
  12. We also estimated a dynamic version of equation (3) with a lagged-dependent variable using the method set out in Arellano and Bond (1991). The results, however, produced few significant regressors other than the lagged dependent variable.
  13. We also experimented with the use of an alternative set of weightings based on population stability within each area, estimated from the ONS migration and immigration data. In common with the other robustness experiments, these results also reduce the size of the abortion coefficient and render it insignificant.
  14. In addition, results from a panel unit root test by Levin *et al.* (2002) indicated possible non-stationarity for the recorded violence and sex crime variables. Thus, first-difference estimation may alleviate this problem.
  15. The numbers cautioned or guilty fall very rapidly below the age of 15, and hence we follow D&L and other authors in excluding these younger age groups.
  16. A particular issue in this regard is juvenile crime, which is covered by a different system from crime by adults. Before 1992 juveniles were defined as those aged under 18. After this point, the definition changes to those under 17 years of age. We consequently included a dummy variable for juveniles to control for this change. D&L broke down their area-age-year regressions into violent crime and property crime. This is not possible for England and Wales, as some area-year-age cells have zero entries for violent crime, particularly for juveniles in rural areas.
  17. A complete set of estimated covariate coefficients is available from the authors.

#### REFERENCES

- AKERLOF, G. A., YELLEN, J. L. and KATZ, M. L. (1996). An analysis of out-of-wedlock childbearing in the United States. *Quarterly Journal of Economics*, **111**, 277–317.
- ARELLANO, M. and BOND, S. (1991). Some tests of specification for panel data: Monte Carlo evidence and an application to employment equations. *Review of Economic Studies*, **58**, 277–97.
- BECKER, G. (1968). Crime and punishment: an economics approach. *Journal of Political Economy*, **76**, 175–209.
- BERTRAND, M., DUFLO, E. and MULLAINATHAN, S. (2004). How much should we trust difference-in-difference estimates? *Quarterly Journal of Economics*, **119**, 249–75.
- CAVADINO, P. (1976). Illegal abortions and the Abortion Act 1967. *British Journal of Criminology*, **16**, 63–7.
- DONOHUE, J. J. III and LEVITT, S. D. (2001). The impact of legalized abortion on crime. *Quarterly Journal of Economics*, **116**, 379–420.
- and ——— (2004). Further evidence that legalized abortion lowered crime: a reply to Joyce. *Journal of Human Resources*, **39**, 29–49.
- and ——— (2006). Measurement error, legalized abortion and the decline in crime: a response to Foote and Goetz (2005). National Bureau of Economic Research Working Paper no. 11987.
- EDMARK, K. (2005). Unemployment and crime: is there a connection? *Scandinavian Journal of Economics*, **107**, 353–73.
- EHRlich, I. (1973). Participation in illegal activities: a theoretical and empirical investigation. *Journal of Political Economy*, **81**, 551–65.
- FINER, L. B. and HENSHAW, S. K. (2006). Disparities in rates of unintended pregnancy in the United States, 1994 and 2001. *Perspectives on Sexual and Reproductive Health*, **38**, 90–6.
- FOOTE, C. L. and GOETZ, C. F. (2006). Testing economic hypotheses with state-level data: a comment on Donohue and Levitt (2001). Federal Reserve Bank of Boston Working Paper no. 05–15.
- FREEMAN, R. (1999). The economics of crime. In O. Ashenfelter and D. Card (eds.), *Handbook of Labor Economics*, vol. 3, Amsterdam: North-Holland.
- GOODHART, C. B. (1969). Estimation of illegal abortions. *Journal of Biosocial Science*, **1**, 235–8.
- (1972). On the incidence of illegal abortion. *Population Studies*, **26**, 207–33.
- GREENWOOD, V. and YOUNG, J. (1976). *Abortion in Demand*. London: Pluto Press.
- GRUBER, J., LEVINE, P. and STAIGER, D. (1999). Abortion legalization and child living circumstances: who is the ‘marginal child’? *Quarterly Journal of Economics*, **114**, 263–91.
- JOYCE, T. (2004a). Did legalized abortion lower crime? *Journal of Human Resources*, **39**, 1–38.
- (2004b). Further tests of abortion and crime. National Bureau of Economic Research Working Paper no. 10564.

- (2006). The inconsequential association between legalized abortion and age-specific crime rates: a response to Donohue and Levitt (2001, 2004, 2006). Unpublished manuscript.
- LEIGH, A. and WOLFERS, J. (2000). Abortion and crime. *AQ: Journal of Contemporary Analysis*, **72**, 28–30.
- LEVIN, A., LIN, C.-F. and CHU, C.-S. J. (2002). Unit root tests in panel data: asymptotic and finite sample properties. *Journal of Econometrics*, **108**, 1–24.
- LEVITT, S. D. (1997). Using electoral cycles in police hiring to estimate the effect of police on crime. *American Economic Review*, **87**, 270–90.
- (2004). Understanding why crime fell in the 1990s: four factors that explain the decline and six that do not. *Journal of Economic Perspectives*, **18**, 163–90.
- and DUBNER, S. J. (2005). *Freakonomics: A Rogue Economist Explores the Hidden Side of Everything*. New York: William Morrow.
- LOTT, J. R. Jr and WHITLEY, J. (2007). Abortion and crime: unwanted children and out-of-wedlock births. *Economic Inquiry*, **45**, 304–24.
- MACDONALD, Z. (2002). Official crime statistics: their use and interpretation. *Economic Journal*, **112**, F85–F106.
- POP-ELECHES, C. (2006). The impact of an abortion ban on socio-economic outcomes of children: evidence from Romania. *Journal of Political Economy*, **114**, 744–73.
- ROYAL COLLEGE OF OBSTETRICIANS AND GYNAECOLOGISTS (1966). Legalised Abortion: Report by the Council of the Royal College of Obstetricians and Gynaecologists. *British Medical Journal*, no. 297 (2 April), 850–4.
- SEN, A. (2002). Does increased abortion lead to lower crime? Evaluating the relationship between crime, abortion and fertility. Unpublished manuscript, University of Waterloo Department of Economics.
- WITT, R., CLARKE, A. and FIELDING, N. (1999). Crime and economic activity: a panel data approach. *British Journal of Criminology*, **39**, 391–400.
- WOOLDRIDGE, J. M. (2002). *Econometric Analysis of Cross Section and Panel Data*. Cambridge, Mass: MIT Press.