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Youth Homicide and the Legalization of Abortion

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Abstract

In this paper, we examine the association between the legalization of abortion with the 1973 Roe v. Wade decision and youth homicide in the 1980s and 1990s. An interrupted time series design was used examine the deaths of all U.S. 15-24 year olds that were classified as homicides according to the *International Classification of Diseases* (codes E960-969) from 1970-1998. The legalization of abortion is associated over a decade later with a gradual reduction in the homicides of white and non-white young men. The effect on the homicides of young women is minimal. We conclude that the 1990s decline in the homicide of young men is statistically associated with the legalization of abortion. Findings are not consistent with several alternative explanations, such as changes in drug markets such as in "crack" cocaine. It is almost inconceivable that in the United States of today policies affecting the choice to have children would be justified as a means to control crime. Yet, if the legalization of abortion had this unintended effect, the full range of policy implications needs to be discussed.

Youth Homicide and the Legalization of Abortion

Whatever the moral and political content of Roe v. Wade, there is the important empirical question of the decision's impact on violent crime. In a recent paper by Donohue and Levitt (2001), findings are presented which would seem to support the view that the legalization of abortion is associated with declines in crime nearly two decades later. Two explanations are proposed: the size of the population at risk is reduced; and, "unwanted" children, who would likely receive inferior care, are not born. The former shrinks the number of potential perpetrators and victims overall, while the latter reduces in particular the number of children more likely be associated with in serious criminal conduct (Loeber & Stouthamer-Loeber, 1986; Laub, 1993; Widom & Maxfield, 2001).

The response from the academic community was swift and critical. Joyce (2001), using different statistical models, failed to find any meaningful statistical relationships between the legalization of abortion and subsequent crime. Lott and Whitley (2001), using still other statistical models and somewhat different data, asserted that if anything the relationship is positive--the legalization of abortion is linked to subsequent increases in crime.

As we explain shortly, it is impossible to determine from the three extant studies where the truth of the matter may lie. In this paper, therefore, we step back from the dueling regression models and try first to answer a simpler question: what is the evidence for or against any empirical relationship between the legalization of abortion and subsequent crime? We use data and a research design that we hope avoids many of the complications faced by the three earlier studies. Then we turn to the matter of explanation, and here, too, we use data and a research design that we hope will, in a more accessible manner, clarify the issues.

Previous work

We cannot here review in detail the very extensive and complicated analyses previously conducted. There are a host of arcane details that would obscure more than they would enlighten. Fortunately, it is easy to describe enough about the data and research designs used to suggest that another approach could be instructive.

All three studies noted above lean most heavily on a clever design that capitalizes on some states legalizing abortion before other states. States are the unit of analysis. Roe v. Wade gave women nationwide the right to choose, but some states had legalized abortion a few years before the 1973 decision. Thus, the late legalizers can serve as a comparison group for the early legalizers. If there are abortion effects on subsequent crime, they should be found for the early legalizers and not the late legalizers during a window that depends on the legalization dates. For example, suppose that State A legalized abortion beginning in 1970 and State B legalized abortion in 1973. If there are subsequent reductions in crime as a result of legalized abortion, State A should begin seeing them 3 years earlier (e.g., 1985 compared to 1988), so that during that 3-year window State B is a control observation for State A. The five States that repealed their antiabortion laws before 1973--New York, Washington, Alaska, Hawaii, and California--constitute the experimental group with all other states in the control group.

In each study, a range of crime measures served as the response that was hypothesized to decline in the late 1980's or early 1990's. Data on the suspects in these crimes were used in most of the analyses. It is widely appreciated that law enforcement data can be a problem in studies such as this for two key reasons: first, not all crimes are reported to police and, second, not all crimes are solved. For example, although homicide is the crime for which the best law enforcement data exist, an arrest was made in fewer than one-half of the homicides committed in Los Angeles in 1990-1994, a situation that was not uncommon in large U.S. cities at the time. From 1980 to 1996, clearance rates dropped by 7% nationally (Wellford & Cronin,

The analyses that each article deemed most complete introduced, through regression-based causal modeling, a variety of statistical controls for the many potential confounded effects at the state level. Depending on the authors and the models, these can be almost anything that might affect aggregate crime: a variety of demographic variables, economic conditions, crime control measures, and social services programs, or various proxies thereof. In some cases, the regression models employ a very large number of parameters, representing numerous double and triple interaction effects.

As one would expect, the statistical analyses of such complexity were burdened by all of the usual regression analysis problems: missing data, measurement error, untestable assumptions, overfitting, multicollinearity, and the like. The statistical procedures used are certainly routine enough and the underlying mathematics well understood. The complexity results from the large number of parameters estimated and translating the estimates into meaningful substantive statements. Often the problems are extremely damaging. For example, there are no credible measures of the number of abortions before legalization, and the numbers officially reported afterwards are believed to be rough approximations of the truth (Alan Guttmacher Institute, 1997). Thus, the intervention (i.e., number of abortions) is not accurately measured. Misleading estimates of any associations with the intervention can follow. When, instead, legalization is treated as an intervention step function through a binary variable (i.e., 0 before and 1 after), one necessarily neglects that legalization is not the same as implementation. It could take several years for clinics to gear-up, for example. Again, this can yield misleading estimates of potential intervention effects.

To the authors' credit, many such problems were openly discussed and for some of the difficulties, a serious effort was made to explore their implications. Unfortunately, none of the difficulties addressed could be solved definitively, and none of the manuscripts provided readily accessible results from the range of regression diagnostics one would ideally like to see (Cook, 1998). In our view, however, an even more fundamental concern lies with using states as the unit of analysis. In particular, one has to assume that abortions in a given state affect crime in that same state many years later. One obvious problem is that pregnant women may well travel across state lines to seek an abortion, especially in the early 1970s when abortion was legal in some states but not others. Equally obvious is that during nearly two decades between the time a pregnancy is either terminated by an abortion or produces a live birth and when impacts on crime could materialize, the mother and/or the child could move to another state. (Joyce [2001] attempted to address these issues by using state of birth for homicide victims and state of residence for women who obtained abortions. The latter data are incomplete because, among other reasons, some states [e.g., California] do not require abortion reporting.) States also differ widely in immigration from abroad, which can affect the number of potential crime victims and potential crime perpetrators whatever the abortion practices of the state in question. Destination states such as California, Texas, and New York are good examples of states where international in-migration would substantially increase the number of both potential victims and perpetrators.

In summary, we suspect that the past studies have asked too much of the data and of the causal modeling approach, with the risk that compelling results in any direction are unlikely. This is, more generally, a widely recognized problem. Box (1976: 792) provocatively noted a generation ago that “all models are wrong,” while Mosteller and Tukey (1977: 387) concluded at about the same time that “the whole area of guided regression is fraught with intellectual, statistical, computational, and subject-matter difficulties.” Such concerns have certainly not gone away. Commenting on regression models, Breiman (2001:203) recently observed that “Nowadays, I think most statisticians would agree that this is a suspect way to arrive at conclusions.” Thus, we consider below an alternative strategy with somewhat less ambitious aspirations that does not demand more than the data could possibly deliver.

Methods

Design and Statistical Analysis

We take a quasi-experimental perspective and employ an interrupted time series design (Campbell and Stanley, 1963). The interrupted time series design provides an alternative framework to observational studies analyzed with regression-based causal modeling. The unit of analysis is the country as a whole. Aggregating over states eliminates state boundaries, which surely makes sense for efforts to characterize the impact of the federal decision.

We use yearly data on the number of homicides beginning in 1970 and ending in 1998, allowing for a roughly equal number of years before and after the time when a change in homicide could be expected. The years before the intervention’s possible effect serve as the control group, and the years after serve as the experimental group.

