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***500 MIRANDA'S PRACTICAL EFFECT: SUBSTANTIAL BENEFITS AND VANISHINGLY SMALL
SOCIAL COSTS**

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***501** This year marks the thirtieth anniversary of the Supreme Court's landmark decision in *Miranda v. Arizona*. [\[FN1\]](#) *Miranda* held that custodial police interrogation involves inherently compelling pressures and that, in the absence of safeguards sufficient to dispel that pressure, any statement given under custodial interrogation must be regarded as the product of compulsion, in violation of the Fifth Amendment. [\[FN2\]](#) Widely maligned at first, *Miranda* has gradually won acceptance across a broad spectrum. Among ordinary citizens and law enforcement professionals alike, a two-fold perception is now pervasively shared. First, compliance with the *Miranda* safeguards is widely considered an elementary prerequisite of fair procedure and the decent restraint of police power. [\[FN3\]](#) Second, the *Miranda* safeguards do not pose any serious impediment to effective law enforcement. [\[FN4\]](#)

Professor Paul Cassell challenges the empirical strand of the current conventional wisdom. [\[FN5\]](#) Meticulously revisiting a series of before-and-after studies conducted at the time of the *Miranda* decision, he estimates that *Miranda* was (and still is) responsible for the loss of a conviction in 3.8% of all serious criminal cases. He also argues that this 3.8% attrition represents a substantial social cost and that we could avoid this cost by replacing the *Miranda* safeguards with a different set of standards.

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Professor Cassell makes no effort to argue, as did early critics of Miranda, that the decision has had catastrophic effects on law enforcement. [\[FN6\]](#) On the contrary, the 3.8% attrition figure that forms the centerpiece of Cassell's article stands as one of the many refutations of *502 those apocalyptic claims. Miranda's supporters have long argued that the decision's effect on conviction rates was negligible. [\[FN7\]](#) Cassell's 3.8% figure could well be offered as evidence for the pro- Miranda side of this debate.

It is a surprise to discover, therefore, that even the scaled-down claim of a 3.8% impact quickly collapses. Professor Cassell's focus on old studies of the immediate post- Miranda period, before police had a chance to adapt to the new framework, carries a built-in risk of exaggerating Miranda's current impact on police effectiveness. But even if we accept Cassell's basic approach to the old before-after studies, a close look at the details shows that inconsistent and highly partisan procedures are necessary to bring Miranda's supposed attrition effect up to Cassell's 3.8% figure. Although Cassell is sometimes cautious and willing to forego exaggerated claims, at critical points in his analysis, data are cited selectively, sources are quoted out of context, weak studies showing negative impacts are uncritically accepted, and small methodological problems are invoked to discredit a no-harm conclusion when the same difficulties are present -- to an even greater extent -- in the negative-impact studies that Cassell chooses to feature. If we accept Cassell's premise that the old studies are relevant, and reanalyze their data with all necessary qualifications in mind, we find that the properly adjusted attrition rate is not 3.8% but at most only 0.78% -- a mere seventy-eight one-hundredths of one percent for the immediate post- Miranda period, and most likely even less today. For all practical purposes, Miranda's empirically detectable harm to law enforcement shrinks virtually to zero.

In a large country with a high crime rate, even 0.1% of all arrests represents a lot of cases. More to the point, the release of only one guilty murderer or rapist is one too many. A single case of that sort must be counted as a substantial social cost, if the rules requiring that disposition are unjustified and if the harm they cause is avoidable. Whether the right estimate for lost cases is 3.8% or (as I believe) only a small fraction of that figure, the key question is whether the rules leading to those results -- the Miranda rules -- are justified by their constitutional mandate. To carry the day, an alternative to Miranda not only must promise more convictions, but also must preserve justice and respect for constitutional values in the 99% (or perhaps only 96.2%) of convictions that will be obtained successfully under either regime -- and in all the arrests that will not produce convictions under either regime.

Professor Cassell would replace the Miranda safeguards with a system of diluted warnings and restricted counsel for the indigent, and *503 he would abrogate Miranda's requirement that police must cease questioning a suspect who unequivocally asserts the desire to remain silent. In effect, Cassell's regime would authorize the involuntary interrogation of unwilling suspects while they are held in police custody. Cassell would peel away those parts of Miranda that he considers most inconvenient, but he offers no theory to connect the features of his "compromise" to the Fifth Amendment principles that necessarily govern. Yet with or without a video camera to record the procedure, involuntary custodial interrogation violates the arrested suspect's privilege against self-incrimination, under any defensible conception of the Fifth Amendment. And a "compromise" that exposes hundreds of thousands of unwilling arrestees to the certainty of involuntary interrogation, with only the vaguest limits on its duration and the methods that can be deployed against the protesting suspect, all in an elusive quest for an uncertain (but at most extremely small) crime control benefit, is a poor bargain to boot.

Part I of this Article examines the empirical evidence and explains why, once we make essential adjustments to the data, Miranda's detectable net impact on conviction rates shrinks virtually to zero. Part II turns to the critical normative issues and considers whether alternative regimes for controlling police interrogation can avoid any net costs that Miranda might possibly inflict. I reach three conclusions. First, videotaping of interrogation would be a useful complement to, but not a substitute for, the Miranda safeguards. Second, other genuine alternatives to Miranda have law enforcement costs that dwarf those of Miranda itself. Third, Professor Cassell's proposed replacement for Miranda, permitting police to press forward with an

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unwanted custodial interrogation, cannot be seen as an alternate mechanism for protecting suspects from the pressures of custodial questioning; on the contrary it would expose them to direct violations of their Fifth Amendment privilege. Even if Miranda poses a problem for effective law enforcement (and there is no reason to believe that it does), Cassell's proposal would not be a desirable or constitutionally permissible solution.

I. Empirical Estimates of Miranda 's Impact

Shortly after the Miranda decision, a flurry of studies attempted to measure its impact on confession and conviction rates. Though some early, crudely designed studies did seem to show substantial effects, the weight of the evidence tended to suggest that any such effects were small and rapidly diminishing over time. [FN8] The informal *504 impressions of law enforcement personnel and other criminal justice practitioners were -- and continue to be -- strongly in accord with this view. [FN9] By the time the Meese Justice Department launched its call for overruling Miranda, [FN10] that position had scant support in the law enforcement community. The conventional wisdom had turned to the view that the Miranda rules were playing a beneficial role in sustaining the professionalism of modern police [FN11] and that their impact on conviction rates was negligible. [FN12] As a blue-ribbon American Bar Association committee reported in 1988, after extensive hearings in three cities and a carefully randomized telephone survey of over 800 criminal justice officials, "[a] very strong majority of those surveyed -- prosecutors, judges, and police officers -- agree that compliance with Miranda does not present serious problems for law enforcement." [FN13]

Professor Cassell challenges this view through a re-examination of before-after studies conducted in the immediate aftermath of the Miranda decision. [FN14] He concludes that confessions were not obtained, when they would have been obtained before Miranda, in 16% of all interrogations. [FN15] He also estimates that such confessions were necessary for conviction in 24% of the cases. [FN16] The confessions not obtained in cases where they were necessary for conviction (.16 x .24) represent convictions lost because of Miranda in 3.8% of all serious criminal cases.

*505 Professor Cassell's effort to compile and analyze the available empirical evidence is thorough and detailed. He recognizes that none of the available studies is as solid as one would wish and correctly emphasizes the need for more and better empirical research.

For now, however, the variously flawed studies are all we have, and Cassell devotes considerable effort to milking them for empirical conclusions. This move is surely debatable, but the size of a legal problem does matter, and we cannot avoid thinking about it, or rely only on our intuitions, just because a perfect study has yet to be done. Cassell's difficult enterprise of sifting the crude evidence deserves to be taken seriously. Estimates derived in this fashion can be useful, provided that all the important methodological problems are taken into account.

My review of the data suggests many essential qualifications that Cassell overlooks or decides to deemphasize. Some of these qualifications raise large doubts about the before-and-after studies in general; other caveats require only small changes in the magnitude of a particular estimate. But even the minor details are critical. We are attempting to tease out an impact that, on Professor Cassell's analysis, affects 3.8% of criminal cases. With small adjustments, the critical effects can quickly go to zero. My examination of the data suggests that several such adjustments are required. The cumulative effect of these adjustments radically alters Professor Cassell's bottom-line conclusion.

My discussion of the empirical sources proceeds in four steps. Subpart A discusses the current relevance of before-after measurements taken in the immediate wake of Miranda, and subpart B considers issues that affect the validity of any before-after comparison. Subpart C evaluates the before-after studies that form the core of Professor Cassell's analysis, and subpart D shows why, after making necessary adjustments, the best estimate for net cases lost due to Miranda falls from 3.8%

to something close to zero. Subpart E recapitulates the results of the empirical analysis.

Because the discussion to follow becomes more than tolerably technical and detailed, a brief overview of the terrain ahead is in order. I explain first why any measurement of Miranda's negative impact in the immediate post- Miranda period greatly overstates Miranda's current social costs, if any. I then turn to the old before-after studies and consider the adjustments that are required by data problems specific to each of the eleven studies analyzed by Cassell. When these site-specific adjustments are taken into account, the confession-rate change attributable to Miranda shrinks from 16% (Cassell's figure) to 6.4%. [\[FN17\]](#) Several adjustments not specific to particular *506 sites are explained in subpart D, and these reduce this 6.4% estimate to 4.1%. Finally, a detectable drop in the confession rate does not translate directly into lost convictions because, as Professor Cassell recognizes, confessions are not necessary for conviction in all cases. After needed adjustments to Cassell's necessity-rate figure, the lost-conviction rate shrinks still further. Even if we accept the usefulness of before-after comparisons focused on the immediate post- Miranda period, the best estimate of lost convictions that such studies will support is at most a mere 0.78%, and Miranda's impact on current conviction rates is almost certainly even lower. For practical purposes, Miranda's empirically demonstrable harm to law enforcement is essentially nil.

The detailed data analysis that necessarily dominates this Part will no doubt leave many readers daunted, distracted, or bored to death. Those whose eyes glaze over or who jump impatiently ahead have my sympathy. But the theme of this Part is straightforward. Professor Cassell argues that because of Miranda, convictions are lost in 3.8% of criminal cases, but inconsistent assumptions and methodological double standards are required to generate an attrition rate that rises to this level. This Part shows that with necessary adjustments, the 3.8% figure shrinks to a far smaller number and rapidly approaches zero.

A. Are the Immediate Post- Miranda Studies Relevant Today?

Professor Cassell wants to base his estimate of Miranda's current impact on a series of before-after studies conducted almost thirty years ago. There are two major problems here. First, all of the studies suffer from significant methodological flaws. Second, even if we can assume that the studies give a reliable picture of Miranda's costs thirty years ago, there is strong reason to believe that such costs were transitory and that confession rates have since rebounded from any temporary decline.

1. Common Methodological Flaws. -- All the before-after studies were crudely designed attempts to capture Miranda's immediate impact. Few, if any, included all necessary segments of the caseload, used proper sampling procedures, insured strict equivalence of the groups compared, and controlled for relevant causal variables other than Miranda. [\[FN18\]](#) As Professor Richard Leo notes in a comprehensive review of this literature, "virtually all [the studies] were conducted by *507 lawyers or law professors not trained in the research methods of social science [and they] are replete with methodological weaknesses . . . [T]he methodological weaknesses of virtually all of the Miranda impact studies should necessarily temper, and in some instances should cause us to question, the conclusions that have been drawn from these studies." [\[FN19\]](#)

Because the studies are poorly designed, they cannot by themselves prove that Miranda does not damage law enforcement. The "conventional wisdom" about Miranda's negligible costs is not based on such studies alone but on a distillation of numerous factors -- the weak and inconsistent effects detected in the early studies, [\[FN20\]](#) pervasive evidence that confession rates rebounded as police adjusted to Miranda, [\[FN21\]](#) and the practical experience of officers in the field who, since the mid-1970s, have consistently reported their impressions that compliance with Miranda does not produce significant negative

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effects. [\[FN22\]](#) All this evidence might be misleading or incorrect, but to raise substantial doubts about its validity requires more than speculation or admittedly flawed studies that are more than a quarter-century old. To build a refutation of "conventional wisdom" on the kinds of studies Cassell so meticulously tabulates is equivalent to building a modern computerized courthouse on a foundation of sand.

2. Adaptation to Miranda . -- Law enforcement professionals and academic researchers consistently report that police officers adjusted to Miranda over time and that any negative impacts quickly dissipated. Officers learned how to avoid mistakes that would create admissibility problems; they adapted interviewing methods so they could honor constitutional rights and still get confessions; and they altered investigatory practices so they could bolster the evidence in cases where no confession was obtained. Since the mid-1970s, the body of police testimony to this effect has been widespread and consistent.

[\[FN23\]](#) *508 Against this background, Miranda's current net cost (if any) is almost certainly far lower than the costs incurred in the immediate post-Miranda period.

Cassell cannot avoid acknowledging the importance of this accommodation or "rebound" effect. [\[FN24\]](#) His strategy is to dismiss it as a mere "hypothesis," no more plausible than the competing possibility that "as police have complied more strictly with the Miranda rules, confession rates [might] have dropped even further than shown in the early studies." [\[FN25\]](#) This is an imaginative but hollow speculation. The confession rates at issue, we must remember, are not those observed in cities where initial compliance with Miranda was sporadic, because Cassell decides (correctly) to exclude these cities altogether when constructing his estimate of Miranda's initial impact. [\[FN26\]](#) Since the only cities included in the calculus of Miranda's initial impact are those in which compliance with Miranda was reasonably rapid and complete, the problem of inadequate compliance has already been taken into account.

