
Avoidance and Misunderstanding: A Rejoinder to McDowall et al.

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Mcdowall, Loftin, and Wiersma (1996) avoid our criticisms of the univariate interrupted time series design (ITSD) by misstating our criticisms, by addressing what they claim we “implied” rather than what we plainly stated, and by ignoring crucial points in our article in this issue (Britt et al. 1996). We reiterate: McDowall et al.’s (1992) conclusions regarding the alleged efficacy of the Washington, D.C., handgun ban (misabeled by McDowall et al. as a licensing law) collapsed once any of three improvements were made to their analysis: (1) use of a more appropriate control series, (2) use of a more appropriate, theoretically based specification of the intervention model, and (3) use of an alternative, extended time series beyond the period studied by McDowall et al. (1992). Nothing McDowall et al. (1996) write has rebutted our assertions.

Use of Control Series

The additional analyses performed by McDowall et al. (1996) serve only to strengthen our conclusions. Their analysis of homicide series for Boston and Memphis (see their Table 1, panel B) reinforces our argument that use of inappropriate control series leads to incorrect conclusions. Aside from population sizes, there is little to recommend either Boston or Memphis as a control area for evaluating the impact of an intervention in Washington, D.C. Unlike Baltimore, neither city is in the same geographic area, and both cities have homicide rates radically different from D.C. For example, for 1979–81, the average homicide rate in D.C. was 31.5, while it was 29.2 in Baltimore, but only 16.8 in Boston and 20.0 in Memphis (U.S. National Center for Health Statistics 1983). It is not clear what is gained by using additional

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inappropriate control series. Interestingly, readers might note that McDowall et al. neither acknowledge nor deny our observation that the use of the D.C. suburbs as a control area was unsuitable. Presumably their silence means agreement.

McDowall et al. seem to believe that implied in our comments is that “only a *single* comparison series is worth analyzing.” The reader will search in vain for any passage in our article that states or implies such a conclusion. Indeed, this flies in the face of our demonstration that McDowall et al.’s use of a single control area (the D.C. suburbs) produced results that collapsed as soon as a more appropriate control area (Baltimore) was used. Further, in an earlier version of our article, we explicitly noted the danger of relying on any one control area (Kleck, Britt, & Bordua 1993).

The reader will also search in vain for any passages where we insist “on one perfect control” area, which McDowall et al. claim was our “counsel of despair.” We did not assert that there was one perfect control area that Loftin et al. (1991) failed to identify. Rather, we illustrated a far more simple and straightforward claim: the control area used (the D.C. suburbs) by Loftin et al. (1991) was grossly inappropriate, regardless of the criteria one might use to judge the adequacy of a control area, and they had ignored a far more appropriate (*not* perfect) control area (Baltimore). Our recommendation is to use control areas *as similar as possible* to the intervention area, since a perfect control area could never be found.

McDowall et al. then go on to suggest that they have minimized the problems associated with studying a single area by using additional “internal control series,” such as nongun homicides.¹ They seem to have ignored our criticisms of the use of such series. Since the upward and downward shifts in gun homicide are routinely more pronounced than shifts in nongun homicide, regardless of whether the shifts could be due to changes in gun law strictness, it is not at all clear what is gained by the use of such “control series.”

Finally, in regard to the use of control series, McDowall et al. are mistaken when they claim, in note 3, that Kleck (1991:254, 386–87) and Britt et al. (1996) made incompatible recommendations about the selection of control areas. There are no inconsistencies. Britt et al. expressed a preference for control areas that are similar to the intervention area *both* cross-sectionally and cross-temporally, while Kleck (1991:387) criticized studies using

¹ Although McDowall et al. (1996) assert in note 2 that we are mistaken about their nongun homicide series, we persist in our claim that Loftin et al. (1991) included legal intervention homicides in their nongun series for D.C. We were unable to replicate their results for nongun homicides until legal intervention homicides were added to the series. We were then able to replicate their results exactly.

control areas that showed *only* cross-sectional similarity (and only with respect to region and/population size, at that).

Specification of the Intervention Model

McDowall et al. note that conducting multiple tests of the impact of an intervention by using multiple time points as intervention points can easily lead to at least one “significant” finding by chance. We agree. However, we must also point out that this is yet another reason why interrupted time series designs are problematic. Since the researcher cannot realistically identify one specific time point when the intervention’s impact (if any) will start to become evident, the researcher is forced into one of two bad choices: (1) an arbitrary, possibly unrealistic selection of a single intervention point, such as the law’s effective date, potentially leading to the confusion of the impact of one intervention with that of another intervention, or (2) tests of multiple intervention points and their attendant significance testing problems.