The number of homicides each year is the response variable. We chose homicide because it is the most accurately reported crime and the crime that state penal codes treat as most the serious (i.e., can lead to the longest sentences and/or the death penalty). We used national mortality data from: 1) National Center for Health Statistics, Public use data file documentation: Multiple cause of death for ICD-8 1970-78 data, Public Health Service. Hyattsville, Maryland; and 2) National Center for Health Statistics, Public use data file documentation: Multiple cause of death for ICD-9 1979-98 data, Public Health Service. Hyattsville, Maryland. We included all deaths that were classified as homicides according to the *International Classification of Diseases* (codes E960-969) (ICD, 1969, 1979). As we describe shortly, it is also possible to obtain homicide breakdowns by age, gender, and race/ethnicity that will prove to be important when we elaborate on the basic research design.

Because we are considering the national as a whole, the intervention is defined as the 1973 U.S. Supreme Court decision in Roe v. Wade. Anticipating the use of a transfer function to capture possible associations (Box, Jenkins, and Reinsel, 1994), the intervention is represented by a 1-0 binary variable formulated in three ways: 1) as a spike (an abrupt change in a differenced series), 2) as a step (a change in slope in a differenced series), or 3) as a ramp (a change in slope in an undifferenced series). These variations permit the modeled response to take on a wide variety of shapes. We also explore empirically several lagged effects, beginning in 1973, in an effort to determine when any associations might begin to appear. It is important to stress that we are looking for a discrete change in the response, that is, in homicide trends. Consistent with the interrupted time series design, “pre-existing” changes in levels and trends are removed by differencing, for example, differencing would remove any relatively smooth trends in population sizes. In other words, the original time series is taken to be non-stationary but homogeneous. In practice, the homogeneity assumption does not prove to be especially restrictive, but differencing cannot remove, for example, explosive (i.e., exponential) growth or decay. The goal is not to explicitly represent a large number of potential confounded variables but to remove their collective effects.

But even at its best, the interrupted times series design will only indicate that the response variable is altered in some specified manner at about the same time as the intervention occurs. It cannot eliminate the possibility that the change in the response is linked to one or more other discrete events that occurred at approximately the same time. Moreover, whatever patterns observed in the data are extrapolated beyond the end of the time series at great risk.

To address these concerns, we employ four strategies. First, we undertake separate analyses for male and female victims and for white and non-white victims: white males, non-white males, white females, and non-white females. The goal is to evaluate possible confounders against an explanation resting heavily on abortion legalization. For such analyses to be credible, we assume, consistent with past research, that people tend to kill others like themselves.[1] Two broad expectations follow.

An apparent drop in non-white male homicides associated with earlier abortion legalization might be alternatively explained by a stabilization in the crack cocaine markets in the early 1990s (e.g., Blumstein et al., 2000). However, that explanation does not seem credible for a similar decline in white male homicides because crack was not widely used by whites, and whites generally were not seeking to control crack markets in inner cities. Abortion legalization, in contrast, could well impact both white and non-white males. Thus,

[1] This 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. The ways in which income often produces class and racial homogeneity within neighborhoods is one key instance.

if we find an association between the legalization of abortion and subsequent crime for white and non-white males, an explanation based primarily on patterns of crack use is unsatisfying.

In addition, females are far more likely than males to be killed by people they know and especially by men with whom they have a romantic relationship (Greenfield et al., 1998). Most "domestic" homicides involve female victims beyond their teenage years and still older male perpetrators. One might anticipate no strong associations between the legalization of abortion and subsequent female homicides. For any alternative explanations to be credible, they would have to similarly predict substantial associations for male homicides but not for female homicides.

The second strategy is to conduct separate analyses by the age of the homicide victim as well as by ethnicity and gender. We use 15 year olds as the youngest age group because that is a reasonable initial age in which one might observe involvement in very serious crime. It follows then, that the earliest time at which one could find any subsequent changes in homicides would be 1988 (i.e., 1973 + 15 years). The next year, 1989, one might anticipate as the first in which one might see changes in homicides for 16 year olds. Generalizing from this concept, one can expect such change to occur in later years (i.e., at longer lags) with increases in age. However, age differences of a few years or less do not provide social boundaries as clear as ethnicity or gender. It would not be statistically aberrant, for instance, to find a 16-year old male killed by a 19-year-old male. Therefore, empirical support for this pattern could be difficult to find.

Our third strategy rests on the expected size of any intervention effects for different age cohorts. Recall that legalized abortion could, in principle, affect both the number of potential homicide victims and the number of homicide perpetrators. But for individuals over about 25 years of age, there is not enough time by the end of the homicide time series for legalized abortion to affect them directly. Insufficient time has passed since 1973. Therefore, any reductions in their homicides could only result from a decrease in the number of younger perpetrators; the number of potential victims older than 25 is not being altered. Thus, one might anticipate that any declines in homicide associated with the intervention would be smaller for the cohorts 25 and over compared to cohorts less than 25 years of age. Moreover, any declines should be less substantial with increased age over 25 because of greater distance in age from potential perpetrators.

Our final strategy also rests on the size of any homicide declines for different age groups. One can think of the ages of potential homicide victims as providing a moving window for prospective homicide perpetrators. For example, a 15 year old may be most likely to be killed by an 18 year old, and 18 year old may be most likely to be killed by a 20 year old, and so on. Other things equal, this implies that the largest reduction in homicide victims (associated with the intervention) should be among age cohorts between about 18 and 22 years old. For the youngest ages, only older perpetrators could be affected by the legalization of abortion and for the oldest ages only younger perpetrators could be affected.

Results

We begin with the results for 15-24 year old homicide victims. Table 1 shows several summary statistics for each of the 40 time series, one for each race-gender-age combination. The average number of homicide victims per year is largest for non-white males, followed by white males, followed by both sets of females, which are rather similar. Such large differences between males and females raise the prospect that the underlying causes and dynamics of homicide could be rather dissimilar. Within each race-gender subset, the average number of homicide victims generally increases with age until around 20, at which time the averages tend to level off. Somewhat in contrast, the standard deviations stabilize in the late teens and for nonwhite males actually decline in their 20's.

Insert Table 1 about here

Figures 1a-1d show, for each race-gender subset, the number of homicide victims by year for persons who are 15, 19, and 24 years old. (The other ages are not plotted in order to make the trends more apparent, but the longitudinal story is much the same if they are included.) The series for white and non-white males (e.g., Figures 1a and 1b) all show a transition from a positive to a negative slope in the early to middle 1990s. It is difficult to see a similar pattern for white females (e.g., Figure 1c) although there are hints. For nonwhite females, we see a bit stronger suggestion of the positive to negative transition in the early to middle 1990s (see Figure 1d).

Insert Figures1a-1d about here

The intervention analysis followed conventional procedures. Each of the 40 time series were first analyzed using ARIMA models. Each time series for the males produced white noise after a first difference. No autoregressive or moving average parameters were required. The time series for the females was white noise without any differencing or autoregressive or moving average parameters. In each case, white noise time series were confirmed with conventional tests.

The timing of the intervention was taken to be 1988, 15 years after Roe v. Wade (Lott and Whitley, 2001). For the males, cross-correlations between the intervention and the differenced response failed to reveal any structure when the intervention was defined as a spike or a ramp for lags up to 10 years after 1988. With the intervention defined as a step function, cross-correlations between each of the white noise time series and the intervention showed promising negative correlations at lags of between approximately 2 and 8 years. We also estimated impulse response functions for each time series, which arrived at the same conclusions. Based on the cross-correlations and estimated impulse response function, changes in the slope of the original (undifferenced) time series seemed to be the most the plausible. The rate of increase in the number of homicide victims declined.

For the females, similar analyses were undertaken. Changes in the number of homicide victims associated with abortion legalization were small and could be equally well characterized by a change in level or a change in slope. In other words, the “signal” was too weak to convincingly summarize.