Cassell offers no evidence -- empirical, impressionistic, or otherwise -- to support his conjecture that confession rates in the selected high-compliance cities might have declined after the initial post- Miranda period. [\[FN27\]](#) Instead he attempts to put the two competing hypotheses on an equal footing, by suggesting that there is no evidence to support the rebound hypothesis either. But that claim is simply untenable. *509 As Cassell's own table of the empirical research makes clear, many post-1970 studies -- including his own 1994 study of Salt Lake City -- report confession rates that equal or even exceed those recorded prior to Miranda. [\[FN28\]](#) Moreover, "data" of this sort -- though important when available -- is not the only legitimate way to choose between plausible speculations. Another possibility is to find knowledgeable people and ask them. This has been done, and the answers are clear -- a "very strong majority" of law enforcement professionals surveyed since the mid-1970s have reported satisfactory accommodation to Miranda; [\[FN29\]](#) there are virtually no complaints (and Cassell cites none) that increasing compliance has aggravated Miranda's effects over time.

Richard Leo's 1994 empirical study is especially telling here. [\[FN30\]](#) In a careful and balanced appraisal of interrogation in three California cities, Leo finds that interrogation tactics were changing prior to Miranda, that such tactics continued to change in response to Miranda's requirements, and that interrogators have become very skilled at using deception and psychological manipulation to elicit confessions. [\[FN31\]](#) While his data do not permit any direct before-after comparison, he reports that the police he observed in the 1990s obtained confessions or other useful statements in 64% of their interrogations -- a confession *510 rate much higher than most of the estimates available for the period prior to Miranda. [\[FN32\]](#) Leo's data give no hint of a countervailing "compliance" effect and, to the contrary, strongly support the prevailing view that Miranda does not significantly burden the interrogation process. Indeed, Leo argues, now that police have learned how to work with Miranda, "warnings may, in part, actually aid detectives in obtaining confessions" because they "foster[] the illusion that the

suspect and investigator share a commonality of interest." [\[FN33\]](#)

In short, there is strong and consistent evidence, from diverse sources, that the law enforcement burdens experienced in the immediate wake of Miranda represent the high-water mark of Miranda's negative impact. Any current damage to law enforcement is almost certainly smaller. Thus, Cassell's estimate of 3.8% lost convictions in the late 1960s, if sound, greatly overstates Miranda's current impact. As the next subparts indicate, however, Miranda's empirically detectable costs, even in the immediate post- Miranda period, were only a small fraction of Cassell's 3.8% estimate.

B. Drawing Inferences from Before-After Comparisons

Comparing a situation before Event X to the situation after Event X can support valid inferences only if the variables measured *511 before and after are truly comparable. Many of the before-after studies concerning Miranda are critically flawed because the kinds of cases examined or the kinds of statements counted as confessions were not identical in the before and after periods. Often it was not even clear that the first period studied was really a "before" or that the second period was really an "after": Miranda's requirements were anticipated in some jurisdictions before the decision was announced, and elsewhere those requirements were ignored long after they became the law. Professor Cassell pays close attention to these kinds of issues, and they will be a major concern when I turn to an examination of specific before-after studies. [\[FN34\]](#)

Imagine, however, a carefully designed study that measured confession rates for the same kind of cases, investigated in the same way by the same police personnel, for a full year before any Miranda requirements were introduced and for a full year after Miranda was conscientiously implemented. The study reports a confession rate of 60% before and 45% after. Are we now justified in concluding (as Cassell would) that Miranda was responsible for losing confessions in 15% of the cases?

Not necessarily. Time-series analysis poses tricky issues in social science research, and even when studies are carefully designed to insure comparable methods of measurement, there are a number of obstacles to drawing valid causal inferences. [\[FN35\]](#) If the confession rate really did drop by 15%, the conclusion that Event X (Miranda) caused the drop is only a hypothesis. Several competing hypotheses may be equally plausible. Three of these possibilities are especially relevant to before-after comparisons of Miranda: the observed change may be the result of a long-term trend unrelated to Event X, the change may be the result of other Events Y or Z that are close in time but unrelated to Event X, or the change may be the result of instability (random fluctuations) in the observed variable. In addition, we must be clear about the content of the "before- Miranda" baseline against which Miranda is being compared.

1. Long-term Trends. -- Considerable anecdotal and empirical evidence suggests that confession rates in the 1960s had started to drop well before Miranda.

[\[FN36\]](#) Some of this drop may be attributable to anticipatory implementation of Miranda-like requirements, but that is *512 clearly not the whole story. Other trends were at work. Police departments were becoming more professionalized, physical abuse was becoming less frequent, and courts applying the due-process voluntariness test were becoming less tolerant of extreme psychological pressure and marathon, all-night interrogation sessions.

[\[FN37\]](#) Concurrently, the population (including the population of criminal suspects) was becoming better educated, better informed about constitutional rights, and less deferential to authority. [\[FN38\]](#)

These developments seem to have produced modestly declining confession rates prior to and independent of Miranda. [\[FN39\]](#) A well-designed comparison of Period A (e.g., 1965) to Period B (1967) would probably show some drop in the confession rate, even if no Miranda-like requirements had been implemented in the second period.

How large was this trend-driven drop in the confession rate? A long-term trend could not account for a 25% drop over a few months before and after Miranda. [\[FN40\]](#) But small adjustments can affect Cassell's sensitive bottom-line figure, and some small adjustment for the long-term trend factor seems imperative here. The New Haven study, for example, found a 10 -15% drop in the confession rate from 1960 to 1966. [\[FN41\]](#) Professor Cassell acknowledges that this 10 -15% drop cannot be attributed to Miranda [\[FN42\]](#) and that it therefore seems to reflect a long-term trend. [\[FN43\]](#) We have no data sufficient for specifying the extent of this trend on a national basis, but in the absence of better data, the New Haven figure, a decline of 2% per year during the early and mid-1960s, can serve as a rough approximation. This long-term rate of decline must be subtracted from any total before-after change, in order to isolate the portion of the change that might be attributable to Miranda.

2. Competing Causal Events. -- Analytically distinct from the hypothesis of a long-term trend is the hypothesis that observed changes might be caused by a discrete event, close in time to the event under study but not caused by it. Two important candidates for "competing event" status are *Mapp v. Ohio* [\[FN44\]](#) and *Gideon v. Wainwright*. [\[FN45\]](#) *Mapp*, decided in 1961, and *Gideon*, decided in 1963, were already on the books when "before- Miranda" measurements were taken, but these *513 decisions (like *Miranda* itself) were implemented slowly and imperfectly in many jurisdictions. Since their effects would be felt more strongly in a post- *Miranda* period (e.g., 1967) than in a pre- *Miranda* period (e.g., 1965), those effects must be subtracted in order to isolate the portion of any change that might be attributable to *Miranda* itself. [\[FN46\]](#)

Mapp and *Gideon* could affect the confession rate in a number of ways. To the extent that *Mapp* reduced the frequency of illegal searches and seizures, police would be less likely to have physical evidence for use in confronting suspects under interrogation. *Gideon* increased access to counsel not only in states that had formally denied any right to appointed counsel but also in those states that theoretically recognized the right but had only haphazard methods of implementing it. [\[FN47\]](#) Although *Gideon* did not by itself afford a right to counsel at the interrogation phase, the right to appointed counsel meant that more suspects had some pre-interrogation contact with counsel, and if not, they learned later on, when they did meet with counsel, that they should have kept their mouths shut. This last effect of *Gideon* could generate declining confession rates among suspects who had previously been arrested, since "repeat players" interrogated post- *Miranda* would be more likely to have had some prior contact with counsel than would repeat players who were interrogated pre- *Miranda*.

The magnitude of these effects is a matter for speculation and might not be especially large. The declining confession rates in New Haven over the 1960 - 66 period, previously discussed in connection with long-term trends, may also reflect, in part, the impact of discrete events such as *Mapp* and *Gideon*. As an approximation, we may take the 2% average annual decline as a rough estimate of the combined effect of long-term trends and discrete events independent of *Miranda*.

3. Instability. -- Like crime rates, conviction rates, and automobile accident rates, confession rates in any police department no doubt vary from month to month and from year to year. A drop in any of these rates from year A to year B could be the result of an important intervening event (appointment of a new police chief, imposition of new interrogation rules), or it could be purely the result of random fluctuation. Changes that are very large (relative to the pattern of up-and-down movements in other years) are less likely to be due entirely *514 to chance, but even then, the size of the change could be

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partly due to chance. [\[FN48\]](#)

In studies comparing two groups of known sizes, we can determine the exact probability that an observed difference between the groups is merely due to chance. By convention in the social sciences, a difference is considered "statistically significant" if there is less than a .05 (or .01) probability that the difference could have occurred by chance.

Statistical significance, in its conventional sense, is not a problem in most of the before-after studies of Miranda, if other methodological problems can be put aside, most of the observed differences are statistically significant. [\[FN49\]](#)

But statistical significance does not avoid the instability problem. Statistical significance indicates that two populations (for example, those drawn from the "before" and "after" periods) are probably different in fact, but it cannot determine the cause of that difference, which may simply be the result of large year-to-year ups and downs, independent of any event intervening between the two years. And there is no agreed procedure in the social sciences for determining whether instability accounts for a difference between two years in a time-series analysis. [\[FN50\]](#)

4. Baselines. -- A recurrent question in measuring Miranda's impact is, "Compared to what?" In the "before- Miranda" world of the 1960s, many police departments used high-pressure tactics that might be questionable under today's voluntariness standards, other departments were more scrupulous but gave suspects no warnings of their rights, and still others did give some form of warnings. Whether we are attempting to measure Miranda's impact as a matter of purely historical interest or as a question of current policy and reform, we must be clear about the kind of legal regime that serves as the before- Miranda or instead-of- Miranda point of comparison.

In legal scholarship, moreover, historical inquiry is seldom pursued purely for its own sake; policy and reform are the ultimate objectives. In that context, the implicit focus of interest, as a point of comparison to "after Miranda," is never really the "before- Miranda" period but instead the regime that would exist after the "after- Miranda" period. A before-after comparison will have limited relevance for current policy if the instead-of-Miranda regime for the 1990s differs *515 significantly from the before-Miranda regime on which the historical research focuses.

The due-process voluntariness requirement, as interpreted in the 1960s, provides an especially tricky baseline. A comparison of Miranda to this regime is of legitimate historical interest, but of much less policy interest. Though we can speak loosely of "the" voluntariness requirement, voluntariness is in fact not a discrete test but a shifting set of numerous standards, which are not as permissive today as they were in the 1960s. [\[FN51\]](#) Thus a comparison of conviction rates under Miranda to conviction rates under the 1960s voluntariness test would probably exaggerate the gain, if any, to be expected from replacing Miranda with a voluntariness standard today.

For similar reasons, none of the voluntariness tests can provide an accurate baseline for policy purposes, if the instead-of-Miranda regime would require warnings or some other system of safeguards to supplement voluntariness standards. Though at least one prominent legal scholar advocates replacing Miranda with a traditional voluntariness approach, [\[FN52\]](#) warnings requirements of some sort have been a prominent feature of nearly all the replacement regimes proposed by Miranda's critics, including the Meese Justice Department [\[FN53\]](#) and Professor Cassell himself. [\[FN54\]](#) For purposes of policy assessment and reform, therefore, the most relevant measure of Miranda's impact is a comparison of conviction rates under Miranda to conviction rates under a regime that warns arrested suspects of their rights.

The analysis that follows attempts to keep clear these distinct conceptions of Miranda's "cost" by focusing, where the

evidence permits, on comparisons of Miranda to both traditional voluntariness standards and regimes with the kinds of alternative safeguards that are pertinent to potential reform.

* * *

The four problems canvassed here -- long-term trends, competing events, instability, and baselines -- must be kept in mind when we turn in the next subpart to considering what inferences we can draw from specific before-after studies.

***516 C. Before-After Changes at Specific Sites**

Professor Cassell meticulously reviews eleven before-after studies designed to test Miranda's effect on confession rates. My own analysis essentially parallels Cassell's with respect to three of the studies, and in five others I find a need for relatively minor corrections. For three of the studies, my view differs more substantially from Cassell's.

1. Pittsburgh. -- The Pittsburgh study [\[FN55\]](#) found that detectives obtained confessions or other useful statements from 48.5% of suspects during the 1963 - 66 period (prior to Miranda), but only from 32.3% of suspects during the 1966 - 67 period (after Miranda), for a loss of confessions in 16.2% of the cases. In a third sample, drawn in the summer of 1967, the confession rate was an even lower 27.1%, for a drop of 21.4% from the pre-Miranda baseline. Professor Cassell uses a weighted average of the 16.2% and 21.4% figures to produce an estimated drop of 18.6%. [\[FN56\]](#)

Cassell notes that Pittsburgh police were already complying with some of Miranda's requirements even in the "pre-Miranda" period: they advised the suspect of the right to remain silent and the right to counsel (but not the right to counsel free of charge). But the 48.5% confession rate in 1963 - 66 still provides a genuine pre-Miranda baseline for two reasons: First, as Cassell notes, the limited warnings were not given at the outset but were "woven into" the conversation with the suspect. Second, Cassell's own alternative to Miranda involves warnings at least as significant as those used in 1964 - 65 in Pittsburgh. [\[FN57\]](#)

Implementation of Miranda apparently was quick and conscientious in Pittsburgh, [\[FN58\]](#) and the post-Miranda confession rate of 32.3% reported for 1966 - 67 was measured in the same way as the 48.5% confession rate in the pre-Miranda period. The difference -- 16.2% -- therefore provides one usable measure of Miranda's short-run effect.

