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ARTICLE: Falling Clearance Rates After Miranda: Coincidence or Consequence?

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SUMMARY:
 ... Donohue also explains, as we did initially, that in many cases, Miranda would have no impact on clearance rates because police can frequently make an arrest and "clear" a crime even where Miranda blocks a confession and later effective prosecution. ... He concludes that the best explanatory equation indicates that clearance rates fell 11.7% after Miranda, a result that is statistically significant at a very high confidence level. ... The effects are statistically significant at the conventional level 95% confidence level for violent crimes and larceny and at the 90% confidence level for property crimes and vehicle theft. Donohue concludes from his equations that "nonetheless, the consistency of the size and signs of the [Miranda] effects, particularly for the violent crime variable, does provide some evidence in support of an unexplained post-Miranda downward deviation from trend." ... Donohue's equations suggest that Miranda reduced the clearance rate for violent crimes by 11%, larceny 13%, property crimes 9%, and vehicle theft by 15% - figures on police effectiveness (it is worth emphasizing again) that do not capture Miranda's presumably greater effects on subsequent prosecutions. ... But for bad data to explain the findings, one would need some theory of data distortions that places murder, rape, and assault in the "no effect" category and armed robbery, burglary, larceny, and vehicle theft in the "effect" category. ... Also, the homicide data is an imperfect indicator of Miranda's impact for several reasons. ...

TEXT:
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     In our initial article, we suggest that the fall in crime clearance rates in the years immediately following Miranda was at least in part attributable to the new restrictions the decision placed on police interrogation.   n1 In reply, Professor John Donohue thoughtfully reviews and confirms many of our findings, while raising some cautionary notes about our conclusions.   n2 We appreciate not only his interest in our work but also his willingness to avoid the ideological rigidity that too often plagues the empirical debate about Miranda's effects. In this short rejoinder, we highlight our many of points of agreement and respond briefly to his questions.

Professor Donohue begins by emphasizing a point raised only briefly in our article. Ordinarily, he explains, detecting the effects of even the most significant judicial opinions is quite difficult. n3 Only substantial and severe impacts are captured in our review of aggregate national crime statistics, suggesting that our methodology is biased heavily against finding Miranda had any influence. Donohue also explains, as we did initially, that in many cases, Miranda would have no impact on clearance rates because police can frequently make an arrest and "clear" a crime even where Miranda blocks a confession and later effective prosecution. n4 This, too, confirms that our approach tilts significantly against finding a "Miranda effect" on police investigations and fails to capture Miranda's presumably more substantial effects on criminal prosecutions. It also indicates that our analysis is most likely to detect Miranda's impact on the investigation of crimes for which interrogation of one suspect can clear multiple crimes. Donohue helpfully styles this as an "other-crimes" effect and recognizes that the crimes where this most [*1182] frequently occurs (e.g., robbery, burglary, and vehicle theft) are the same crimes for which we found a Miranda effect. n5

Donohue also properly cautions that alternate causalities can confound interrupted time series analysis. This is a critical point, for its raises the issue of whether the drop in clearance rates after Miranda was simply coincident with - or a consequence of - the decision. As an illustration of the difficulties in determining causality, Donohue quotes at length from a critique of Sam Peltzman's finding that auto safety legislation in the 1960s caused an increase in pedestrian deaths and suggests that similar problems may be lurking in drawing causal conclusions here. n6 Donohue is careful only to flag the issue, not to argue that our suggestion of Miranda's adverse impact is, in fact, disproved by any such alternate causalities. We appreciate the cautions and discussed at length these and others in our initial paper. n7 The point is sufficiently important one, though, that it is useful to distinguish Peltzman's debatable approach from what is, we think, the much more straightforward technique we applied.