Finally, based on these results, transfer function models were specified that included a constant and a binary variable for the intervention. The particular binary form chosen was designed to capture any changes in slope for both the males and females. We decided to apply a change in slope formulation for the females so that it would be consistent with the model for males. The overall conclusions we discuss later are effectively the same if a change in level approach is used instead.

We also experimented with lagged values of the response and with autoregressive and/or moving average terms for the disturbances. For both males and females, the fit of the models was improved a bit by adding an autoregressive term at lag 1 to the residual structure. While the estimated effect parameters did not change enough to affect any overall conclusions, the estimated standard errors usefully improved.[2]

To shore up the cross-correlation and impulse response function results, we experimented with transfer function lags of from 1 to 7 time periods. Depending on the particular time series, the anticipated negative effects began to appear after a lag of between 1 and 3 years. But for most, the largest negative effects were found at lags of 5 years or longer. In the interest of parsimony, we show below only the earliest negative effects with p-values less than .05 (one-tailed tests). If there is no p-value that small, we show the effect with the smallest p-value. While these decisions are somewhat arbitrary, they also are not very important. For the vast majority of the analyses, all of the effects after a lag of 2 or 3 years were negative.[3]

Sex-ethnicity-age findings

Table 2 shows the model results. For white males, the reductions in slope range from a little more than 14 homicide victims a year to nearly 28 homicide victims a year. P-values vary between .016 and .19, with associated lags of between 3 and 7 years (after 1988). Beginning at age 19, the reductions are all in the middle to high 20's. These figures reflect a change in slope, which necessarily translates into a far larger change in level by the end of the

[2] Each time series is a count for which the Poisson distribution would seem appropriate. However, the mean of each series is sufficiently large so that the normal distribution should be closely approximated. Q-Q plots of the residuals were consistent with this.
[3] Given all of the model building tests and the many time series analyzed, conventional p-values cannot be taken very seriously to begin with.

time series. For example, if the effect estimate is –20 homicide victims, the slope of the number of homicide victims per year is 20 homicide victims smaller each year. Consequently, over a 5-year period, there would be 100 fewer homicide victims. This is a large relative reduction given that the average number of homicide victims per year for white males is in the middle 200s. One can plainly see just such a reduction in Figure 1a after about 1994, especially for the older white males. A drop from, say, 250 to 150 homicides would lead to a rejection the null hypothesis of no effect with a p-value far smaller than .001. In short, the balance of evidence suggests there is a decline in growth of homicide victimization is roughly consistent with abortion legalization.

Insert Table 2 about here

For nonwhite males, the reductions in slope range from about 12 to over 50, with lags from 4 to 7 years. P-values vary from .01 to .08 with all but one less than .05. Just as with the white males, the reductions accumulate so that over a 5-year period, for instance, an overall decline of well over 200 homicide victims can be predicted by the model. This corresponds well with Figure 1b, and such reductions lead to a definitive rejection of the null hypothesis of no change. Clearly, the association with abortion legalization is strong.

For white females, the reductions in level range from a little over 1 homicide a year to a little over 4 homicides a year. P-values range from .03 to .37. While these small reductions accumulate over time, even the accumulated reductions are unimpressive. Indeed, one cannot reject the null hypothesis of no change even for these cumulative reductions. Therefore, while the consistent negative pattern (over time and for different age cohorts) fits with the findings for white males, the associations are at best weak.

For nonwhite females, the reduction in slope ranges from effectively 0 to over 4. Lags vary between 0 and 7 years with p-values between .004 and .47. Once again, even the accumulated reductions are small, and the null hypothesis cannot be rejected. Thus, while most of the estimated effects are once again negative, the associations with abortion legalization are unimpressive.

To briefly summarize, over all of the time series, negative effects predominate that are roughly consistent with the timing of abortion legalization. But the effects are strong only for the males and, coupled the weight of the statistical evidence, heavily against the null hypothesis.

<u>Causation</u>

What can now be said about legalized abortion as a causal explanation? Perhaps the best way to answer that question is to try to rule out alternative explanations. A good starting point is to keep in mind that any alternative explanation for the homicide declines must correspond in time and form to hypothesized impacts of abortion legalization. Thus, changes in police or incarceration practices that might account for the results would have to be implemented abruptly in the late 1980s or early 1990s across the country as a whole (or at least in a number of large states). A similar argument holds for changes in social processes that might explain the homicide reductions.

Consider the explanation based on a waning of the crack epidemic and the violence associated with it. Our findings reflect large homicide reductions for both white and non-white males. Yet, crack cocaine use and distribution was dominated by non-white males and, therefore, cannot easily explain the drop in homicides for white males. Insofar as one concludes that the homicide declines for white and non-white females are real, there may be further evidence against the reduction in crack popularity as an across-the-board explanation. It might still be an important factor in the declines for non-white males.

Recall that we also hypothesized that the associations between the legalization of abortion and homicide reductions would be found at longer lags for older victims. For white and non-white males, this is true. The correlation between the lag at which the effect is first found and the age of the victims is .58 for white males and .57 for non-white males. No such effects are found for females. This implies that at least for males, alternative explanations for the apparent role of abortion legalization would have to account for a delayed impact positively associated with age. This would seem to rule out a wide variety of law enforcement explanations for the drop in male homicides, and yet is consistent with our notions about how legalized abortion might function. The same argument would seem to further undercut the impact of crack cocaine as an across-the-board explanation.

Finally, we hypothesized that if abortion legalization were an important explanation, any declines in homicide associated with abortion legalization would be greater for age cohorts under 25 than age cohorts 25 and older. In addition, more generally, the estimated declines in homicide victims would be greatest in the late teens through the early to middle 20s. To test these notions, we obtained time series data for homicide victims between 26 and 34 years of age, estimated the same kinds of transfer function models used for the younger cohorts, and then, in Figures 2a-2d address these predictions. All are scatter plots with the estimated effect size on the vertical axis and age on the horizontal axis.[4] The overlay is a lowess smooth (Cleveland, 1979).

For white males (Figure 2a), we clearly see that the effects are indeed larger for those under 25 and generally higher in the late teens and early to middle 20s. Much the same story can be found for non-white males (Figure 2b). For white and non-white females (Figures 2c and 2d), neither hypothesis is supported. Thus for males, one would be again be hard pressed to find a law enforcement explanation that would fit the patterns in the data and the crack cocaine explanation would be likewise challenged.

Insert Figures 2a-2d about here

Discussion

For males, there appear to be strong associations between the number of homicides and lagged effects of abortion legalization. For females, the associations are at best small. But is there a causal story in all this? Our statistical model is not causal; unlike previous work, there are no causal mechanisms explicitly built into the data analysis. Potential causal explanations, therefore, must be evaluated through the effect patterns by race, gender and age.

For a causal explanation about homicide trends to be credible, it has to account for the following: 1) a relatively abrupt reduction in slope for male homicides in the early 1990s; 2) at best, weak associations with legalized abortion and homicide for the females; 3) for the

[4] The effects selected from each analysis are, just as before, those with the shortest lag with a p-vale less than .05, or if there were no p-vales smaller than .05, the effect for the lag with the smallest p-value.

males, a strong tendency for longer lagged effects to be found for older age cohorts; and 4) larger effects for males in their late teens and early 20s. The legalization of abortion would seem to fit all four requirements. We can think of no other explanations that do.