The other post-Miranda figure -- the confession rate reported for 1967 -- is more problematic. Although the pre-Miranda rate and the first post-Miranda rate were generated by the same methodology, the third figure -- the 1967 post-Miranda rate -- was not: The first two figures were based on the study author's count of usable statements, as determined from a review of completed case files, while the 1967 post-Miranda rate was based on new forms that police officers began ***517** completing immediately after interrogations. [\[FN59\]](#) Small differences are important to the final conclusions, and we have no reason to assume that officers' on-the-spot judgments about what counted as a usable statement were the same as those that the study authors had made for the first post-Miranda period by reviewing the entire file after the fact. In addition, the third figure was drawn from a much broader and not reasonably comparable set of offenses. [\[FN60\]](#) All told, the rate observed in the third sample cannot be used to estimate the change in confession rates from Pittsburgh's pre-Miranda baseline, but the 1966 - 67 rate does provide a roughly comparable figure that can be used to estimate the before-after change. For Pittsburgh, that change appears to be a drop in the confession rate of 16.2%.

2. New York County. -- At Senate hearings conducted in 1967, New York County (Manhattan) District Attorney Frank Hogan testified that in the six-month

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period prior to Miranda, 49% of the nonhomicide felony cases presented to the grand jury included a confession, but that in the six-month period following Miranda, 14.5% of such cases included a confession -- a drop of 34.5%.

[FN61] The potential significance of this sudden, sharp drop makes Professor Cassell's decision to include the New York data especially important. Yet the New York study suffers from a clear-cut and well-known flaw that renders Hogan's comparison untenable.

The difficulty is that the New York study measured a change in the rate of confessions presented to grand juries. Can this figure serve as a measure of the rate of confessions made by suspects? Under some circumstances it might, but for 1966 there is a major problem. Miranda applied to all cases tried after the date of the decision (June 13, 1966), even if defendants in those cases had given confessions prior *518 to June 13. [FN62] Since few interrogators, if any, anticipated and satisfied all of Miranda's requirements before they were announced, Miranda's partial retroactivity barred nearly all pre- Miranda confessions from post- Miranda trials; during this transition period, Miranda (and its companion retroactivity case) reduced the rate of admissible confessions virtually to zero. But this rate clearly cannot serve as an acceptable measure of Miranda's impact on interrogations conducted after June 13. Moreover, New York law barred prosecutors from presenting any evidence to the grand jury that would be inadmissible at trial. [FN63] In the immediate post- Miranda period, therefore, the count of confessions presented to grand juries would seriously understate both the number of confessions actually given by suspects in those cases and the number of confessions that could have been obtained by investigators who complied with Miranda.

These circumstances help explain why the sudden confession-rate drop in Hogan's grand jury figures is so far out of line with that reported in other contemporaneous studies. Apart from Hogan's 34.5% figure, recorded in a six-month before-after period, the largest drop Cassell reports is the 24.6% decrease estimated to have occurred over a three- to four-year period in Philadelphia. [FN64] The New York City police -- the same force that supposedly experienced a 34.5% confession-rate drop in its New York County (Manhattan) precincts -- had a confession-rate drop estimated at only 15.5% for the interrogations it conducted in the neighboring borough of Brooklyn. [FN65] And if we put New York County aside, the average for the other studies Cassell considers reliable was only 13.5%, roughly a third of Hogan's figure. [FN66] The New York County result is clearly an exaggerated outlier, the result of a transparently flawed comparison.

Professor Cassell is nonetheless determined to include the New York data in his tabulations. He offers four arguments in an effort to establish that the post- Miranda grand jury figures are a suitable proxy for the post- Miranda confession rate. His arguments are resourceful and elaborate, but ultimately quite strained.

(1) First, Cassell argues, some of the grand jury cases in the July-December post- Miranda period involved post- Miranda interrogations, and thus there would be "some dissipation" of the retroactivity effects. [FN67] This is true but irrelevant. The problem with the reliability of Hogan's so-called post- Miranda figure is that his July-December 1966 *519 cases are a mixed bag of pre- Miranda and post- Miranda interrogations. The undoubted presence of some post- Miranda confessions in the sample cannot help us determine to what extent the reported confession rate of 14.5% is artificially reduced by the presence of pre- Miranda confessions that could not be included in the totals.

Of course, if all the grand jury presentations for July-December 1966 involved post- Miranda (post-June 13, 1966) interrogations, the retroactivity problem would disappear. But Professor Cassell does not claim that this was the case, and the available evidence strongly suggests the contrary. As Cassell acknowledges, a Vera Institute study noted the "sometimes extended period" between arraignment and grand jury action in New York City. [FN68] And in a different context, Cassell mentions a pre- Miranda interrogation in a New York case that reached the indictment stage only in November 1966, more than six months after the suspect's arrest. [FN69] Thus, it is virtually certain (and undisputed) that a significant portion of

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Hogan's July-December grand jury cases involved pre- Miranda interrogations with important retroactivity problems. The case sample cannot be treated as comparable to one drawn entirely from post- Miranda interrogations.

Cassell also implies that retroactivity problems are mitigated because prosecutors presented some pre- Miranda confessions to grand juries (and may have counted these confessions in the July-December sample) even though it was illegal for them to do so. [\[FN70\]](#) But the inclusion of such cases, like the inclusion of some post- Miranda interrogations, cannot shore up the reliability of the 14.5% figure. The reported confession rate is artificially lowered because confessions from some of the suspects in the July-December sample were excluded from the count. [\[FN71\]](#) The fact that some of these confessions were not excluded reduces the downward bias but cannot eliminate it or give any indication of its extent.

(2) Cassell next seeks support in a passage from the congressional testimony in which Hogan, summarizing the grand jury data, stated that "[o]nly 15% confessed in the six months after the Miranda ruling and 49% confessed before." [\[FN72\]](#) Because Hogan's phraseology appeared to equate the post-July grand jury data with the actual confession rate in post-July interrogations, Cassell assumes that Hogan must have ascertained that the two were identical, "[b]arring misrepresentation *520 by the report." [\[FN73\]](#) But misrepresentation or hyperbole (which Cassell apparently considers unthinkable) are not the only possibilities. The sentence that proves so critical here could easily have been intended as nontechnical shorthand for summarizing the grand jury presentation rates. Or perhaps Hogan made a well-intentioned but slightly careless choice of words. Anyone who has been on either side of a law review cite check will understand the problem immediately. Due respect for Hogan's integrity surely does not compel us to accept literally and apart from its context everything he said.

The context of Hogan's comment makes Cassell's literal reading of the testimony extraordinarily strained. Hogan's statement was, after all, the submission of an advocate who opposed the Miranda innovations. Cassell's attempt to get so much mileage from the phraseology of a single sentence in such a submission makes it necessary to quote the relevant passage in full. Following an explanation of how the grand jury data were collected, Hogan reported: [\[FN74\]](#)

[Cases] which reach the grand jury are the more solid and serious felony cases. During the period from December 1965 through May 1966, such cases against 2610 defendants were presented to the grand jury.

Evidence of a confession or admission of guilt by 1280 of these 2610 defendants was presented to the grand jury. Thus, in the six month period before Miranda, roughly 49% of the non-homicide felony defendants made incriminating statements.

From July 1966 through December 1966, the six months after Miranda, cases against 2448 non-homicide defendants were presented to the grand jury. Only 354 admissions or confessions could be presented during this post Miranda period. Only 15% confessed in the six months after the Miranda ruling and 49% confessed before. Taken in context, the last sentence, on which Cassell places so much weight, clearly was intended to supply the percentage computation for the specific grand jury data that precede it (354 out of 2448), not to introduce new information about an entirely different pool of data (suspects interrogated after Miranda) that is nowhere described in Hogan's report. To read the last sentence literally and in isolation seems particularly implausible when Hogan's testimony offers no clue as to the methods he might have used to infer a confession rate for post- Miranda interrogations from the grand jury data he specifically describes.

Cassell defends his strained reading of Hogan's concluding sentence by attempting to supply the missing steps in an analysis he thinks Hogan might have made. Cassell argues that "[b]arring misrepresentation . . ., [Hogan's statement] implies that pre-Miranda confessions *521 were presented to grand juries regardless of their inadmissibility at trial, or that the number of cases affected by retroactivity was not statistically significant." [\[FN75\]](#) Apart from the fact that this effort to reconstruct Hogan's thinking is pure conjecture, it cannot succeed in saving Cassell's implausibly literal interpretation. Miranda was decided on June 13, extended periods of time often elapsed in New York between arrest and grand jury action, [\[FN76\]](#) and

yet Hogan began collecting data from his "post- Miranda" period with all cases going to the grand jury after July 1, 1966. It cannot be the case (and Cassell does not claim) that the number of pre-June 13 arrests in the July-December grand jury pool was statistically insignificant.

This leaves only the possibility that "pre- Miranda confessions were presented to grand juries regardless of their inadmissibility at trial." [FN77] Such illegal behavior could not have occurred uniformly in every pre- Miranda case, [FN78] and even if it did, Hogan's concluding sentence, read literally, would still have to be false: If prosecutors presented every pre-Miranda confession during the July-December 1966 period and recorded it in their study, we would have only a measure of the percentage who confessed during interrogations conducted in the pre- Miranda/post- Miranda straddle period, not -- as Cassell wants the sentence to mean -- those who "confessed in the six months after the Miranda ruling" [FN79]

*522 The loose language in a single sentence of Hogan's testimony simply cannot serve as a substitute for direct evidence on the percentage of suspects who confessed after the date of the Miranda decision.

(3) As a third source of support for his claim that New York's large post-Miranda drop is plausible, Cassell selectively cites data from a study of homicide interrogations in the year following Miranda. Hogan reported that 30% of homicide suspects refused to talk. [FN80] Cassell contrasts the 30% refusal rate to the situation prior to Miranda, when (as Hogan described it) "rarely did a suspect refuse to make any kind of statement, even if it was only to protest his innocence." [FN81]

Hogan's homicide study in no way supports the grand jury figures, however. In fact, when quoted in context, that study strongly undercuts the reliability of the grand jury data. The estimate reported for pre- Miranda homicide cases lumps confessions together with purely exculpatory statements, as indicated by the italicized language above, which Cassell ignores in discussing Hogan's report of his results. [FN82] The full quotation draws no direct contrast in confession rates, but only the comparison that pre- Miranda suspects usually confessed or protested their innocence, while post- Miranda suspects were more likely either to confess or simply to remain silent. Hogan's actual homicide data (not reported by Cassell) reveal the figure that is critical -- the confession rate post- Miranda -- which was 35%. [FN83] (Another 35% of the suspects gave exculpatory statements and 30% remained silent.) [FN84] The 35% post- Miranda confession rate is more than double the 14.5% post- Miranda rate recorded in the problematic grand jury figures. Far from validating the grand jury data, Hogan's homicide study reinforces the concern that his grand jury confession rate must be an artificially deflated figure.

(4) Cassell's final attempt to shore up the grand jury study is based on a Vera Institute survey that found post- Miranda confession rates of 16.8% and 23.7% in two samples of 1967 interrogations. [FN85] Cassell argues that these figures reinforce his speculation that the confession rate in the July-December grand jury presentations (14.5%) is an accurate estimate of the confession rate in July-December interrogations. In two major respects, however, the Vera sample is not comparable to the sample from which Hogan's grand jury data were drawn. The first difference is that Vera studied interrogations of all *523 arrested suspects, not just those who were ultimately prosecuted. Since the former group includes defendants whose cases were eventually dropped for lack of evidence, and since the confession rate in such cases is typically very low (or zero), the confession rate for the former group as a whole (the group Vera studied) will inevitably be lower than the confession rate for the subset of that group (studied by Hogan) which was pursued to the grand jury stage.

The second major difference between the Vera and Hogan samples reinforces this expectation that the Vera confession rates should be relatively low. Hogan's grand jury figures cover only "the more solid and serious felony cases" (other than homicides), but the Vera sample covered all felonies and all "finger-printable misdemeanors," a data base that included forty-five offenses ranging from homicide to loitering. [FN86] This difference is not merely a technical one, because misdemeanors and less serious felonies were a large segment of the Vera sample, [FN87] and the Vera study found that "[i]n

the more serious specific offenses, the percentage making statements was considerably higher than that in comparatively less serious offenses." [\[FN88\]](#)

The result of these two differences between the Vera and Hogan samples is important. If we take the group Vera studied and exclude from it the misdemeanors, the less serious felonies, and the serious cases dropped for lack of evidence, the confession rate in the remaining cases (the ones presented to the grand jury) will inevitably be much higher than the 16.8% and 23% confession rates for Vera's combined felony-misdemeanor samples of all arrested suspects. Yet Hogan somehow obtained a confession rate for the grand jury stage (14.5%) that is significantly lower than the Vera figures. Far from supporting the validity of Hogan's grand jury study, the related contemporaneous data again reinforce the other indications that the confession rate Hogan reported for the July-December period was an artificially deflated figure. [\[FN89\]](#)

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In sum, Cassell's arguments for making use of the Hogan study are imaginative and detailed, but partisan and selective. The decisive flaw remains. The confession-rate data for July-December grand jury presentations are not a direct measure of the confession rate in July-December interrogations. Whether the one rate is similar to the other is at best a matter of speculation, and all the evidence suggests that the latter rate (the one that matters) had to be significantly higher than the former rate (which Cassell wants to use). The specific numbers are crucial here, and there is no way that the grand jury figures Hogan reports can serve as an accurate count of the confession rate in post- Miranda interrogations. The Hogan study must be excluded.