The first distinction is one of the a priori plausibility of the model under consideration. Peltzman posited an elaborate theory of "increased risk taking" that would, in turn, be indirectly revealed by such things as a greater propensity of young people to drive or of drivers to drink, an argument that was styled as "peculiar" by those of a different view. n8 In contrast, we simply investigated the obvious question of whether crime clearance rates fell after Miranda, an approach first suggested by Miranda's defenders. n9 When we began our research, there was widespread agreement in the literature that this was an appropriate model of some of the effects of the decision - perhaps because the prevailing (albeit incorrect) view was that clearance rates remained essentially stable after the decision. Our Miranda effect is also distinguishable from Peltzman's rather complicated "risk-taking" effect, which is only "observable" through reading of complex equations; our effect appears immediately on a simple graph of clearance rates, which reveals an obvious drop in clearance rates for several crimes immediately following Miranda. n10 Our finding also stands up to a wide array of specifications of [*1183] the model, as both Donohue's results and our extreme bound analysis ("EBA") confirm. n11

In possible contrast to Peltzman, we also spend considerable time on and devote effort to identifying conceivable alternative causes and considering whether they affected our conclusions. A full section of our paper is devoted to the conclusion that Miranda is the strongest candidate to explain the sharp change in crime clearance rates over the period 1966-1968. n12 In raising his cautions, Donohue alludes to the general possibility of social changes during the 1960s. Donohue, however, investigates possible starting dates for the Miranda effect from 1962 to 1969. He concludes that the best model is one in which the effect starts in mid-1966 and develops its impact over the following eighteen months. n13 The timing is striking, because this is exactly when police altered their questioning procedures in response to the decision. n14 Moreover, we investigate longer lag structures in our initial paper. We find that the best fit for the Miranda effect is 1966-1968, not longer periods such as 1964-1968 or 1964-1969. n15 This suggests that we are detecting a phenomenon that struck relatively quickly over one or a couple of years, rather than longer term social changes such as youth rebellion or gradual improvements in police recordkeeping procedures. n16

A final and important distinction from Peltzman's analysis is that our findings on clearance rates fit within a larger pattern of evidence suggesting that Miranda harmed law enforcement. We have not concluded that Miranda "handcuffed" the cops based simply on reading regression coefficients in our clearance rate equations. Instead, that data fits coherently within a broader picture, including:

. contemporaneous reports, from both the FBI and police, of adverse effects on clearance rates from court decisions;

 [*1184] . declining confession rates measured in the "before-and-after" Miranda studies;

. lower confession rates reported in this country after Miranda;

. higher confession rates reported in other countries that do not follow the Miranda rules; and

. the common-sense observation that unprecedented restrictions on law enforced caused some adverse effect on the police. n17

Taken together, this consistent body of evidence forms the basis for our conclusion that Miranda has adversely affected police effectiveness.

Perhaps the most exciting part of Donohue's paper is his own regressions, which replicate many of our most significant findings. Most of his equations are directed to analyzing violent crime clearance crimes. He concludes that the best explanatory equation indicates that clearance rates fell 11.7% after Miranda, a result that is statistically significant at a very high confidence level. n18 Donohue finds an even higher post-Miranda drop (15.3%) using the murder rates as a measure of actual burdens on police. n19

Analyzing the data in individual crime categories, Donohue also finds negative "Miranda" effects for all the collective categories of violent and property crimes and all individual crime categories. The effects are statistically significant at the conventional level 95% confidence level for violent crimes and larceny and at the 90% confidence level for property crimes and vehicle theft. Donohue concludes from his equations that "nonetheless, the consistency of the size and signs of the [Miranda] effects, particularly for the violent crime variable, does provide some evidence in support of an unexplained post-Miranda downward deviation from trend." n20 Donohue's findings also undercut the suggestion of Miranda's most ardent defenders that increases in crime rates combined with stagnant criminal justice resources entirely account for the falling clearance rates. n21 The effects that are under [*1185] discussion are, in our view, quite large. Donohue's equations suggest that Miranda reduced the clearance rate for violent crimes by 11%, larceny 13%, property crimes 9%, and vehicle theft by 15% n22 - figures on police effectiveness (it is worth emphasizing again) that do not capture Miranda's presumably greater effects on subsequent prosecutions. n23

We were tempted to simply leave matters there, because Donohue's own findings were generally corroborative of our results. However, we were interested that Donohue's individual crime equations produced fewer statistically significant Miranda effects than our own. It appears that three differences between our specifications and Donohue's account for the variance in results. We believe our specifications are preferable on all three points.