Title X is one possibility, but other data do not support it as an alternative explanation. In 1970, Title X of the Public Health Service Act was signed into law, creating for the first time a comprehensive federal program that provides family planning services to low income women. The number of publicly funded Title X clinics grew rapidly and in 1972, Congress amended statute (Title XIX of the Social Security Act) to mandate inclusion of family planning services into all state Medicaid programs to address the disparities of services across states (Gold, 2001). The primary type of service provided by Title X clinics is contraceptive counseling and contraceptive method dissemination. Title X clinics are explicitly forbidden to use program funds for abortion services. Evaluation studies show that the Title X program has substantially reduced the number of unintended pregnancies (Darroch & Samara, 1996), by one estimate, over 20 million unintended pregnancies were averted in the last two decades (Gold, 2001). Title X has been described as the single most successful U.S. family planning (i.e., non-abortion) policy (Meier & McFarlane, 1995). Some research suggests that the prevention of pregnancy and the abortion decision are independent processes (e.g., Friedlander, Kaul, & Stimel, 1984). Moreover, as pointed out in the introduction, Donohue and Levitt (2001), by comparing states that were early- and late-legalizers of abortion, essentially rule out Title X as an explanation for their study findings. Given that we confirm their empirical results, it appears that the observed effect is largely related to Roe v. Wade, not Title X.

We stress, however, that some other explanations may account for part of the findings. Thus, it is still plausible that the decline in crack use can explain at least part of the reduction in homicides for non-white males. It may, indeed, help explain why the reductions for non-white males were so large--the legalization of abortion and the declines in crack both played a role.

We cannot think of explanations for the pattern of findings that would compete with the legalization of abortion. However, that does not mean there are none and we certainly do not definitively demonstrate that legalization caused the reductions in male homicides. There may never be a definitive answer.

Policy implications

Policy has both intended consequences and unintended consequences. The legalization of abortion was anticipated to have several intended consequences, among them to increase women's control over their reproductive choices, decrease births of children who were "unwanted," and eliminate negative health effects of illegal abortions. There have been unintended, or perhaps at least unanticipated, consequences as well. Providers of legal abortion services have been the target of picketing, terrorist threats, and homicide, as have the women who use their services. More recently, researchers have examined the possibility that the legalization of abortion has been responsible for changing the size and nature of an age cohort, which, in turn, reduced violent crime in the U.S.

Legalized abortion is different from many, albeit not all, other policies in that a behavior has been interpreted as an individual right accorded by the U.S. Constitution. Looking at the *effects* of women's reproductive choices shifts the focus from women's reproductive freedom to the effects on a cohort and, by extension, society. It is almost inconceivable that in the

United States of today policies affecting the choice to have children would be justified as a means to control crime. Yet, if the legalization of abortion had this unintended effect, the full range of policy implications needs to be discussed. In particular, there may well be interventions of a less controversial nature that could have some of the same beneficial consequences. If, for instance, an intermediate goal is to reduce the number of children who are born into environments that put them and, ultimately, others at risk, reducing unwanted pregnancy may merit attention as a potential intervention. Policies and reproductive technologies (e.g., long-term hormonal contraception) that will allow women to make decisions regarding their child bearing should increase wantedness of their children. One likely outcome will be parents who raise children when they consider themselves ready and able, which has the greatest potential for positive life outcomes for the next generation.

Conclusions

Determining a causal relationship between legalized abortion and homicide is a difficult task. Many other variables, far too numerous and complex to be sufficiently taken into consideration, influence the occurrence of homicide. Using a straightforward methodological design to look for the potential effect of legalizing abortion in the U.S. upon youth homicide, we find that the data are consistent with such a possibility for young men. The findings do not appear to be consistent with alternative explanations, but we cannot rule out that such alternatives exist. However, if the results are correct, various means of reducing unwanted pregnancy need to be re-examined as potential instruments to reduce crime.

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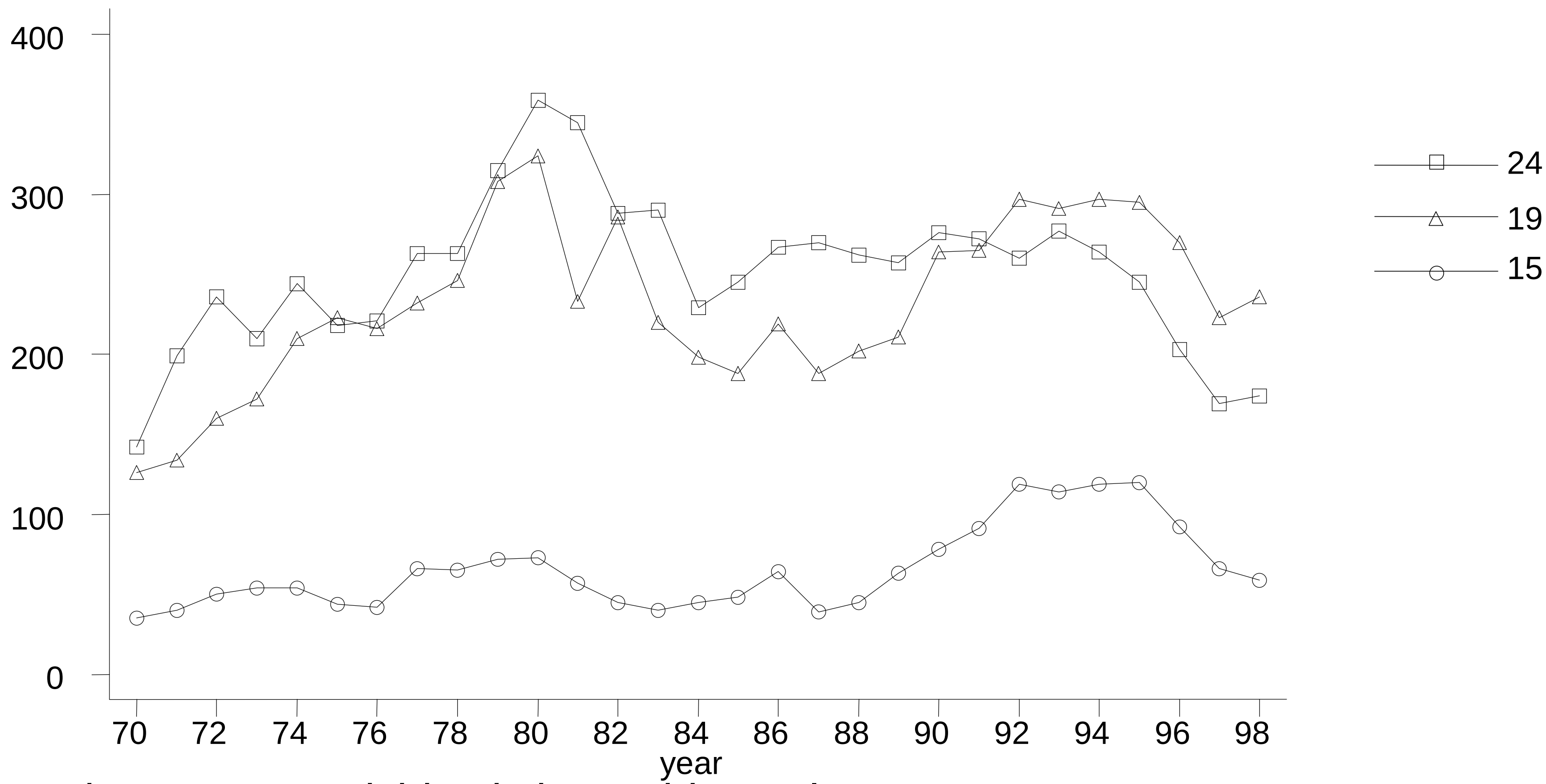