McDowall et al. address our discussion of the possibility of different intervention points, by providing an example time series and noting that misplacing the “intervention point” would lead to an underestimation of the “intervention effect.” This is obviously correct for the example they present. What McDowall et al. fail to consider, however, is the possibility that they have selected the wrong intervention point in their analysis of the D.C. handgun ban. For example, Figure 1 presents the estimated value for the “intervention effect” for the 36 months (at 6-month intervals) before and after the law’s effective date in October 1976. What we see are even larger “intervention effects” than McDowall et al. found in their analysis in the 18 months before the law became effective. Clearly, monthly homicides dropped in D.C. in the mid-1970s, but these results suggest to us that something other than the gun law was responsible for the observed decline.

McDowall et al. also appear to have missed our point about specifying the intervention model as abrupt or gradual based on substantive or theoretical grounds. The D.C. gun homicide series showed evidence of an abrupt drop. McDowall et al. assert that it was our “advice” that this finding be ignored. To the contrary, we stressed that the finding should be taken seriously for what it indicates about the likely source of the decline in gun homicides. The D.C. handgun law was a freeze or ban on handguns that allowed existing owners of legally registered handguns to continue owning them as long as they lived. Thus, it was designed to have only a gradual effect on the supply of legal handguns, producing a decline only as legal owners died or moved out of the District. It was precisely because the trend in gun homicides showed an abrupt drop that McDowall et al. should have been

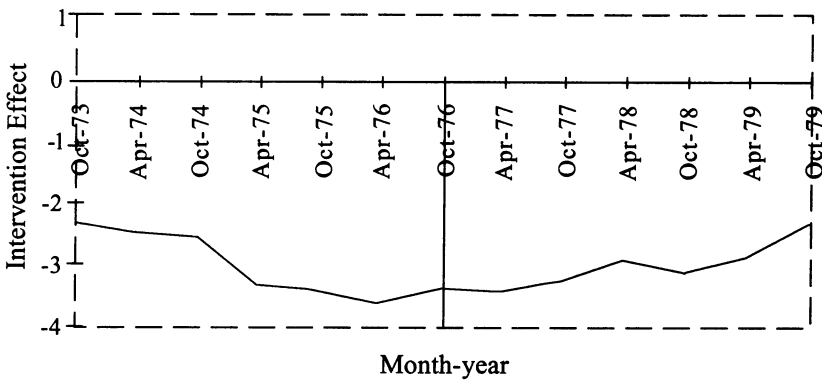


Figure 1. Estimated Intervention Effects in Washington, D.C.: October 1973 to October 1979.

leery about concluding the gun law was responsible, and should have given more serious attention to the possibility that some other factor was responsible for the change. Yet, despite the evidence that neither the abruptness nor the timing of the drop was consistent with the hypothesis that the gun law was responsible, McDowall et al. accepted the hypothesis anyway.

Specification of the Time Series

We agree with McDowall et al. that the crack epidemic contributed to the rise in gun homicides beginning in the 1980s. However, this is precisely our point on how historical effects can undermine the internal validity of a univariate time series analysis. If the crack epidemic could, in the 1980s, wipe out all evidence of what McDowall et al. insist was a substantial law-induced decline in gun homicides, then it is clearly impossible for them to rule out the possibility that any of hundreds of other factors could have been responsible for the mid-1970s decline in gun homicides in D.C. that McDowall attributed to the D.C. handgun ban.

Summary

If one insists on using interrupted time series designs to study policy impact, then a better way to do it would be to include appropriate control areas that are similar, both cross-sectionally and temporally, to the intervention area, to study multiple time periods, to fit the intervention model more intelligently based on substantive and theoretical reasons, and to test for multiple intervention points (statistical hypothesis tests notwithstanding). It strikes us as absurd to conclude, as McDowall et al. have, that it was impossible to meet the standards we suggested, since we obvi-

ously followed them in our own analysis of the impact of the D.C. handgun ban.

In the long term, we see well-designed pooled cross-sectional studies as a more useful approach to testing for aggregate legal effects. Pooled cross-sectional studies, which make use of variation in the dependent variables across both space and time, can be applied to a large number of jurisdictions, even all states, and thereby reduce the problems faced by case studies of arbitrarily selected intervention sites. Recently, Marvell and Moody (1995) have used these methods to assess the impact of laws providing more severe punishment for felonies committed with guns. Interestingly, their results, which apply to all 49 states with such laws, contradict those obtained by McDowall et al. (1992), who used interrupted time series methods and focused on just six cities in three nonrandomly selected states.

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