3. Philadelphia. -- In 1967 Senate hearings, Philadelphia's then-District Attorney Arlen Specter reported the results of his before-after study of Miranda's effects. [\[FN90\]](#) Based on discussions with police officers and assistant D.A.s, Specter estimated that prior to Escobedo, [\[FN91\]](#) 90% of suspects made some form of statement and that immediately after Escobedo, 80% made such a statement. When Russo, a 1965 Third Circuit decision, [\[FN92\]](#) explicitly required police to warn suspects of their right to counsel, police began to collect actual data on how many suspects gave statements. After Russo and before Miranda, 68.3% gave statements, but after Miranda only 40.7% did so. [\[FN93\]](#)

To calculate the change in the rate of incriminating statements, Cassell assumes that 50% of the statements were incriminating. [\[FN94\]](#) Thus, the rate of incriminating statements in Philadelphia was estimated *525 at 45% before Escobedo; 40% in the post- Escobedo, pre- Russo period; 34.2% post-Russo, pre- Miranda; and 20.4% post- Miranda.

A variety of questions have been raised about the Specter study, including the question whether the cases sampled in the last two periods were strictly comparable to one another. [\[FN95\]](#) Cassell gives fair responses to these arguments, [\[FN96\]](#) and though doubts may remain, the Specter study seems a plausible candidate for inclusion in a tally of roughly comparable before-after measurements.

Though I proceed here by accepting the assumptions of Professor Cassell's narrow before-after focus, it is important to recall that this focus isolates the confession-rate drop immediately after Miranda, before police had time to adjust interviewing techniques to the Miranda requirements; an impact measured in this way will tend to exaggerate Miranda's effect. [\[FN97\]](#) This caveat is especially important for the Philadelphia data because Specter himself has stated publicly that whatever Miranda's initial impact, he believes that police have now adapted to its requirements and that confession rates have not suffered. [\[FN98\]](#)

In an immediate before-after comparison like Cassell's, the critical question is to determine the baseline against which to compare the post- Miranda rate, which for Philadelphia was 20.4%. Cassell uses the pre- Escobedo rate of 45%, but there are three major difficulties here. First, the 45% figure (unlike the 20.4% post- Miranda figure) was not drawn from an actual count but is simply a crude, ballpark estimate based on recollections and conversations; this cannot be an acceptable method for determining numbers when small differences are important.

Second, the 45% and 20.4% figures were obtained by taking Specter's reported statement rates (90% and 40.8%) and then assuming that half the statements were incriminating. But we cannot assume that the proportion of statements that were incriminating remained constant before and after warnings were required; on the contrary, Cassell's own sources indicate that the proportion of statements that were incriminating rose after Miranda because some suspects who made purely exculpatory statements before Miranda chose to remain silent once warnings were required. [FN99] The exact magnitude of the change is not known, but we cannot assume that the proportion of incriminating statements remained constant, and any conclusions based on Specter's pre- Escobedo baseline are extremely sensitive to *526 assumptions about what the change might have been. If the proportion of statements that were incriminating rose from 40% before warnings were required to 50% after, then the rate of incriminating statements would be 36% before Escobedo (.90 x .40) and 20.4% after Miranda (.408 x .50); the correct estimate for the confession-rate drop would not be 24.6% (Cassell's figure) but only 15.6%.

The third problem in attempting to use Specter's estimate for the pre-Escobedo period as a baseline is that pre- Escobedo confession rates are relevant only if the pre- Escobedo legal regime is the one being offered as the alternative to Miranda. But this is not the case, because Cassell's proposed substitute for Miranda involves a set of detailed warnings roughly comparable to those the Philadelphia police began giving only after the Russo decision. [FN100] In the pre- Escobedo period, in contrast, Philadelphia police apparently were not required to give any warnings at all. [FN101] Moreover, the voluntariness test, as understood in Pennsylvania before 1964, fell considerably short of today's due process standards that Cassell wants to retain; immediately prior to Escobedo, investigators still used, and Pennsylvania courts still approved as "standard practice," such interrogation tactics as questioning suspects after they had been stripped naked. [FN102]

*527 It would not be surprising if 90% of the Philadelphia suspects did give statements under such a regime, but Cassell does not propose a return to that world, so the confession rate it produced cannot serve as the appropriate baseline for a policy-oriented comparison. The legal regime closest to the one Cassell advocates is the one that Philadelphia police followed after Russo but before Miranda, [FN103] and this is also the period for which statement rates were determined by an actual count, under procedures comparable to those used to determine the post- Miranda rate. The correct "pre- Miranda" baseline for Philadelphia therefore is clearly the 34.2% post- Russo, pre- Miranda rate, and the confession-rate drop following Miranda was 13.8%. [FN104]

For purposes of a purely historical inquiry, designed to compare Miranda to the 1960s voluntariness regime, the proper treatment of the Philadelphia data is more debatable. One could either discard the 13.8% figure (the change from post- Russo to post- Miranda) as irrelevant or one could treat it as a useful but possibly low estimate. In either event, we cannot use the pre- Russo estimates as an appropriate *528 basis for a "before- Miranda" comparison, because the confession-rate figures for this period are hopelessly speculative.

4. "Seaside City". -- James Witt's study of a small community in the Los Angeles area [FN105] is especially interesting because its author was a former police officer to whom "Seaside" police had repeatedly reported that Miranda's impact was substantial. [FN106] Witt expected that his empirical study would document the reported effects. [FN107] But he discovered that

68.9% of suspects had made incriminating statements before Miranda and that 66.9% had done so afterward -- a drop of only 2%.

Cassell includes the Seaside figure in his calculations but believes that it probably understates Miranda's effect because the study did not cover one category of suspects (those detained for questioning but not incarcerated) and because Witt did not give details about how fully the Seaside police were complying with Miranda. Information gaps of this sort affect all the before-after studies, however; in this instance there is no reason to believe that the missing details distort the Seaside results. If anything, the interviews in which police reported their impressions of a large post-Miranda impact [\[FN108\]](#) tend to suggest that those officers believed compliance with Miranda's requirements to be high.

Cassell also notes that in the pre-Miranda period, Seaside police were already giving warnings to suspects, as required by California's 1965 Dorado decision; [\[FN109\]](#) as a result, he argues, "Witt's 'before and after' Miranda figures do not capture any effect on suspects' willingness to confess caused by warnings." [\[FN110\]](#) Cassell's observation is correct, but the conclusion he draws (that "the Seaside City study . . . probably understates Miranda's effects" [\[FN111\]](#)) is not. The proper pre-Miranda baseline is one that approximates the legal regime that would exist in the absence of Miranda. Since Cassell's alternative to Miranda involves warnings much like those required by Dorado, [\[FN112\]](#) the *529 post-Dorado, pre-Miranda period Witt used is an ideal choice for the before-Miranda part of any comparison of Miranda to a regime with partial warnings but no right to cut off unwanted police questioning. Cassell's reservations about Seaside are thus unwarranted, and his ultimate decision to include it among the usable before-after comparisons is clearly correct. [\[FN113\]](#)

A close look at the Seaside data indicates, however, that the 2% drop reported in the study (and accepted by Cassell) is not quite the right number. Because this reported decrease was not statistically significant, it was possible that the actual confession rate did not change at all. [\[FN114\]](#) Moreover, Witt tabulated confessions as a percentage of all suspects incarcerated, but the figure we need is confessions as a percentage of suspects who were actually questioned. The two figures can differ significantly if police never try to interrogate some of their incarcerated suspects, and before-after comparisons can be misleading if there is a significant change in the number of arrestees whom police try to question. Witt's raw data reveal that four pre-Miranda arrestees and twenty post-Miranda detainees were never questioned at all. [\[FN115\]](#) Since there is no apparent reason why Miranda would prevent police from making an initial attempt to question a suspect, the proper measure of Miranda's impact is a before-after comparison of incriminating statements as a percentage of the cases in which police try to obtain one. Here Witt's raw data show confessions in 71% of the pre-Miranda interrogations and in 73% of the post-Miranda interrogations. [\[FN116\]](#) The post-Miranda period thus had a 2% increase in confessions rather than a 2% drop. But since neither the actual 2% increase nor Cassell's figure of a 2% drop is statistically significant, [\[FN117\]](#) the safest conclusion for Seaside is that there was no observable change in the confession rate following Miranda.

5. New Haven. -- The New Haven study reported a decline of 10 -15% in the confession rate from 1960 to 1966. [\[FN118\]](#) As Cassell notes, New Haven police were not complying with Miranda's requirements at the end point of the study, so the 10 -15% decline cannot be a measure of Miranda's impact. Cassell correctly excludes the New Haven study from his before-after tabulations. As discussed above, however, the 10 -15% decline observed in New Haven remains relevant in a different sense. If Miranda did not cause this decline, something else presumably did. New Haven provides a measure of the causal impact of 1960s developments that were independent of Miranda, and that causal impact (roughly a 2% decrease per year) must be deducted from any before-after change to arrive at the portion of that change attributable to Miranda.

[\[FN119\]](#)

Though New Haven is not a genuine before-after study, Cassell unpacks the raw data to discern a post- Miranda effect. Of eighty-one suspects whose conduct could be analyzed, warnings apparently influenced three suspects in refusing to confess, and ten confessions were obtained only after police violated Miranda by questioning suspects who had wanted to terminate the interview. [FN120] Thus, of eighty-one interrogations, Cassell argues that thirteen (16%) involved confessions lost because of Miranda. [FN121] The inclusion of the first three suspects (who clammed up after warnings) is questionable. We have no reason to assume that suspects who clam up after warnings would have confessed without warnings. In addition, Professor Cassell's alternative to Miranda also involves a detailed set of warnings, so the three suspects might well have chosen silence even under Cassell's conception of a "pre- Miranda" regime. In any event, we cannot be sure that New Haven police gave these suspects full Miranda warnings, and we know that they normally did not do so at the time. If New Haven police followed their normal practice with regard to these suspects, these were confessions lost under a voluntariness regime without systematic warnings. The proper measure of confessions lost in the immediate post- Miranda period is therefore ten out of eighty-one interrogations, or 12.3%.

6. Washington, D.C. -- The Washington, D.C. study showed a post- Miranda decline in the confession rate of only 1.5%. [FN122] But as

*531 Professor Cassell notes, the study's post- Miranda data come from a period before the D.C. police were fully implementing Miranda safeguards. [FN123] The 1.5% figure therefore cannot serve as a measure of Miranda's effect, and Professor Cassell correctly excludes it.

7. New Orleans. -- In a study of the period immediately following Miranda, the New Orleans Police Department determined that "988 out of 3506 [arrested] persons (28.2%)" made incriminating statements. [FN124] In contrast, for the two-year period prior to Miranda, "the Department estimated that self-incriminating statements were made in about 40% of the arrests." [FN125] Cassell compares the 40% pre- Miranda rate to the 28.2% post- Miranda rate and concludes that the confession rate dropped by 11.8%. [FN126] It should be evident, however, that valid before-after comparisons cannot be based on these data. The 40% figure is a rough, ballpark estimate, not a precise count of actual cases. If the actual pre- Miranda rate had been 45% or 35%, the magnitude of the before-after change would differ radically from the seemingly exact 11.8% figure Cassell uses. Precision is critical here, and we cannot base reliable estimates on numbers derived in this crude fashion. [FN127]

The New Orleans study must be excluded from the tabulation.

8. Kansas City. -- In a comment to the Wall Street Journal a few months after Miranda, Kansas City Police Chief (later FBI Director) Clarence Kelley stated that "only about 12% fewer suspects are giving statements since Miranda -- and this [loss of confessions] ' will probably decline as the case becomes more remote' in suspects' minds." [FN128]

*532 Cassell includes Kansas City in his tabulations, but many ambiguities impair the value of Kelley's comment as a reliable quantitative study. Cassell "[a]ssum[es] conservatively that Kelley was referring to all statements rather than incriminating statements," [FN129] applies a 50% discount for statements that were not incriminating. [FN130] and arrives at a 6% drop in the confession rate. If Kelley was referring only to incriminating statements, the drop in the confession rate could be much larger. Conversely (as Cassell acknowledges in a footnote), [FN131] Kelley's literal statement does not claim a drop of twelve points in the statement rate (e.g., from 60% to 48%) but only that "about 12% fewer suspects are giving statements."

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[FN132] As phrased, the 12% change could indicate a drop of only about 7.2 percentage points in the statement rate (e.g., from 60% to 52.8%). If that is what Kelley meant, the drop in the confession rate would be only 3.6%. A further problem is that Kelley's 12% figure, though it sounds like it must be an actual count, could be derived from comparing a count for one period with a rough estimate for the other; we know nothing about how Kelley's figure was derived. Finally, the gist of Kelley's statement, from a law enforcement official we might have expected to criticize Miranda, is that Miranda's overall impact was not substantial. Indeed, Kelley added -- in the portion of his statement italicized above -- that he expected the effects to dissipate over time. In effect, Kelley himself clearly regarded the 12% statement-rate change as overstating Miranda's impact.