Figure 1a. Homicide victims: white males age 15, 19, 24

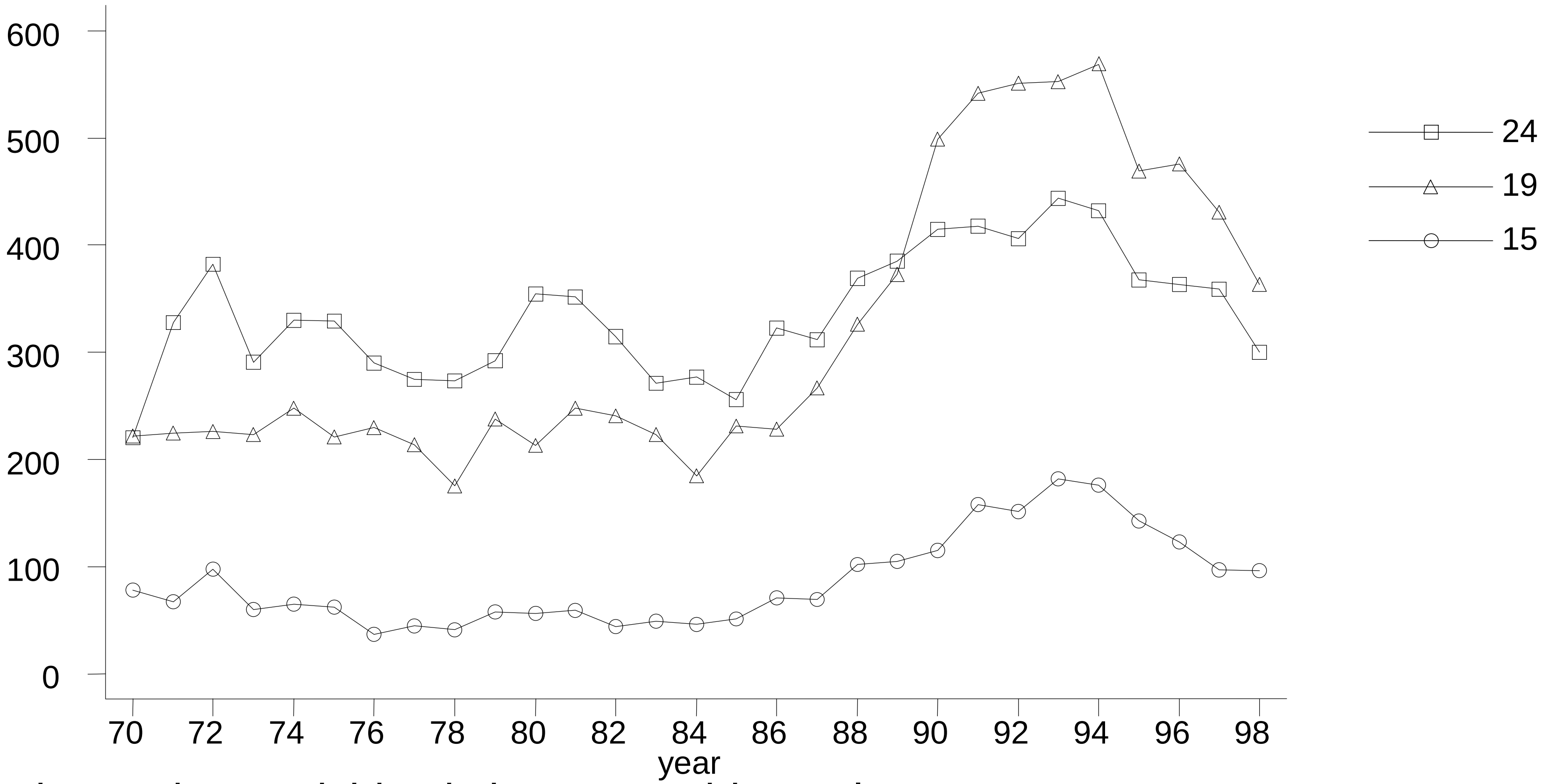

Figure 1b. Homicide victims: non-white males age 15, 19, 24

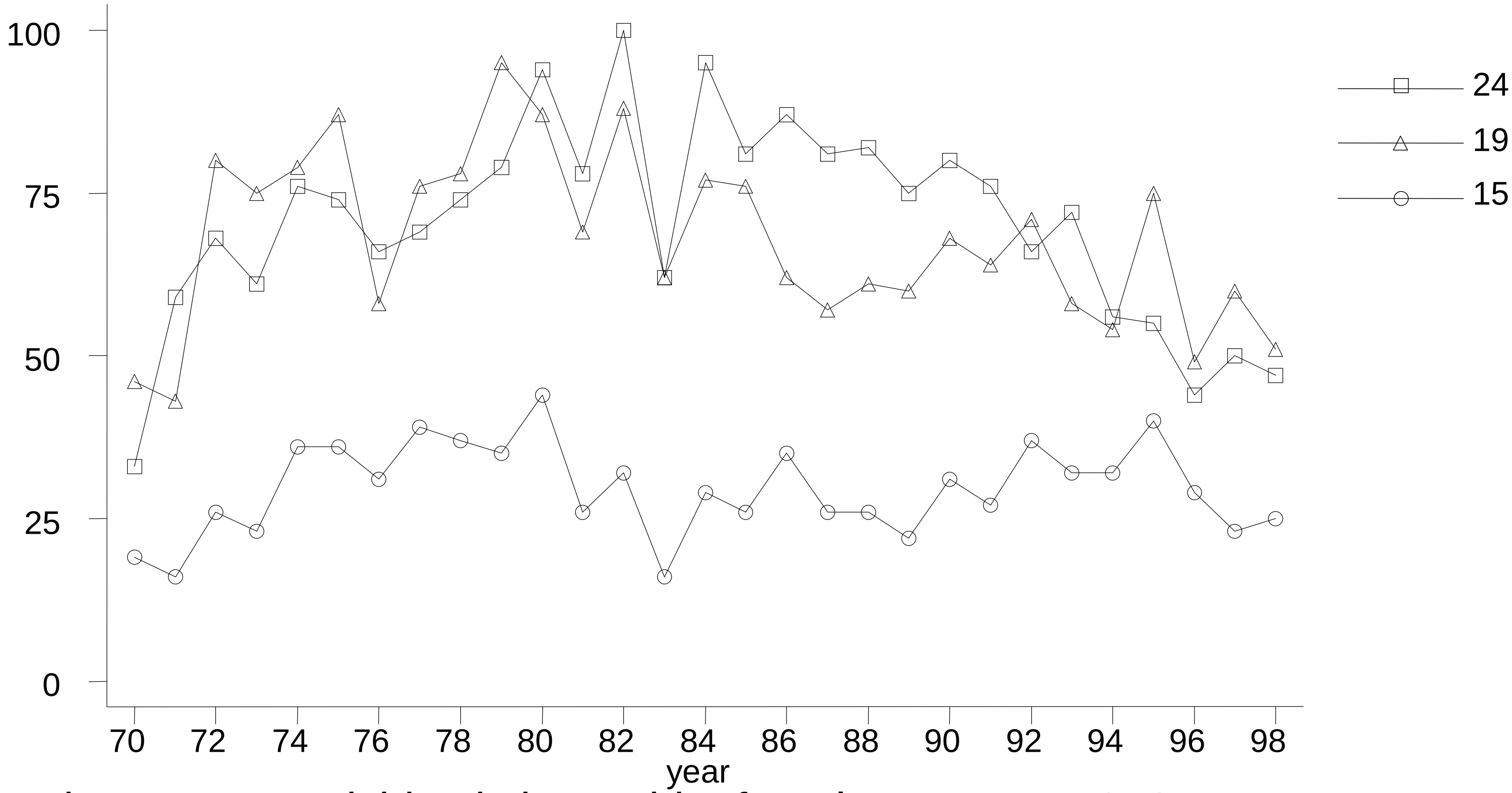

Figure 1c. Homicide victims: white females age 15, 19, 24

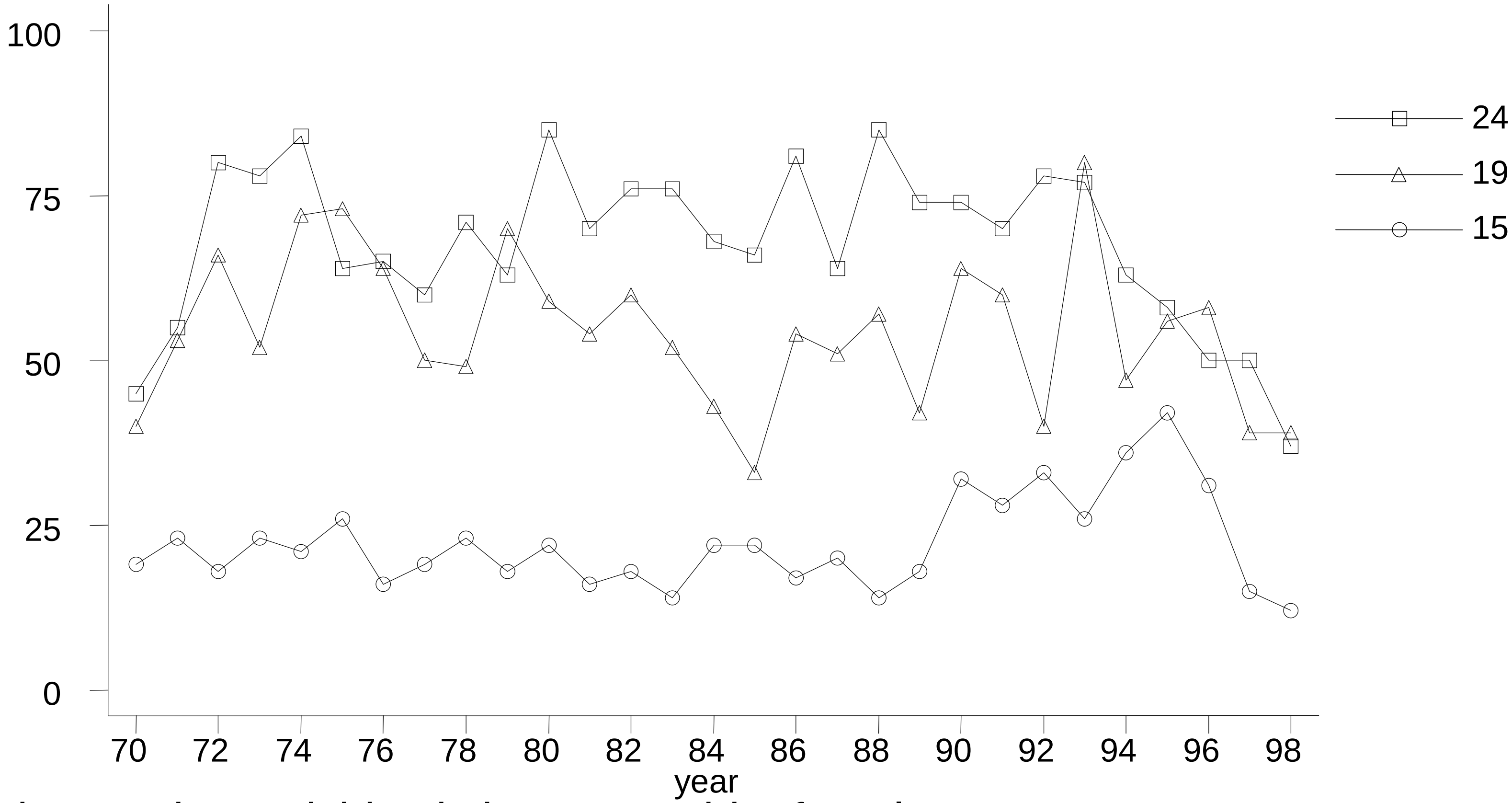

Figure 1d. Homicide victims: non-white females age 15, 19, 24

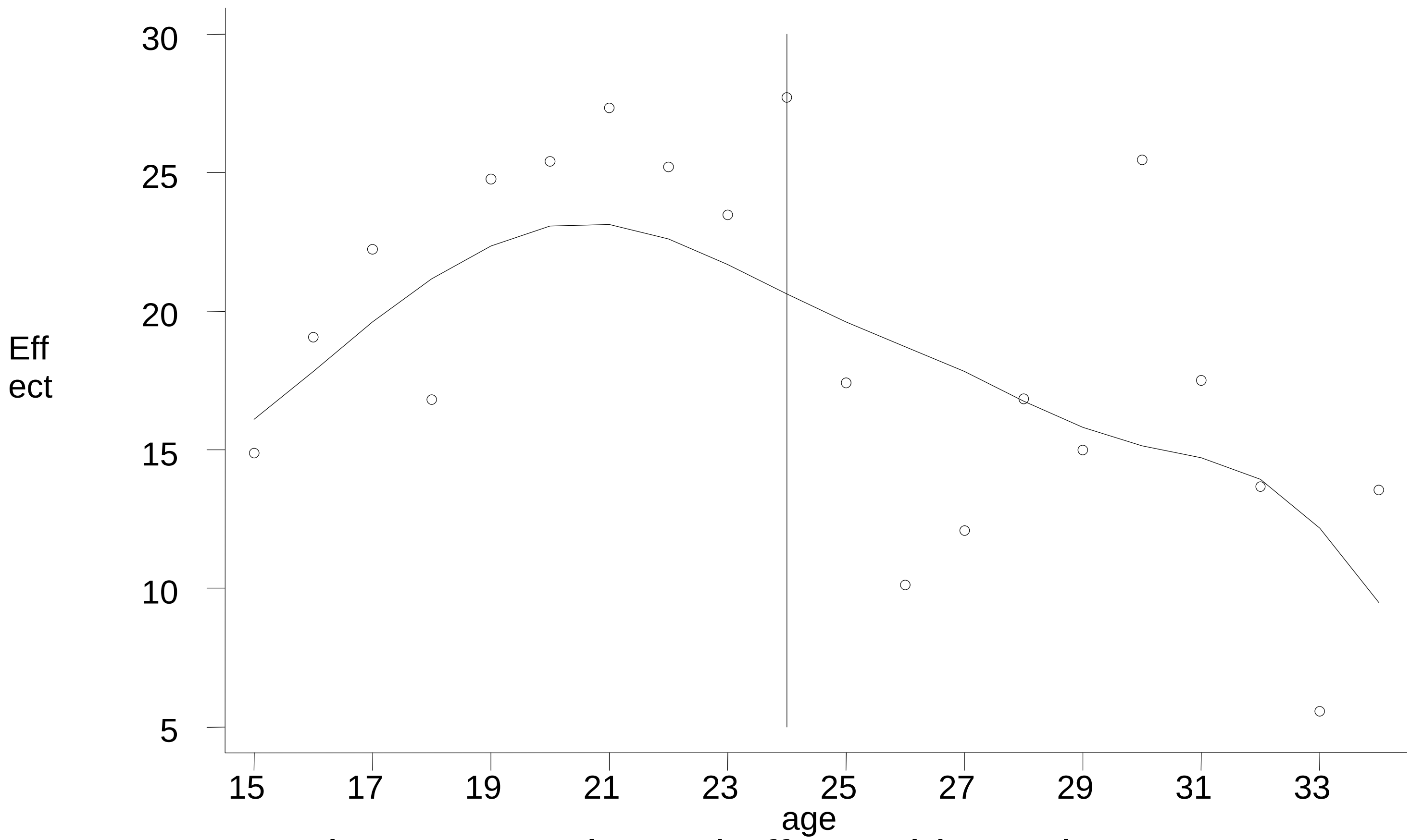

Figure 2a. Estimated effect: white males

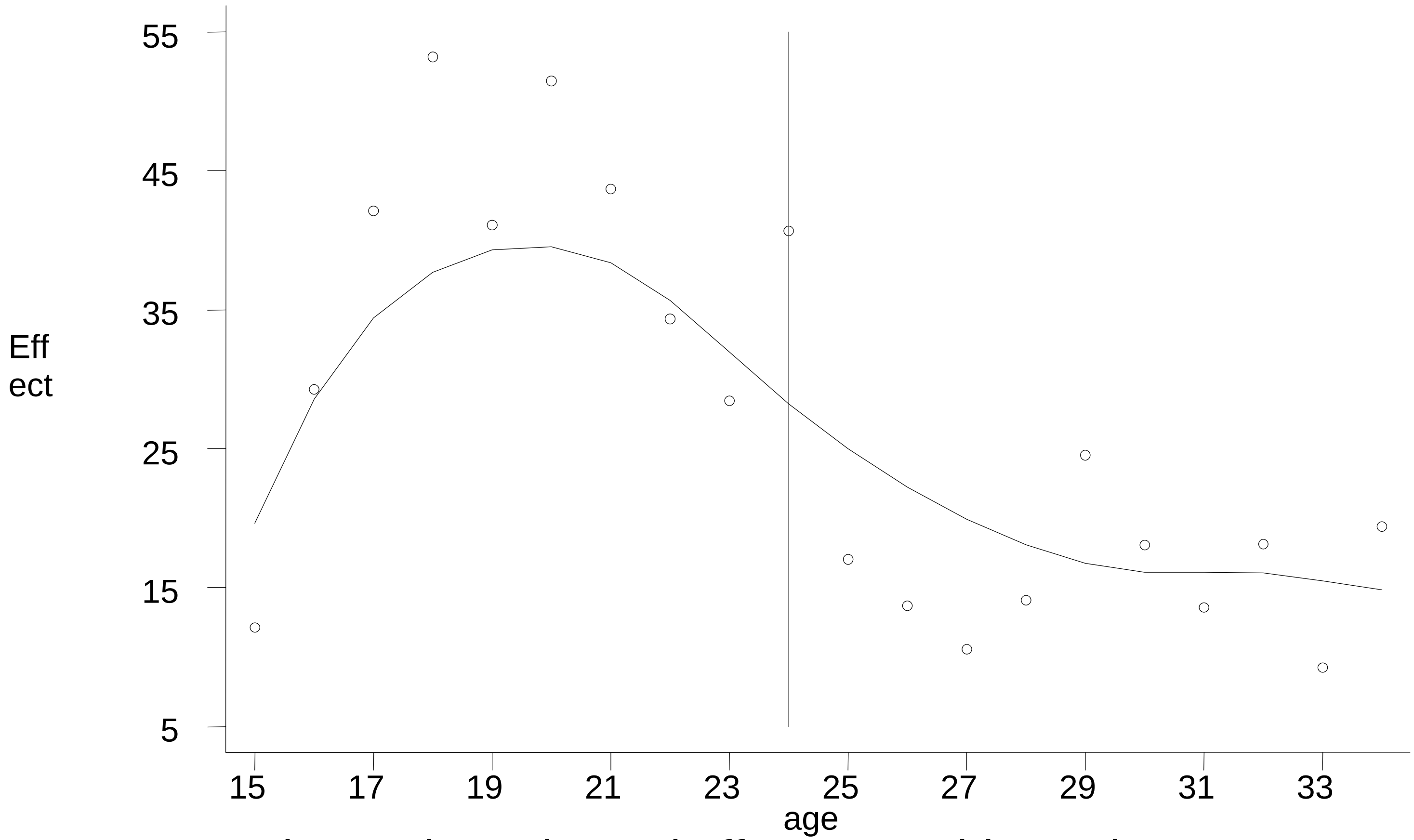

Figure 2b. Estimated effect: non-white males

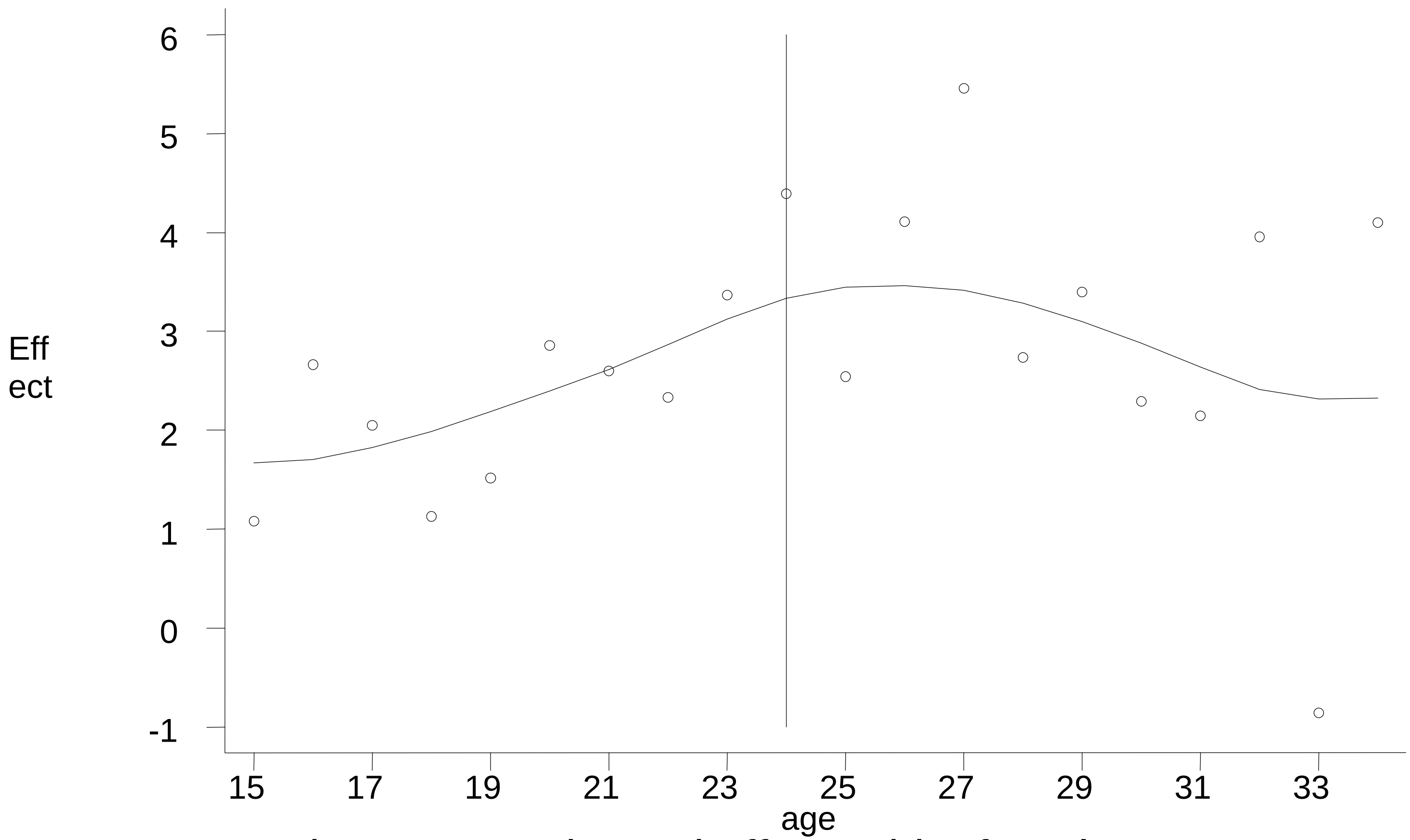

Figure 2c. Estimated effect: white females

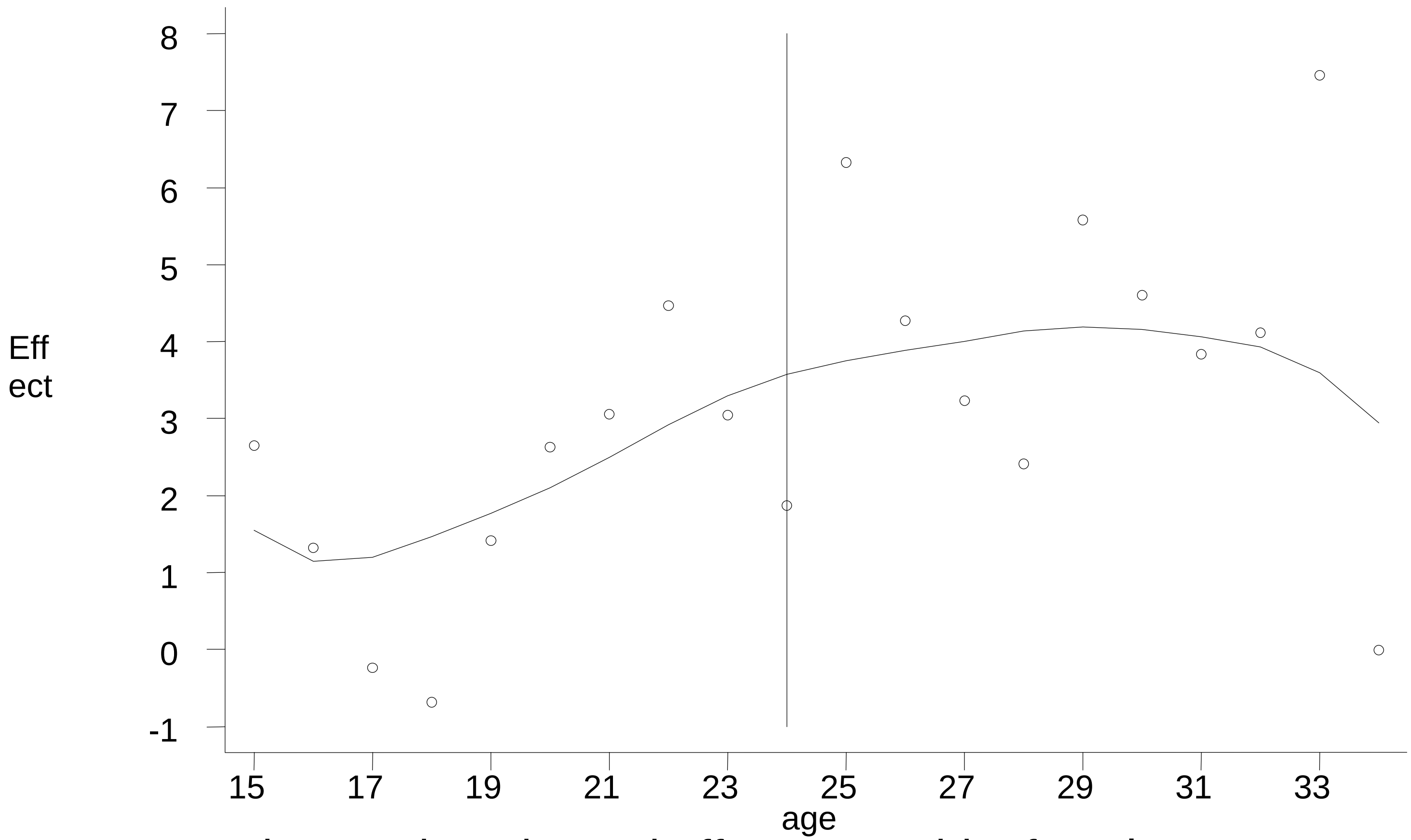

Figure 2d. Estimated effect: non-white females

Table 1. Characteristics of 40 homicide time series: 1970-1998

Sex-race-age group			Obs	Min	Max	Mean	Med	SD