First, while we include, with some trepidation, n24 a linear time variable in our equations, Donohue adds in addition a time-squared variable. He leaves unexplained specifically what evidence of a need for correction he observed in his equations. n25 Adding such an unconventional variable without good justification is problematic, because a time-squared variable might appropriate for itself some of the variance in clearance rates that are actually attributable to the Miranda variable. In any event, there is a presumption in favor of parsimony, which Donohue follows aggressively in jettisoning all of our socioeconomic and demographic variables. That presumption counsels against an unjustified inclusion of time squared.

Second, Donohue multiplied all of his police and resource measures by the ratio of police officers to total employees. Because the proportion of police employees who were "officers" rather than civilian employees fell from 94% in 1950 to 78% in 1995, n26 this adjustment imparts a long-term downward "tilt" to all police resource variables and could give them increased "explanatory" power for the clearance rate series that trend downward over time. The rationale for this adjustment is Donohue's "assumption ... that the number of officers is a better measure of the crime-fighting resources of the police than the number of total police employees." n27 We believe this assumption is open to question. A more plausible view is that increasing use of civilians by police agencies over time simply transfers

functions previously performed by the uniformed officers to presumptively less expensive civilians. The total "crime-fighting" power available to communities has not de- [*1186] clined as a result of this bureaucratic shuffle. n28 Some "civilian" employees can even perform police law enforcement functions directly, n29 so their civilian status may be of little real world consequence. Therefore, the presumption ought to be against making an "officer" adjustment, unless the equations revealed some good empirical justification for it.

Third, while we use index crime as the measure of police workload, Donohue explored narrower measures. He begins by hypothesizing that murder rates might be the best measure of workload, ultimately rejecting them in favor of the broader measure of violent crimes. n30 As far as we can tell, however, Donohue does not consider whether the still broader measure of index crimes rates, which we used, is superior to the violent crime rate that he uses. As a matter of first impression, index crimes would appear to form the preferred model. Police are kept busy by their total workload, not a fraction thereof. n31

It is possible to analyze empirically these three issues by comparing Donohue's results with the results produced by changing each of these three assumptions in what we believe is the theoretically preferred direction. Table I depicts the results. As can be seen, changing any of Donohue's three assumptions produces stronger Miranda results. n32 Changing all three of the assumptions produces results virtually identical to ours: a statistically significant Miranda effect for violent crimes, robbery, property crimes, burglary, vehicle theft, and (at the .90 level) larceny. Most of these alternative specifications have a better "fit" than Donohue's specifications, as measured by his selected measure of fit, the adjusted R-squared. n33 The consistent results from these modified equations, it should be recalled, come from adopt- [*1187] ing all of Donohue's assumptions and simplifications, such as jettisoning all socioeconomic and demographic variables. That Donohue's equations can be made to conform precisely to ours with such little "tweaking" is, in our view, strong confirmation of our conclusions.

One statistical cautionary note that Donohue raises is the possibility of multicollinearity in our data, a point he elaborates with a table showing high correlations between our various variables. n34 Because this concern always looms large in time series analysis, we applied extreme bounds analysis as a diagnostic. n35 Econometricians use extreme bounds analysis less frequently than other corrective techniques, perhaps because often it unforgivingly suggests multicollinearity by revealing that the inclusion or exclusion of particular variables is necessary to generate the desired results. Extreme bounds analysis confirms that the results in our original paper are independent of model specification, casting strong doubt on multicollinearity as an explanation for our results. n36 Interestingly, extreme bounds analysis of Donohue's equations is identical to the extreme bounds analysis of our own. As shown in Table II, "tight bounds" exist for violent crimes, robbery, property crimes, larceny, and vehicle theft - which means that regardless of the combinations of the various explanatory variables, MIRANDA always produces a negative effect on clearance rates for these crimes in Donohue's equations. n37