Overall, the safer course would be to exclude the Kansas City data entirely, or to count them as indicating a much smaller confession-rate drop than the 6% figure Cassell uses. Within the limitations of Cassell's before-after framework, however, his decision to include Kansas City as a 6% drop is not implausible, and so I will defer to his treatment in my reanalysis of the data.

9. Kings County. -- At the 1967 Senate hearings, Kings County (Brooklyn) District Attorney Aaron Koota testified that before Miranda "approximately 10 percent" of interrogated suspects refused to make some sort of statement, but that after Miranda 41% refused to make any statement. [FN133] Cassell takes the drop in the rate of those who did give statements (from 90% to 59%), applies a 50% discount to exclude nonincriminating statements, [FN134] and concludes that the confession rate dropped from 45% to 29.5% -- a drop of 15.5%.

*533 Once again, Cassell's confession-rate change is calculated by comparing an actual count for the post- Miranda period to an interested observer's seat-of-the-pants estimate for the pre- Miranda period. We cannot hope to obtain sufficiently precise estimates from numbers derived in this fashion.

A further problem for Kings County is that even if we had reliable data to confirm a drop from 90% to 59% in the statement rate, we could not convert these figures to a drop from 45% to 29.5% in the confession rate, because we have no reason to assume that the proportion of nonincriminating statements remained constant. On the contrary, one effect of the Miranda warning of the right to remain silent was that many suspects who made flat denials pre-Miranda chose simply to remain silent post- Miranda. That result was precisely what the New York City police (who patrol both Brooklyn and Manhattan) experienced on the Manhattan side of the East River. [FN135] The proportion of statements that were incriminating therefore almost certainly rose after Miranda. If the proportion of statements that were incriminating changed by only 10% (from 40% pre- Miranda to 50% post- Miranda), then the drop from a 90% to a 59% statement rate would imply a drop from 36% (i.e., $.90 \times .40$) to 29.5% (i.e., $.59 \times .50$) in the confession rate, a confession-rate drop of only 6.5%. Of course, we do not know the exact extent of the change in the proportion of statements that were incriminating, but we cannot assume that the proportion remained constant, and -- as in the case of Philadelphia's pre-Escobedo data [FN136] -- any conclusions derived from the Kings County data are extremely sensitive to assumptions about what that change might have been. Because there is no reliable basis for inferring what (if any) confession-rate change occurred in Kings County, the study must be excluded.

10. Chicago. -- In an oral comment at a 1967 conference, Northwestern University Professor (later Governor) James Thompson stated that the number of confessions obtained from arrested Chicago homicide suspects had dropped "about 50%" [FN137] since Escobedo. [FN138] Cassell notes that very little is known about how the 50% figure was derived or what, exactly, it represents.

[FN139] He therefore correctly excludes the Chicago homicide data from his tabulations.

11. Los Angeles. -- A study by Los Angeles District Attorney Evelle Younger determined that pre- Miranda (in December 1965) *534 confessions or admissions were present in 40.4% of the requests for felony complaints received from the police. [FN140] In the immediate post- Miranda period the figure rose to 50.2% -- a confession-rate increase of 9.8%. [FN141]

Cassell believes that the Los Angeles data are flawed and should be excluded. His first concern is that the finding of a confession-rate increase is in itself a reason to doubt the study's validity: "The notion that Miranda would cause an immediate, substantial increase in the confession rate seems paradoxical" [FN142] But as we have seen, [FN143] the Miranda safeguards have mixed and partially offsetting effects; they undoubtedly squelch some confessions, and they help in eliciting others. An objective inquiry cannot rule out from the start the possibility that Miranda's net effect could be in the direction of a confession-rate increase. More basically, Cassell's assumption that any before-after differences must be due to Miranda (or to flaws in the study) reflects the causal confusion that pervades his analysis. A post- Miranda increase in a well-designed study does not indicate that Miranda caused that effect, and for the same reason, we cannot take a post-Miranda drop in the confession rate as proving the opposite. Before-after differences may be attributable to other causes, long-term trends, or random fluctuation. [FN144] By averaging a number of discrete observations, we can attempt to reduce the impact of such problems, but to do so successfully we must include all the usable observations, unexpected or not.

Cassell also considers the Los Angeles results unsatisfactory because the pre- Miranda data come from a period after the California Supreme Court's Dorado decision, [FN145] which anticipated Miranda by requiring that suspects be advised of their right to counsel. [FN146] Cassell argues that the Los Angeles study is therefore really an "after-and-after" study. [FN147] This objection would be well-founded in a purely historical comparison of Miranda to the 1960s voluntariness test; for that purpose the Los Angeles data should be discarded. But Cassell's objection is relevant in a policy analysis only if California's post- Dorado, pre- Miranda regime was significantly more restrictive than the one that would exist in the absence of Miranda. Dorado did not anticipate the portions of Miranda that Cassell wants to rescind (the waiver requirements and interrogation cut-off rules), and the warnings that Dorado *535 required were much like those that Cassell himself thinks should be imposed as an alternative to Miranda. [FN148] Thus, California's legal regime in the post- Dorado, pre- Miranda period closely approximates the one that would exist (and that Cassell himself advocates) in the absence of Miranda. As such, it serves as a nearly perfect baseline for any before-after test of Miranda's impact.

Cassell's other major objection is based on his belief that the D.A.'s office "apparently redesigned" [FN149] the study questionnaire after the December 1965 survey period, and "changed from asking about 'confessions and admissions' to asking about a 'confession, admission or other statement.'" [FN150] Cassell argues that because many suspects make "other statements," the expanded category probably swelled the number of statements recorded for the post- Miranda period. [FN151] The claim seems plausible until we take a close look at the sources for Cassell's argument. The category for "confession, admission or other statement" was taken from the actual survey questionnaire for the June 1966 study, [FN152] but the category for "confessions and admissions" (which Cassell assumes to represent something narrower) was not taken from the December 1965 questionnaires but only from the "summary sheets" used by the law clerk who subsequently tabulated these questionnaires. [FN153] There is no indication that this shorthand designation would have led the clerk to exclude incriminating statements that were technically not admissions. Nor is there any indication that the questionnaire category for the kind of statements to be reported was rephrased at all. Indeed, the categories for "admission" and "admission or other statement" are simply treated as synonymous throughout the Los Angeles material. [FN154]

*536 Moreover, even if the category for confessions and admissions was worded somewhat differently in the two sets of

questionnaires, Cassell's claim -- that the final questionnaire would have tallied non incriminating statements -- remains untenable. The Los Angeles survey did not track all statements made in interrogations but only those statements attached to requests for issuance of a complaint. [FN155] The category for "admissions" attached to complaint requests and the category for "admissions or other statements" attached to complaint requests are substantially equivalent in this context. Both are designed to capture incriminating statements that did not rise to the level of a full confession; neither category would include nonincriminating statements, because these would not be attached to requests for issuance of a complaint. [FN156] Younger himself viewed the categories in precisely these terms and expressly rejected the interpretation (now advanced by Cassell) that the category for "confessions, admissions or other statements" could have swept in non incriminating statements. When Younger reported the statement rate for the broadly worded category used in the final study, he treated that category as synonymous with confessions or admissions, and stated simply that "[c]onfessions and admissions were present in 50% of the requests for felony complaints" [FN157] Against the background of Cassell's insistence on a literal reading of every word in Hogan's summary of the New York data, [FN158] his willingness to reject Younger's interpretation of the Los Angeles results seems particularly jarring and partisan. [FN159]

*537 We must remember, in choosing between Younger's interpretation and Cassell's, that Younger had no reason to understate Miranda's effect. On the contrary, Younger vigorously opposed Miranda and urged the Supreme Court to overturn it; [FN160] other District Attorneys "plead[ed] with him" to re-examine his study and to withdraw its inconvenient finding that harmful effects had not occurred. [FN161] If a flaw like the one Cassell sees in the study had actually existed, it is hard to believe that first-hand participants with every reason to report the difficulty would have remained silent.

In sum, there is simply no basis for suspecting that there was any significant difference in the way the "before" and "after" Los Angeles surveys were compiled. Cassell's scruples about this minute detail seem especially strained in light of his willingness to include Philadelphia, New Orleans, and Kings County comparisons in which the methods used to compile the data were entirely different in the before and after periods, with only impressionistic ballpark estimates generating the data for the "before" side of the comparison. [FN162]

Cassell also objects that "[b]ecause the Miranda survey was conducted only a few weeks after the decision, some of the post-Miranda data actually may have been pre- Miranda data." [FN163] The partisan character of Cassell's method again becomes transparent here, since he gave no weight to this very concern in connection with the New York grand jury data, where the problem clearly affected a substantial portion of the cases. In the Los Angeles data, in contrast, Younger's report explicitly refutes Cassell's speculation: after June 21, virtually all the cases at the complaint stage involved post- Miranda interrogations, and the implementation of Miranda in these cases was widespread. Of 721 cases in which incriminating statements were obtained, Miranda warnings had been given in 668 cases (93% of the total); in 14 of the remaining cases, Miranda warnings were not required because the *538 suspects who gave statements were not in custody, and in another 25 cases, Miranda was inapplicable for other reasons. Altogether only 14 cases (2% of the total) involved a failure to comply with Miranda. [FN164]

All told, the Los Angeles study is one of the least vulnerable of the eleven surveyed here. It certainly deserves to be included in any tabulation of usable before-after comparisons.

12. Summary of the Site-Specific Adjustments. -- My reanalysis of the data leads me to accept Professor Cassell's treatment of three of the studies, and in four other cases, I agree that the studies are usable but suggest adjustments to the magnitude of the post- Miranda effect. For three of the studies that Professor Cassell includes (New York, Kings County, and New

Orleans) there are, in my view, decisive reasons to exclude them altogether. Conversely, one of the studies he excludes (Los Angeles) clearly deserves to be included. Table 1 below summarizes the results of this reanalysis, which modifies the average before-after change from a 16.1% drop (Professor Cassell's figure) to a confession-rate drop of only 9.7% in comparison to the 1960s voluntariness test and a drop of only 6.4% in comparison to a regime that warns arrested suspects of their rights.

***539** Table 1 Confession-Rate Changes in Before-After Studies

	Change in Rate Used by Cassell	Corrected Comparison to Regime Without Warnings	Corrected Comparison to Regime With Some Warnings**
Pittsburgh	-18.6%	-16.2%	-16.2%
New York County	-34.5%	excluded	excluded
Philadelphia	-24.6%	-13.8%*	-13.8%
Seaside City	-2.0%	+0.0%*	+0.0%
New Haven	-16.0%	-12.3%	-12.3%
Washington, D.C.	excluded	excluded	excluded
New Orleans	-11.8%	excluded	excluded
Kansas City	-6.0%	-6.0%	-6.0%
Kings County	-15.5%	excluded	excluded
Chicago	excluded	excluded	excluded
Los Angeles	excluded	excluded	+9.8%
Average Change	-16.1%	-9.7%	-6.4%

* As explained above, see supra text accompanying notes 103-05, 113, the Philadelphia and Seaside data may somewhat understate the difference between Miranda and a regime without any warnings, because partial warnings inspired by Escobedo were given during the 'before' period in both cities. But the

extent of this problem appears to be quite limited; as Cassell acknowledges, there was a 'clear consensus' among prosecutors that 'confession rates remained constant [in 1965-66] even in those states where Escobedo had been extended to require the police to warn suspects of their rights.' Cassell, *supra* note 5, at 493. Accordingly, in the comparison of Miranda to a regime with no warnings, the Seaside and Philadelphia figures can be included as useful (though perhaps slightly low) estimates. Cassell likewise includes Seaside in his tabulations. See *supra* note 113 and accompanying text.

** To the extent that figures in this column were derived from studies in which the pre- Miranda baseline did not require warnings, the changes reported could overstate Miranda's negative impact in comparison to a regime with some warnings.

D. Other Adjustments

In addition to adjustments specific to studies at particular sites, several other corrections must be considered. This subpart focuses on three particularly important concerns: Miranda's initial negative effects may have been concentrated in the larger cities, confession rates were declining in the 1960s for reasons independent of Miranda, and declining confession rates overstate law enforcement damage because confessions are not always necessary for conviction.

1. Large City Effects. -- Cassell points to a number of factors suggesting that " Miranda has a larger effect on major urban areas." [FN165] Although the data are not strong enough to permit confident conclusions, some of the evidence supports his view that in before-after comparisons *540 from the 1960s, " Miranda has differential effects on the confession rate in cities of varying sizes." [FN166] For cities over 250,000 population, the average confession-rate drop was 18.5% in the studies Cassell uses, and 6.6% in my reanalysis; in contrast, the two smaller cities (Seaside and New Haven) had a confession-rate drop of only 9% in Cassell's tabulation and 6.2% in my reanalysis. [FN167]

Cassell acknowledges that overrepresentation of large urban areas among his studies presents a possible problem, but he justifies the tilt toward large cities in his tabulation on two grounds. First, large cities, with 19% of our population but 32% of our index crimes, [FN168] suffer a disproportionate share of our nation's crime problem. Second, the inclusion of one smaller jurisdiction, Seaside City, partially redresses the balance. But since Seaside City represents only one of the eight studies Cassell uses (and only one of six in the reanalysis), a simple unweighted average cannot be used to estimate Miranda's aggregate nationwide impact. In Cassell's sample, the six large cities had a confession-rate drop of 18.5%, and the two

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smaller jurisdictions had a confession-rate drop of 9%, so in the eight studies Cassell uses, Miranda's average national impact, weighted to reflect the distribution of serious crime $[(18.5 \times .32) + (9.0 \times .68)]$, was not the 16.1% figure Cassell reports but only 12%. Similarly, after the site-specific adjustments, the large cities had a confession-rate drop of 12% and the smaller jurisdictions had a confession-rate drop of 6.2% in comparison with the 1960s voluntariness test, so the weighted average $[(6.6 \times .32) + (6.2 \times .68)]$ was a confession-rate drop of only 8%. For the comparison of Miranda to a regime requiring warnings, the confession-rate drop changes from 6.4% to a weighted average of 6.3%.