Male								
	White	15	29	35	120	65.5	59	25.9
		16	29	53	177	107.3	99	36.3
		17	29	78	235	152.6	140	44.6
		18	29	112	289	205.1	200	52.9
		19	29	126	324	232.2	223	51.4
		20	29	128	333	243.9	242	50.9
		21	29	148	366	254.4	257	50.8
		22	29	159	373	260.0	262	50.9
		23	29	156	355	261.4	266	54.7
		24	29	142	359	250.4	260	48.0
	Non-white	15	29	37	182	86.3	69	42.4
		16	29	61	298	143.3	119	69.8
		17	29	121	431	219.7	160	100.8
		18	29	151	547	280.8	203	131.9
		19	29	175	569	317.6	241	131.3
		20	29	192	595	328.6	283	119.3
		21	29	214	572	348.4	319	100.1
		22	29	218	515	345.7	329	80.0
		23	29	254	519	339.9	330	71.2
		24	29	220	444	335.5	329	57.1
Female								
	White	15	29	16	44	29.5	29	7.1
		16	29	25	58	40.1	41	7.9
		17	29	21	65	46.1	48	11.2
		18	29	35	99	61.6	60	12.7
		19	29	43	95	67.8	68	13.5
		20	29	43	113	71.2	72	19.4
		21	29	39	102	69.8	74	17.2
		22	29	43	104	74.4	73	15.8
		23	29	33	90	70.1	71	15.7
		24	29	33	100	70.3	74	15.6
	Non-white	15	29	12	42	22.2	21	7.1
		16	29	19	47	28.8	27	7.4
		17	29	21	53	35.9	36	9.1
		18	29	32	63	46.5	46	7.2
		19	29	33	80	54.4	54	11.6
		20	29	46	82	60.0	58	10.2
		21	29	40	88	62.4	62	12.0
		22	29	38	86	65.0	67	13.0
		23	29	30	91	65.8	67	13.3
		24	29	37	85	67.8	70	12.3

Table 2. Effect size for earliest lag that is statistically significant

		Lag for the model	Constant	Effect coef.	P
Male					
White	15	5	4.09	-14.89 *	0.020
	16	4	6.60	-19.06 *	0.016
	17	3	9.43	-22.25	0.065
	18	4	8.17	-16.82	0.188
	19	7	7.23	-24.771	0.111
	20	7	6.64	-25.418	0.064
	21	5	8.34	-27.34	0.074
	22	5	7.05	-25.223	0.075
	23	6	5.86	-23.48	0.111
	24	7	5.09	-27.715	0.126
Nonwhite	15	4	3.70	-12.13 *	0.039
	16	4	6.65	-29.28 *	0.027