Donohue also notes that the data from the FBI's Uniform Crime Reports is imperfect because it is potentially subject to both conscious and unconscious manipulations by particular police departments. n38 While we raise similar caveats in our initial article, we also collect qualitative and quantitative evidence that such fluctuations were minimal in aggregate national statistics n39 and that the clearance rates declines in 1966 and 1967 were "universally reported by all population groups and all geographic divisions." n40 Donohue extends the argument by observing we found no Miranda effect for homicide, which is the best reported of the six index crimes. Our failure to find a Miranda effect for the index crime with the best data would be par- [*1188] ticularly troubling if all of the other six manifested the effect. If so, this might suggest the Miranda effect was really a "bad data" effect. But for bad data to explain the findings, one would need some theory of data distortions that places murder, rape, and assault in the "no effect" category and armed robbery, burglary, larceny, n41 and vehicle theft in the "effect" category. A more likely explanation for these groupings is the one that Donohue himself recognizes: that Miranda's "other-crimes" effect is particularly strong for robbery, burglary, larceny, and vehicle theft. n42 Also, the homicide data is an imperfect indicator of Miranda's impact for several reasons. First, while homicide may be the best reported of the individual crime index statistics, it does not always track developments in the other crime categories. n43 Moreover, while reporting of homicide offenses may have remained consistently very high over time, homicide clearances may be subject to fluctuations for reasons having

little to do with the Miranda issue.   n44 Finally, we agree with Donohue's caveat that the failure to find a Miranda result for homicide clearances could well be due to police efforts after the decision to maintain high homicide clearance rates by shifting resources toward these investigations.   n45 All these points indicate that other crime categories apart from homicide should also be investigated. A strong argument can be made that motor vehicle theft is the second-best reported crime. Insurance requirements frequently compel victims to bring such crimes to police attention and [*1189] police to record accurately these reports.   n46 For this second-best reported crime, we found a strong Miranda effect and Donohue's equations, if reformulated along the lines we suggest, indicate the same thing.   n47

Professor Donohue concludes his valuable paper with a reference to the adage nullius in verba - trust not in words. We concur fully with his suggestion that debate about Miranda's impact is an empirical one, to be resolved not with discourse but data. Miranda's defenders justify the decision's balancing of competing interests by claiming it had no adverse effect on law enforcement. This theory conflicts not only with common sense but, more importantly, with the mounting statistical evidence. Donohue's careful replication of the most important features of our study should, we think, be included among that literature. Although social science always involves some measure of ambiguity, the generally consistent thrust of the available research underscores the pressing need to begin exploring reasonable alternatives to the decision. The Miranda rules are not the only way to regulate police interrogation. The accumulating evidence that it "handcuffed the cops" suggests it is not the best way, either.

[SEE TABLES IN ORIGINAL]

Legal Topics:

For related research and practice materials, see the following legal topics:
Criminal Law & ProcedureCriminal OffensesVehicular CrimesGeneral OverviewCriminal Law & ProcedureInterrogationMiranda RightsGeneral OverviewCriminal Law & ProcedureSentencingGuidelinesAdjustments & EnhancementsGeneral Overview

FOOTNOTES:

n1. See Paul G. Cassell & Richard Fowles, Handcuffing the Cops?: A Thirty-Year Perspective on Miranda's Harmful Effects on Law Enforcement, 50 Stan. L. Rev. 1055 (1998).

n2. See John J. Donohue III, Did Miranda Diminish Police Effectiveness?, 50 Stan. L. Rev. 1147 (1998).

n3. See id. at 1149-51.

n4. See id. at 1156; Cassell & Fowles, supra note 1, at 1065.

n5. See Donohue, supra note 2, at 1156 n.52.

n6. See id. at 1158-59.

n7. See Cassell & Fowles, supra note 1, at 1107-08 (noting that causal conclusion can only come from combining regression analysis with other information).

n8. Compare Sam Peltzman, The Regulation of Automobile Safety, in Auto Safety Regulation: The Cure or the Problem? 1 (Henry Manne & Roger LeRoy Miller 1976) [hereinafter Auto Safety], with Richard R. Nelson, Comments on Peltzman's Paper on Automobile Safety Regulation, in Auto Safety, supra, at 63, 65.

n9. See Cassell & Fowles, supra note 1, at 1064 (collecting citations to this effect).

n10. See, e.g., id. at 1069 fig.1 (Violent Crime Clearance Rates); id. at 1085 fig.4 (Robbery Clearance Rates). See generally Edward R. Tufte, Visual Explanations: Images and Quantities, Evidence and Narrative (1997) (noting the importance of visual observations).