2. Trends and Other Causes. -- During the early- and mid-1960s, many developments independent of Miranda contributed to a declining trend in the confession rate. [\[FN169\]](#) Though we cannot quantify those effects with certainty, the trend was clearly present, and the available evidence suggests that a reasonable approximation would be a confession-rate drop -- independent of Miranda -- on the order of 2% per year during this period. If we had a well-designed before-after study showing that the confession rate was 10% lower in January 1967 than it had been in January 1966, we would need to subtract the 2% trend factor to arrive at the part of the change (8%) that might be attributable to Miranda.

To make the proper trend adjustment, we need to know the time between the midpoints of the before and after periods, a detail that is ***541** not clear in some of the studies. As best I can reconstruct, this time interval averaged about sixteen months (i.e., a 2.7% trend-related drop) for the eight studies Cassell uses [\[FN170\]](#) and about thirteen months (i.e., a 2.2% trend-related drop) for the six studies in the reanalysis. [\[FN171\]](#) The average confession-rate drop, in comparison to a regime requiring some warnings, thus declines from 6.4% in the site-specific reanalysis to 6.3% after adjusting for large city effects and to 4.1% after adjusting for trends not related to Miranda. The final figure -- 4.1% -- provides a measure of Miranda's empirically detectable effect on the confession rate in the immediate post-Miranda period.

3. The Need for Confessions. -- We are not yet in a position to determine Miranda's measurable impact on law enforcement. A confession is not always necessary for conviction, and thus a decline in the confession rate will not inevitably produce a decline in the conviction rate. Drawing on the various before-after studies and other empirical sources, Cassell estimates that confessions are needed for conviction in 24% of all cases. He thus calculates that his estimated 16.1% decline in the confession rate will lead to a loss of convictions in 3.8% of criminal cases. If his necessity figure is correct, but we apply it instead to the confession-rate changes demonstrated by this Article's reanalysis of the data (4.1% after adjustments), Miranda's impact on the conviction rate falls to 0.98% $(4.1 \times .24)$, i.e., less than one percent.

The estimates of necessity are almost invariably subjective. It is difficult to know whether the researchers Cassell cites made accurate judgments about the probability of conviction on the evidence at hand, much less about the possibilities of augmenting that evidence by other methods of investigation. Nonetheless, taking at face value the methods Cassell uses to estimate necessity, doubts remain about which studies are sufficiently sound to warrant consideration. My review of these data, though not worth extended discussion here, indicates that the best measure of necessity rates derivable from the empirical studies is not 24% (Cassell's figure) but 19%. [\[FN172\]](#) If this is ***542** the better estimate, then Miranda's measurable impact on the conviction rate falls to 0.78% $(4.1 \times .19)$, i.e., seventy-eight hundredths of one percent.

Cassell argues that the costs of Miranda include not only cases in which a confession is "necessary for conviction" but also

cases in which lack of a confession will mean a less favorable plea bargain. [FN173] His point would be well-taken if the first group of cases (those in which a confession is "necessary for conviction") is narrowly defined to include only cases in which, absent a confession, a conviction is unattainable in any manner. The necessity judgments in the empirical studies were not made in this manner, however. On the contrary, a confession was counted as necessary any time that the other evidence was insufficient to make conviction likely at trial. [FN174] With this trial-oriented focus, cases were counted as lost not only when the lack of a *543 confession would produce a dismissal or acquittal at trial, but also when the prosecutor, facing the risk of defeat at trial, would have to negotiate a reduced sentence in order to get a guilty plea.

A "necessity" figure defined in this fashion already includes sentencing impacts, and in fact it inevitably overstates Miranda's impact on the conviction rate. Plea bargaining dynamics tend to offset the numerically estimated loss of convictions (whether 24% or 19%), because prosecutors can use many sorts of leverage to obtain guilty pleas in some cases counted as "lost," i.e., cases in which a confession would be necessary to make conviction likely at trial. The plea-bargained sentence may be lower in such a case than it would have been with a confession, but the case will not be lost altogether (as the empirically derived necessity figure implies). Serious offenders would be unlikely to "walk." [FN175] And the sentence might not be lower at all. Bargains for a specific sentence are rare in the federal courts and in many state systems; [FN176] absent such bargain, the judge will be free to fit the sentence to her own conception of the "real" offense, and the defendant may get little credit for evidentiary weaknesses or even for a negotiated charge reduction. [FN177]

The justice system has other ameliorative mechanisms to deploy as well. If the defendant cannot be induced to plead guilty to the current charges, he may be convicted for something else, immediately or soon after. The new charges may be less serious, but once the defendant stands convicted of anything, the judge can consider the prior conduct (*544 and under the Federal Sentencing Guidelines, the judge is sometimes required to consider the prior conduct [FN178]) in imposing sentence for the current offense. A confession may have been necessary to convince a unanimous jury beyond a reasonable doubt to convict at trial, but in a sentencing proceeding the prior conduct need be established only to the judge's satisfaction by a preponderance of the broadly admissible evidence (including hearsay). [FN179] As several recent cases illustrate, defendants charged with racketeering, mail fraud, and the like may draw sentences of life imprisonment because the judge is persuaded (despite their refusal to confess) that they are guilty of murder. [FN180] The Federal Sentencing Guidelines cap this conglomeration of devices ameliorating Miranda by providing that defendants who display no "acceptance of responsibility" (by remaining silent and refusing to plead guilty, for example) actually receive substantially higher sentences than defendants who contritely or wisely behave otherwise. [FN181]

With no way to quantify the effect of these ameliorative mechanisms, we can perhaps accept the empirically estimated necessity rate (19%, after adjustments) as a rough working measure. But such a figure, derived from judgments about the likelihood of conviction at trial, significantly overstates the law enforcement damage that failure to obtain a confession will cause. The 19% necessity figure, applied to the best estimate of the confession-rate drop due to Miranda (4.1%) indicates that, in the immediate post-Miranda period, Miranda may have had a harmful impact on law enforcement in at most 0.78% of serious criminal cases.

E. Summary of the Empirical Data

Professor Cassell concludes, in light of the before-after studies, that in the immediate post-Miranda period, Miranda caused a 16.1% drop in the confession rate, that confessions were necessary for conviction in 24% of the cases, and that Miranda accordingly caused a loss of convictions in 3.8% of serious cases. Even if we accept these conclusions at face value, Miranda's estimated harm to law enforcement *545 in the immediate post-Miranda period affected only a small minority of the cases.

With necessary adjustments, however, the losses estimated by Cassell disappear almost completely. Table 2 below summarizes the essential adjustments described in this Article. The estimated confession-rate change drops to 5.8% in comparison to the 1960s voluntariness test, and drops from 16% (Cassell's figure) to 4.1% in comparison to a regime with some warnings. Assuming (generously) that the absence of a confession meant a lost conviction in 19% of these cases, Miranda can be held responsible for harm to law enforcement in at most 0.78%, i.e., seventy-eight hundredths of one percent of the cases in the immediate post- Miranda period.

Table 2 Quantitative Estimates of Miranda 's Impact on Law Enforcement

	Cassell Study	Corrected Comparison to Regime Without Warnings	Corrected Comparison to Regime With Some Warnings
Change in Confession Rates (avg. of usable studies)	-16.1%	-9.6%	-6.4%
Change in Confession Rates B@@(avg. weighted for large city effects)	-12.0%	-8.0%	-6.3%
Adjustment for Trends and Other Causes	+2.7%	+2.2%	+2.2%
Adjusted Change in Confession Rates	-9.3%	-5.8%	-4.1%
Confession Necessity Rate	24.0%	19.0%	19.0%
Lost Convictions Due to Miranda	-2.2%	-1.1%	-0.78%

In a large country with a high crime rate, a 0.78% attrition figure represents a large number of cases. Cassell wants to argue that his 3.8% estimate of Miranda 's impact (and even my 0.78% estimate) imply a shocking and unacceptable loss of cases. Applying his 3.8% figure to FBI arrest statistics for 1993, he calculates that Miranda causes the loss of 28,000 violent crime convictions per year. [\[FN182\]](#) Even if the correct figure is only 0.78%, Cassell would argue that the adjusted *546 attrition figure -- 4700 violent crime convictions per year -- is still a serious cause for concern.

This sort of calculation presents a thoroughly misleading picture, however. Impact estimates in social science are notoriously subject to the fallacy of false precision. In rigorous experimental science, a 0.78% effect (implying, for example, the saving of 0.78% of affected lives in a cancer treatment) might be an important result. In contrast, crude before-after studies in social

science generate "soft" estimates with a wide range of error. Under these circumstances, proportions are revealing, but absolute numbers can create a deceptive illusion of large benefits. Social science researchers cannot responsibly promise to produce 4700 additional convictions with no risk of disrupting the remaining 749,300 cases.

Cassell himself is implicitly aware of these problems, because his own "compromise" -- some warnings but no waiver and cut-off rules [FN183] -- generates fewer convictions than would a regime of no warnings at all. Cassell repeatedly asserts that the number of convictions lost because of his proposed warnings would be small. [FN184] And undoubtedly he is right -- since what he means is that the losses would be relatively small. [FN185] But how many cases would be affected? Based on the kind of before-after studies that Cassell considers reliable, Miranda produces an attrition rate of 0.78% compared to a regime with some warnings, and an attrition rate of 1.1% compared to a regime with no warnings. A regime like Cassell's (with some warnings) would thus lead to roughly 0.32% more attrition than a regime of no warnings at all. The 0.32% figure is a very low rate of attrition, but if Cassell is serious about his method of extrapolation, he must apply this figure to the FBI's estimate of arrests for violent crime, concluding that his recommendations would lead to the release of 1930 violent criminals per year. [FN186] What would Cassell say to the families of all these victims, whose attackers would be released by his proposal? Cassell knows that his sort of extrapolation is simply rhetoric, not a serious foundation for assessing social policy.

Reinforcing these reservations, an extrapolation from the old before-after studies is especially vulnerable, because even the adjusted 0.78% estimate of Miranda 's impact still substantially overstates Miranda 's current effect. One major source of overestimation is organizational failure and general chaos in the criminal justice system. The resulting attrition dwarfs the 0.78% (or 3.8%) figure. To be sure, *547 weak evidence often causes such attrition, and more confessions would therefore help reduce it. But much of the attrition is unrelated to the strength of the evidence. Cassell's own empirical research indicates, for example, that "questioning [was] successful" in 20% of the cases that prosecutors chose to drop. [FN187] To focus on a 0.78% estimate as a concrete, quantifiable gain -- in the porous, rough-and-tumble world of criminal justice -- is simply unreal.

There are at least three additional sources of overestimation in the adjusted 0.78% attrition rate. The first is that several of the police departments included in Cassell's tabulations (and mine) gave no warnings prior to Miranda, or they used other standards lower than those that would be required in a non- Miranda regime today. [FN188] The before-after comparisons used as a basis for the 0.78% estimate therefore exaggerate the difference between Miranda and any replacement regime that would be implemented in the 1990s. Second, plea bargaining and "real offense" sentencing often produce convictions and sentence enhancements without a confession, even when a confession must be counted as "necessary" because it is needed to win a guilty verdict at trial. [FN189] Third, and most important, the great weight of the evidence confirms that police have now adjusted to the Miranda requirements and overcome the limited difficulties experienced in the immediate post-Miranda period. [FN190] Altogether, a realistic working estimate of Miranda 's impact on current law enforcement must be placed far lower than the 0.78% figure. For practical purposes, Miranda 's demonstrable impact on conviction rates today is virtually nil.

To be sure, there could be harmful net effects that did not show up in the studies under consideration. But Professor Cassell wants to use the empirical studies affirmatively, to refute the pervasively held view that Miranda has not caused significant harm. Nothing in the studies comes close to carrying that kind of burden. Indeed, the end result of the empirical exercise, with its vanishingly small evidence of harmful effects, serves only to reinforce the conventional view. If a harmful net impact exists, we will have to build the equivalent of the Superconducting Supercollider in order to find it. For all practical purposes, Miranda's empirically detectable net damage to law enforcement is zero.