	17	4	13.11	-42.15 *	0.021
	18	4	18.09	-53.24 *	0.010
	19	4	15.17	-41.08 *	0.028
	20	4	15.87	-51.49 *	0.020
	21	5	14.11	-43.70 *	0.021
	22	4	10.21	-34.35 *	0.039
	23	4	8.72	-28.47 *	0.023
	24	7	8.46	-40.71	0.076
Female					
White	15	7	29.74	-1.08	0.369
	16	7	40.53	-2.66	0.146
	17	5	47.31	-2.05	0.209
	18	0	63.32	-1.13	0.327
	19	0	71.04	-1.52	0.054
	20	0	76.64	-2.86	0.158
	21	0	75.37	-2.60 *	0.025
	22	0	79.39	-2.33	0.155
	23	0	75.05	-3.37 *	0.028
	24	4	74.03	-4.39	0.091
Non-white	15	7	22.92	-2.65	0.215
	16	6	28.86	-1.33	0.221
	17	7	35.67	0.24	0.471
	18	1	45.16	0.68	0.108
	19	4	55.76	-1.42	0.159
	20	6	61.32	-2.63	0.078
	21	4	65.36	-3.06 *	0.024
	22	6	67.23	-4.47	0.050
	23	2	70.13	-3.05 *	0.004
	24	0	71.35	-1.87 *	0.020

* $p < 0.05$ (one-tailed test)

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