n11. See Donohue, supra note 2, at 1164 (noting that Miranda results are "robust" to certain specification changes); Cassell & Fowles, supra note 1, at 1103-06 (noting that EBA analysis confirms findings do not depend on inclusion of particular variables).

n12. See Cassell & Fowles, supra note 1, at 1107-19.

n13. See Donohue, supra note 2, at 1166. Donohue similarly finds that the Miranda effect is best modeled by assuming a permanent reduction in clearance rates, not, as has been suggested by some of Miranda's defenders, by a short-lived, merely temporary reduction in the rates. See id. at 1166-67.

n14. See Cassell & Fowles, supra note 1, at 1092-94 (collecting available evidence on implementation of Miranda).

n15. Using the same criteria as Donohue (t statistic and adjusted r-squared), the best fit for violent crimes, robbery, property, burglary, and possibly larceny is the 1966-1968 impact model. See id. at 1096 & tbl.IV.

n16. Cf. Donohue, supra note 2, at 1158 (raising social changes in the 1960s); id. at 1152 (raising police recordkeeping practices). A related reason to reject the recordkeeping hypothesis is that in both 1966 and 1967 declining clearance rates were "universally reported." See note 40 infra and accompanying text. Recordkeeping procedures are unlikely to change universally at the same time.

n17. See Cassell & Fowles, supra note 1, at 1119 (collecting citations to evidence on each of these points). See generally Paul G. Cassell, All Benefits, No Costs: The Grand Illusion of Miranda's Defenders, 90 Nw. U. L. Rev. 1084 (1996); Paul G. Cassell, Miranda's "Negligible" Effect on Law Enforcement: Some Skeptical Observations, 20 Harv. J.L. & Pub. Pol'y 327 (1997); Paul G. Cassell, Miranda's Social Costs: An Empirical Reassessment, 90 Nw. U. L. Rev. 387 (1996) [hereinafter Cassell, Miranda's Social Costs]; Paul G. Cassell & Bret S. Hayman, Police Interrogation in the 1990s: An Empirical Study of the Effects of Miranda, 43 UCLA L. Rev. 839 (1996); Paul G. Cassell, Protecting the Innocent from False Confessions and Lost Confessions - and from Miranda, 88 J. Crim. L. & Criminology 497 (1998).

n18. See Donohue, supra note 2, at 1173 tbl.I (second bottom panel).

n19. Id. at 1174 tbl.II (second bottom panel).

n20. Donohue, supra note 2 , at 1171.

n21. Compare Donohue, supra note 2, at 1168-69 (suggesting that something "other than the factors controlled for in the various regressions happened in the mid-1960s to depress violent crime clearance rates"), with Stephen J. Schulhofer, Miranda and Clearance Rates, 91 Nw. U.L. Rev. 278, 280 (1996) (stating that "we need only turn to levels of crime and police resources during the period" to understand the clearance rate decline) (emphasis added).

n22. See Donohue, supra note 2, at 1169.

n23. See note 4 supra and accompanying text.

n24. See Cassell & Fowles, supra note 1, at 1081 & n.137 (noting concerns about trend variable).

n25. One possible justification would be that the time-squared variable had significant explanatory power, but in Donohue's best equations on violent crime time-squared shows no statistically significant effect. See Donohue, supra note 2, at 1173 tbl.I (second bottom panel).

n26. See id.

n27. Id.

n28. See, e.g., Nat'l Inst. of Justice, Public Policing - Privately Provided 4 (1988) ("Some of the tasks commonly carried out by sworn public police officers can alternatively be performed by other public or private employees paid by the government."); Bruce L. Heininger & Janine Urbanek, Civilianization of the American Police: 1970-1980, 11 J. Police Sci. & Admin. 200 (1983) (reporting empirical research that "using civilians probably does not "displace' sworn officers, and there is no apparent relationship between civilians and ... the quality of police protection").

n29. See Nat'l Inst. of Justice, supra note 28, at 5-6 (noting that private firms "increasingly are performing investigations and making arrests for specialized crimes" and that in some states they have "been granted limited powers of peace officers ... rather than allowing legal distinctions to interfere with the growing involvement of private firms in the provision of police-related services").