*548 II. Alternatives to Miranda

Professor Cassell would replace the Miranda safeguards with a new system of rules for police interrogation. He would

require videotaping of interrogation sessions and five explicit warnings, including a warning of the right to remain silent and a warning of the right to counsel once the suspect is brought before a judge. [FN191] This approach would add a new safeguard (videotaping) to those Miranda now requires but would cut back on Miranda in three respects: suspects would no longer have (and thus would not be told that they have) the right to counsel during pre-arraignment interrogation; interrogation could proceed whether or not police obtained an affirmative waiver of the right to remain silent; and police would no longer be required to stop interrogating suspects who tried to end the interview or asked for a lawyer's help. In effect, Cassell would replace the Miranda approach with a system explicitly authorizing the involuntary custodial interrogation of unwilling suspects. He is tellingly vague about how far police could go, and for how long, in persistently questioning the unwilling suspect; [FN192] presumably the only limitation would be the prohibition against "break[ing] the suspect's will." [FN193]

Professor Cassell defends this reshaping of interrogation doctrine in terms that resemble a straightforward political horsetrade: he would excise the parts of Miranda he considers most harmful and compensate Miranda's defenders with a new safeguard that he thinks society can more easily afford. This is a natural posture for a critic who rejects Miranda's premises altogether. But Miranda's defenders (and the courts) are not free to "deal" in this fashion, because the Miranda decision proceeds from a conception of Fifth Amendment principles that the interrogation rules were designed to protect. Any proposal for modifying the Miranda safeguards must effectively further the same underlying principles (or explain why they should be abrogated). Before assessing the pragmatic pro's and con's of the trade Cassell proposes, we must first be clear about the constitutional principles that frame the inquiry. I discuss first, in subpart A, the criteria that determine when a criminal suspect's statement is "compelled" within the meaning of the Fifth Amendment. In subpart B, I consider whether the existing Miranda safeguards can be dismissed as "merely prophylactic," or whether they are instead constitutionally required to prevent Fifth Amendment compulsion; subpart B makes explicit what I consider to be the uncontroversial prerequisites for any constitutionally acceptable set of interrogation safeguards. Subpart C *549 then evaluates the specifics of the Cassell proposals, and subpart D briefly considers other possible alternatives to the Miranda approach.

A. Fifth Amendment Limitations: What is Compulsion?

The only normative Fifth Amendment theory that underlies Professor Cassell's proposed "deal" is his apparent assumption that police pressure designed to elicit a confession never violates the Constitution unless it is so overbearing and coercive that it breaks the suspect's will. Throughout his article, Cassell equates "unconstitutional police coercion" [FN194] with "rubber hoses and the like." [FN195] He argues that "genuinely coerced confessions were, statistically speaking, rare at the time of Miranda" [FN196] and supports that claim by references designed to show that outright physical brutality and equivalent psychological coercion were unusual by the mid-1960s. [FN197] In a similar vein, arguing that Miranda was not necessary to prevent the only kind of police tactics that (in his view) "genuinely" violate the Fifth Amendment, Cassell refers to "international empirical confirmation that high confession rates are obtainable without coercion"; [FN198] his evidence includes one observer's 1959 report that even "the [Soviet] KGB looks upon direct physical brutality as an ineffective method of obtaining the compliance of the prisoner." [FN199]

Cassell's double standard becomes evident again in his handling of the brutality problem. He treats lost convictions as important, even though they are statistically infrequent, because (if his data analysis is correct) they represent a large absolute number of cases. [FN200] Yet he dismisses the police brutality problem because of its "statistical scarcity," [FN201] and "relative infrequency," [FN202] without acknowledging the large absolute number of cases represented by a mere 1% or 2% of all arrests.

If Cassell is right about the kind of coercion that is necessary to make out a "genuine" violation of the Fifth Amendment, Miranda's critics nonetheless have a point. The Miranda safeguards, though they do help prevent outright physical and

psychological brutality, [\[FN203\]](#) might not seem essential for that purpose alone.

***550** But if Cassell is right, the Court not only did something strange in *Miranda*; it has been doing stranger things before and since. Prior to *Miranda*, the Court held in *Griffin v. California* [\[FN204\]](#) that a comment on a defendant's silence at trial violates his Fifth Amendment privilege by creating pressure for him to testify, and this holding has been repeatedly reaffirmed and expanded by lopsided majorities in the Burger and Rehnquist eras. [\[FN205\]](#) Does this kind of comment on silence (or the milder whip of a judge's refusal to instruct the jury not to infer guilt from silence [\[FN206\]](#)) amount to coercion on the order of "the rubber hose and the like"? Stranger still, in *Lefkowitz v. Cunningham*, [\[FN207\]](#) a seven-to-one majority of the Court, in an opinion by Chief Justice Burger, held that the state cannot fire a public employee for claiming his privilege to remain silent, even if the employee has no legally enforceable right to the job. Does the threat to take away "potential economic benefits realistically likely of attainment" [\[FN208\]](#) amount to coercion on the order of "the rubber hose and the like"? If Cassell is correct about what the "real" Fifth Amendment means, large bodies of noninterrogation case law become incomprehensible; everywhere we begin to bump into wildly overbroad notions of involuntariness, or what could only be a proliferation of prophylactic rules run amok.

There are other puzzles as well. Suspects under interrogation are routinely asked to "waive" their Fifth Amendment privilege and many do so. Trial witnesses who begin testifying without asserting the privilege are normally deemed to have waived it. Have these individuals waived their protections against torture or brutal psychological coercion? Are they now fair targets for "the rubber hose and the like"? It would be absurd to say that because of waiver, such individuals become subject to the kind of coercion that violates the due-process voluntariness test. Yet it is equally absurd to say that they haven't (or can't) waive their privilege against compulsory self-incrimination. Such individuals are subject to the kind of compelling pressures (extended compulsory questioning and cross-examination, comments on silence, contempt citations) that would undoubtedly violate the Fifth Amendment in the absence of waiver.

The faulty premise here, obviously, is Cassell's assumption that a person is "genuinely" compelled only when he is coerced by the equivalent of rubber-hose tactics or by the kind of direct physical brutality that even Soviet interrogators disdained in the 1950s. Police do ***551** not have to violate KGB standards in order to violate the Fifth Amendment. The English language explains some of the confusion here, because terms like coercion and compulsion have virtually interchangeable meanings; Supreme Court opinions have sometimes aggravated the problem by using the terms interchangeably. [\[FN209\]](#) But whatever one may think of *Miranda*, it is clear -- and uncontroversial -- that pressure need not rise to the level of overbearing physical or psychological coercion, in the due process sense, before it is sufficiently compelling to violate the Fifth Amendment. Outside the context of police interrogation, the law has long been settled, before *Miranda* and since, that the Fifth Amendment is violated by any pressure or penalty deliberately imposed for the purpose of getting a criminal suspect to speak. [\[FN210\]](#) As the Court stressed in *Bram v. United States*, [\[FN211\]](#) in determining compulsion under the Fifth Amendment, "the law cannot measure the force of the influence used or decide its effect upon the mind of the prisoner, and, therefore, excludes the declaration if any degree of influence has been exerted." [\[FN212\]](#) The opposing view, pressed so hard by *Miranda*'s critics -- that Fifth Amendment compulsion and due-process coercion are identical concepts [\[FN213\]](#) -- would, if taken seriously and applied to contexts other than police interrogation, make shreds of the entire fabric of Fifth Amendment doctrine and tradition.

Miranda's innovation was to hold that police interrogation could no longer be treated as a world apart. Prior to *Miranda*, the courts had uniformly held that police interrogation, because it imposed no formal penalty for silence, was immune from the Fifth Amendment ***552** limitations that apply in every other context. [\[FN214\]](#) Because there was no formal legal obligation to speak, and thus no duty against which a formal privilege of silence could be applied, there simply was no privilege for the arrested suspect to waive; interrogation was thus restricted only by the due process anticoercion principle that protects all

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individuals even after they waive the protections of the Fifth Amendment. Miranda, in a radical break with prior precedent, rejected that view. The Court's central holding was not the now-famous warnings, but the principle that Fifth Amendment standards would henceforth apply. [\[FN215\]](#) And the two premises cited to support that conclusion, though hotly contested then, are surely uncontroversial now: that a formalistic showing of compulsion by legal process or official punishment cannot be essential, and that from every practical vantage point, once a suspect is isolated in police custody and deprived of his freedom to leave, interrogation involves pressures that can dwarf those that were decisive in cases like Griffin and Lefkowitz. [\[FN216\]](#) The pressures are normally compelling in this practical sense, as even Miranda's most committed critics now acknowledge. [\[FN217\]](#) Indeed, as the Miranda Court noted, and as Miranda's critics concede, [\[FN218\]](#) the "interrogation environment is created for no purpose other than to subjugate the individual to the will *553 of his examiner." [\[FN219\]](#) As a result, the typical custodial police interrogation, even if not brutally coercive in the due process sense, will readily (perhaps almost invariably) violate the Fifth Amendment bar on the use of compelling pressure, at least in the absence of safeguards sufficient to dispel that pressure.

B. Are Miranda's Requirements "Merely Prophylactic"?

If the foregoing conception of Fifth Amendment compulsion is sound and, as I have suggested, uncontroversial, what are we to make of the Court's statements, in cases like *Michigan v. Tucker*, [\[FN220\]](#) that the Miranda rules are only "suggested safeguards" [\[FN221\]](#) and that they are "not themselves rights protected by the Constitution"? [\[FN222\]](#) Seizing on this language, Professor Cassell argues that "[t]o obtain a confession in violation of Miranda is not necessarily -- or even usually -- to obtain a coerced confession in violation of the Fifth Amendment." [\[FN223\]](#)

If the Court's language in cases like *Tucker* really means what Cassell suggests, the path is clear to jettison the Miranda safeguards without violating the Fifth Amendment itself. But if Miranda's safeguards are not constitutionally required, how are we to explain such post-*Tucker* cases as *Edwards*, [\[FN224\]](#) *Minnick*, [\[FN225\]](#) and *Roberson*, [\[FN226\]](#) where the Court reversed state criminal convictions on Miranda grounds, without finding (and without having any conceivable basis for finding) "coercion" in the sense that Cassell considers necessary for a "genuine" constitutional violation. [\[FN227\]](#) If the passage in *Tucker* has the literal meaning Cassell would give it, the Court's actual behavior is mysterious. Did a conservative Court, after deciding that Miranda's "codelike" rules [\[FN228\]](#) go too far and are not constitutionally imperative, then claim the power to reverse state criminal convictions in the absence of any constitutional violation? A reading of judicial dicta that has the Court simultaneously criticizing Miranda for its lack of judicial restraint, and then repeatedly exercising an unprecedented power to overturn state proceedings that fully complied with the Constitution, is surely paradoxical.

To read comments about Miranda's "prophylactic" character as implying that the Miranda safeguards can be ignored -- without violating *554 the "real" Fifth Amendment -- is to play with words out of context and to disregard what the Court has been doing. What the *Tucker* passages and similar dicta mean to emphasize is not that the Miranda requirements can be altered or abandoned at will, but only that the specifics of the Miranda approach can be modified or replaced if a state provides equivalent protection to the suspect. Absent an alternative, equally effective system, the Miranda safeguards are constitutionally required to prevent compelling interrogation pressures that violate the Fifth Amendment. As the Court recently stated in *Withrow v. Williams*, "the Constitution requires no 'particular solution for the inherent compulsions of the interrogation process,' and [Miranda] left it open to a State to meet its burden by adopting 'other procedures . . . at least as effective in apprising accused persons' of their rights." [\[FN229\]](#) And the Court in *Withrow* explicitly disavowed the claim that Miranda's "prophylactic" nature detracts from its constitutional importance. Reaffirming that Miranda claims (unlike Mapp search and seizure claims) remain available in habeas corpus proceedings, the Court explained: [\[FN230\]](#)

[W]e have sometimes called the Miranda safeguards "prophylactic" in nature. [[[citations omitted]]] Calling the Miranda safeguards "prophylactic," however, is a far cry from putting Miranda on all fours with *Mapp* [T]he *Mapp* rule "is not a

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personal constitutional right," but serves to deter future constitutional violations Miranda differs from Mapp "Prophylactic" though it may be, in protecting a defendant's Fifth Amendment privilege against self-incrimination Miranda safeguards "a fundamental trial right."

This constitutional grounding of the "prophylactic" rules makes clear that in order to assess any proposed alternative, we must focus not just on its possible law enforcement advantages but on its effectiveness in dispelling the inherently compelling, constitutionally impermissible pressures inherent in custodial questioning. Any acceptable substitute for Miranda must also serve two related purposes: protecting suspects from brutality and providing clear guidance to the police.

First, arrested suspects must have protection from the risk of brutal abuse in the interrogation process. Miranda's critics often suggest that the Miranda safeguards are not needed to serve this goal, [\[FN231\]](#) but nothing could be further from the truth. In theory, brutality is ruled out by the due process test, independent of Miranda, but in operation the due process test sends police a fatally mixed message. The job of *555 the interrogator, of course, is to get the reticent suspect to "come clean." When a tired, confused, or shaky prisoner shows signs of starting to "crack," is the interrogator supposed to keep up the pressure, or back off to avoid breaking the suspect's will? In effect, under the due process test, the officer is expected to do both. Instances of overbearing coercion are bound to occur under such a system, not because some officers will deliberately flout the law but because even the best of professionals will inevitably misjudge the elusive psychological line.

Under the due process approach, moreover, egregious physical coercion is an ever-present risk as well. Conscientious investigators, intent on solving brutal crimes, are not always capable of superhuman restraint; when confronted with a suspect who obstinately lies or refuses to cooperate, they sometimes lose their tempers. Cases of physical abuse, though certainly the exception, could and did occur. [\[FN232\]](#) Miranda itself is not a perfect cure for the risk of physical abuse (no legal rule could be), but it works a crucial change in dynamics by insisting that interrogation cease whenever the suspect clearly expresses the desire to remain silent. The law no longer formally ratifies the volatile and dangerous system in which reticent suspects are required to face a determined interrogator who is authorized to deploy some degree of pressure to get them to talk.