n30. See Donohue, supra note 2, at 1153-55, 1164-65.

n31. Index crimes, of course, represent only a fraction of total police work, so other broader measures are, in principle, preferable. The only consistently reported national data over the relevant time period, however, are index crimes.

n32. There are very minor differences between our Table I and Donohue's tables, produced by the slightly different software packages we each used.

n33. It is also interesting that, after excluding time squared from the equations, there was no particular indication of a serious problem of autocorrelation, as measured by the Durbin-Watson ("D.W.") statistic. For example, without time-squared the D.W. statistic for the violent crime equation is 1.9274; with time-squared included it fell slightly to 1.9259.

n34. See Donohue, supra note 2, at 1165-66 & 1174 tbl.II.

n35. See Cassell & Fowles, supra note 1, at 1103-06.

n36. See Richard Fowles & Mary Merva, Wage Inequality and Criminal Activity: An Extreme Bounds Analysis for the United States, 1975-1990, 34 Criminology 163, 166-69 (1996) (explaining how EBA detects multicollinearity problems).

n37. For the identical pattern in our original equations, see Cassell & Fowles, supra note 1, at 1105 tbl.VII.

n38. Donohue also suggests that the data is imperfect because the changing composition of reporting cities could have an effect on clearance rates. We flagged same possibility in our original paper, see Cassell & Fowles, supra note 1, at 1076, but explained that the effect of these fluctuations in national data was relatively small, see id. at 1076 n.105 (citing James Alan Fox, Forecasting Crime Data: An Econometric Analysis 127 n.11 (1978) ("Although the group of cities included in the FBI tabulations does change annually, the extent of error resulting from these fluctuations is minimal relative to the aggregate data.")).

n39. See Cassell & Fowles, supra note 1, at 1075-76 (collecting materials on this issue).

n40. See id. at 1068 (quoting Uniform Crime Reports for 1966 and 1967).

n41. The data on larceny may be the worst of the seven index crimes. See Cassell & Fowles, supra note 1, at 1136 (noting problems with larceny data). This explains why the explanatory power of both Donohue's equations and ours are so much lower for larceny than any other crimes.

n42. See Donohue, supra note 2, at 1156 n.52.

n43. See John J. Donohue III & Peter Siegelman, Is the United States at the Optimal Rate of Crime? (ABF Working Paper Preliminary Draft, Feb. 13, 1995) (plotting homicide data over time and noting that homicide is an "imperfect" proxy for developments in aggravated assault and robbery).

n44. See Cassell & Fowles, supra note 1, at 1090-91 & n.162 (noting the decline in "family victim" homicides since 1965); Donohue & Siegelman, supra note 43, at 47 (noting the recent rise in gang related murders and its impact on homicide clearance rates).

n45. See Donohue, supra note 2, at 1155; see also Cassell & Fowles, supra note 1, at 1090-91 (raising the shift in resources possibility and providing supporting data from Pittsburgh). From this fact, Donohue argues that Miranda's costs may be limited to resource costs, since society could raise crime clearance rates by spending

more resources on law enforcement. See Donohue, supra note 2, at 1151. But while more resources could, if made available, mitigate some of Miranda's costs, it also appears likely that perhaps the most serious criminals - the "professionals" - are simply placed outside the reach of effective police investigation. See Cassell, Miranda's Social Costs, supra note 17, at 464-65 (collecting evidence to this effect). Donohue also suggests that our findings on homicide clearance rates undermines Justice White's argument that murderers would be set free because of Miranda. See Donohue, supra note 2, at 1151 n.25. While our findings do not confirm White's concern, our methodology is best suited to detecting Miranda's effects on investigations that clear multiple crimes through a single police interrogation. Because serial killers are quite rare, we were perhaps less likely to detect Miranda's effects on homicide than for any other crime. Moreover, Justice White was predicting not that Miranda would harm police investigations, but subsequent criminal prosecutions; our methodology does not fully investigate this issue. See note 4 supra and accompanying text.

n46. See Cassell & Fowles, supra note 1, at 1116 & n.284 (collecting evidence to this effect).

n47. See Table I (finding a statistically significant Miranda effect for vehicle theft in five of seven specifications).