The second concern is related: officers must have clear guidance as to what they may and may not do. Without clear ground rules, suspects are at risk because good faith mistakes inevitably will be made. Without clear rules, law enforcement suffers because officers cannot be sure of the steps needed to ensure admissibility. As Chief Justice Rehnquist has stressed, Miranda's "core virtue" was "afford[ing] police and courts clear guidance on the manner in which to conduct a custodial investigation," [\[FN233\]](#) and Miranda thus "help [[s] police officers conduct interrogations without facing a continued risk that valuable evidence [will] be lost." [\[FN234\]](#) In *Withrow*, the Court cited these concerns as crucial elements in its refusal to "abdicat[e] Miranda's bright-line . . . rules in favor of an exhaustive totality-of-circumstances approach." [\[FN235\]](#)

In sum, Miranda's safeguards, " '[p]rophylactic' though [they] may be," [\[FN236\]](#) cannot simply be abrogated. They can be replaced only by other rules, likewise "prophylactic," and such substitutes must *556 serve at least equally well to dispel compelling interrogation pressures, prevent brutality, and give clear guidance to the police. Professor Cassell's proposals must be tested against those three requirements.

C. The Cassell Proposals

Videotaping is an extremely valuable tool -- for both the police and the suspect. Many police departments now use it routinely, and a number of foreign countries require it. [\[FN237\]](#) It can help protect police from charges of brutality or misrepresentation, and it can provide a record of the defendant's statements if he refuses to sign a typed confession, or if he collapses from exhaustion or stress at the end of the interrogation session. [\[FN238\]](#)

For related reasons, videotaping helps protect the suspect -- by inhibiting brutality (at least when there are safeguards to

prevent the tape from being turned off or erased) and by providing a clear record of what warnings were given and what efforts the suspect made to invoke his rights. It is difficult to see how police interrogators could have any reasons (other than illegitimate reasons) to oppose videotaping. [\[FN239\]](#)

If seen as a supplement to Miranda, videotaping would clearly help protect suspects in custody, without infringing any legitimate law enforcement interest. But a videotaping requirement would lose much of its value if it were merely a replacement for Miranda. In effect, the proposal to substitute videotaping for Miranda amounts to a police offer in the form, "We'll stop lying about what we do, if you allow us to do it." No doubt a videotaped record would often prevent police abuse and manipulation of the "swearing contest." But without clear substantive requirements against which to test the police behavior that the videotape will reveal, the objective record will lack any specific legal implications.

*557 Miranda's central concern, of course, is to dispel the pressures that custodial interrogation brings to bear on the suspect. A videotaping requirement that replaced Miranda would do little to further this goal. Videotaping would deter most officers from beating the suspect, but it would not prevent psychological pressure, ignoring requests to consult counsel, or disregarding pleas to terminate the interview. The principal way that videotaping might help dissipate psychological coercion would be by reassuring the suspect that he won't be beaten while the camera is on. But Cassell is aware that telling the suspect about the videotape sometimes strengthens resistance to confessing. [\[FN240\]](#) so he is willing to let police videotape the interrogation covertly. [\[FN241\]](#) At that point, we lose whatever fear-dispelling effects an announced policy of videotaping might promise. And of course the tape lays down no rules of behavior and gives the police no guidance about the tactics they may use. Thus, videotaping, though an excellent idea, does not meet the constitutional concerns about compulsion to which the Miranda safeguards are addressed. Videotaping could provide a useful complement to the Miranda protections, but it cannot replace them.

The heart of the Cassell proposals is his recommendation to strip arrested suspects of their right to consult counsel during pre-arraignment interrogation, to eliminate the requirement that interrogation be preceded by an explicit waiver of rights, and to eliminate Miranda's requirement that interrogation cease if a suspect makes a clear request to break off questioning or to consult with counsel. The requirements he would eliminate are not only central to the Miranda safeguards but have now become entrenched in the interrogation procedures of many countries around the world. [\[FN242\]](#)

Edwards v. Arizona [\[FN243\]](#) provides a suitable illustration of the difference between the Miranda safeguards and those Cassell would put in their place. In Edwards the defendant, under interrogation, invoked his Miranda right to counsel, and police then returned him to his cell. Later, detectives sought to question him again without counsel; *558 when the defendant refused, a guard told him that he "had to" and led him unwillingly to the interrogation room. The Supreme Court unanimously held the resulting confession inadmissible. Its opinion was written by Justice White, the Court's most long-standing critic of Miranda, and concurring opinions were written by Chief Justice Burger and by Justice Powell, who was joined by Justice Rehnquist.

Tactics like those condemned in Edwards would be encouraged under the Cassell proposal and would no doubt become commonplace. Cassell estimates that 20% of arrested suspects (some 550,000 persons per year) at some point invoke their right to remain silent. [\[FN244\]](#) Under Cassell's approach, police could require (literally compel) these individuals to submit to the interrogation process. We must remember that Miranda's cut-off rules are triggered only by an unequivocal expression of the desire to remain silent. [\[FN245\]](#) The Cassell proposal would permit police investigators to insistently question suspects who have clearly and explicitly asked to be left alone. This is not a regime of prophylactic safeguards, but an explicit recipe for involuntary custodial interrogation, in direct violation of the Fifth Amendment.

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The other elements of Cassell's package would do little to mitigate this problem. Suspects would have no right to consult with counsel prior to formal arraignment. They would be told they have a right to remain silent, but the protective effect of that warning would be immediately negated by police actions. Instead of "scrupulously honoring" a suspect's decision to remain silent, as *Miranda* requires, [FN246] police would be authorized to ignore the suspect's decision and press forward with the interrogation. Will suspects treated this way really have (or think they have) the right to remain silent? All they will have (mixed messages notwithstanding) is the legally sanctioned obligation to submit to an unwanted custodial interrogation. [FN247]

The distinction Cassell needs to draw here, to render his proposal plausible, is the distinction between compelling the suspect to answer, which the Constitution clearly forbids, and compelling him to submit to questions, which Cassell thinks should be permissible. Involuntary statements would be ruled out, but not involuntary interrogation. In the context of custodial police interrogation, there is only a slender conceptual difference here, not a practical difference that can possibly matter. If the suspect in police custody chooses to make no statement and asks for questioning to cease, if the interrogators who hold him in *559 their power ignore that request and continue asking him to say something, are they merely compelling him to listen? Or are they not, in every practical sense, compelling (or trying to compel) him to answer? Justice White, an initial critic of *Miranda*, came to see this point clearly, writing for the Court in *Minnesota v. Murphy*:

[T]he coercion inherent in custodial interrogation derives in large measure from an interrogator's insinuations that the interrogation will continue until a confession is obtained [The suspect] is painfully aware that he literally cannot escape a persistent custodial interrogator. [FN248]

The Cassell system would not only enhance (rather than dispel) these inherently compelling pressures, but it would fail to meet *Miranda*'s other central concerns as well. Officers interrogating an unwilling suspect would have no guidance about the tactics they could use or when they should relent, except for the rule that they should not stop questioning just because the suspect refuses to answer or unequivocally pleads for questioning to cease. Notoriously vague due process concepts would provide the only limits. Cassell offers nothing to fill this crucial void in his proposal, except to state that "[c]ontinued persistence to convince a suspect to change his mind will, at some point, render a confession involuntary" [FN249]

This return to the vague due process test would compel hundreds of thousands of unwilling suspects (roughly half a million suspects per year) to submit to involuntary questioning, and it would leave police (and courts) without any predictable framework for judging how long such an involuntary interrogation could last and what kinds of tactics could be used. Worse yet, since the murky boundaries for dealing with recalcitrant suspects would sometimes be overstepped, serious instances of physical and psychological abuse would inevitably occur even among the best-intentioned officers.

All to what end? Is it the hope that law enforcement practices would eventually settle into the new regime, that truly egregious abuses would not occur too often, that society will tolerate subjecting arrested suspects to pressures the Fifth Amendment clearly forbids in every other context, and that a microscopically small increase in the net conviction rate would eventually be realized as a result? It is no wonder that law enforcement officials themselves are in the forefront of those opposing reconsideration of *Miranda* [FN250] and that the Supreme Court, heeding their voice, has just recently reaffirmed the soundness and constitutional legitimacy of *Miranda*'s approach. [FN251] As Richard Leo writes, overruling *Miranda* at this late date "would represent a *560 regression at a time when the institutions of law enforcement have not only successfully adapted to the legal requirements of *Miranda*, but have publicly embraced *Miranda* as a legitimating symbol of their professionalism and commitment to fairness in the criminal process." [FN252] With or without a video camera to record the process, persistent custodial questioning of unwilling suspects is pragmatically undesirable and constitutionally indefensible.

D. Other Alternatives?

If Professor Cassell's proposals cannot pass constitutional muster, are there other approaches worth considering? Cassell believes that Miranda "blocked" the search for alternative approaches, [\[FN253\]](#) a consequence he sees as an "undeniable tragedy." [\[FN254\]](#) The truth is more nearly the opposite. Miranda encouraged states to seek other ways to safeguard the interrogation process. [\[FN255\]](#) Several states have done so, as Cassell indicates in his discussion of police experimentation with videotaping. [\[FN256\]](#) There is no lack of discussion of other methods for dealing with the pressures of custodial questioning -- methods such as permitting interrogation only before a magistrate in open court, [\[FN257\]](#) requiring that warnings be delivered by a magistrate or attorney, and requiring that any waiver be made in the presence of counsel. [\[FN258\]](#) That states have not adopted such methods as substitutes for Miranda cannot (as Cassell believes [\[FN259\]](#)) stem from fear the Court would invalidate them. Rather, the inattention to reforms of this sort clearly reflects a recognition that virtually any alternative that meets Miranda 's concerns about custodial pressure will impose infinitely greater burdens on law enforcement than do the Miranda rules themselves. [\[FN260\]](#)

Miranda assuredly was a compromise, because its rules permit some compelling pressures to continue despite the Fifth Amendment. That the Court was willing to balance away some part of Fifth Amendment requirements, in order to avert law-enforcement consequences it feared might be extreme, cannot become a justification for putting the whole Fifth Amendment up for grabs, or for tolerating clear cases of compulsory interrogation in the hope of averting some speculative and virtually undetectable social cost. Miranda 's compromise was not the only conceivable one, but any compromise that takes the Fifth *561 Amendment seriously will almost certainly be more restrictive and harder for police to live with than the now-familiar Miranda safeguards.

E. Does It Matter?

Underlying my analysis is a paradox that will trouble many readers, whether they are inclined to support Miranda or oppose it. If Miranda really has so little impact on confession and conviction rates, why bother defending it? Isn't Miranda simply a hollow promise for civil libertarians and an inconvenient nuisance for law enforcement?

If the Miranda Court's goal was to reduce or eliminate confessions, the decision was an abject failure. Plainly, however, the Warren Court had no such thought in mind; it explicitly structured Miranda's warning and waiver requirements to ensure that confessions could continue to be elicited and used. [\[FN261\]](#) Miranda's stated objective was not to eliminate confessions, but to eliminate compelling pressure in the interrogation process.

Yet here, too, there is a paradox. If the flow of confessions has not slackened, it would seem plausible to infer that the pressures deployed to produce those confessions have not slackened either. [\[FN262\]](#) But the dynamics of police interrogation are more complicated than that view implies. As recent observational studies of interrogation demonstrate, [\[FN263\]](#) today's suspects typically confess not because of fear of mistreatment but primarily because of misplaced confidence in their own ability to talk their way out of trouble. Detectives are trained to reinforce the suspect's hope of finding what David Simon calls "the Out":

Homicide detectives in Baltimore . . . like to imagine their suspects imagining a small, open window at the top of the long wall. The open window is the escape hatch, the Out. It is the perfect representation of what every suspect believes when he opens his mouth during an interrogation. Every last one envisions himself parrying questions with the right combination of alibi and excuse; every last one sees himself coming up with the right words, then crawling out the window to go home and sleep in his own bed. More often than not, a guilty man is looking for the Out from his first moments in the interrogation room [\[FN264\]](#) Similarly, Richard Leo's comprehensive study portrays modern police questioning as an elaborate "confidence game," in which the detective *562 subtly establishes rapport with his "mark," presents himself as the suspect's

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ally, and dupes the suspect into believing that he can help himself by letting out a portion of the facts. [\[FN265\]](#)

If there were an affirmative right not to incriminate oneself, analogous to the affirmative Sixth Amendment right to assistance of counsel at trial, then ill-informed, misguided waivers of the right would surely be invalid. But the Fifth Amendment protects suspects only against state-orchestrated compulsion, not against their own poor judgment.

For those concerned with the "bottom line," Miranda may appear to be a mere symbol. But the symbolic effects of criminal procedure safeguards are important. Those guarantees help shape the self-conception and define the role of conscientious police professionals; they underscore our constitutional commitment to restraint in an area in which emotions easily run uncontrolled. [\[FN266\]](#)

Miranda is, in any event, more than a mere symbol. In a constitutional system, procedure matters; the means to the end are never irrelevant. Miranda does not protect suspects from conviction but only from a particular method of conviction. The rate of confessions has not changed, but those confessions are now mostly the result of persuasion and the suspect's overconfidence, not of pressure and fear. That difference in method is crucial.

Still, one might wonder, is Miranda really necessary? At one time police may have thought that confessions could be obtained only through physical threats or compelling psychological pressure. Dragged unwillingly into Miranda's system of safeguards, interrogators quickly learned that they could achieve the same rates of success without pressuring unwilling suspects to talk. They now know that the Miranda approach "works," and many would no doubt continue to adhere to Miranda even if it were no longer mandated. But the temptations to revert to the old system -- whether in the heat of the moment or as a general practice -- are great. Attempts to evade the Miranda requirements or "bend" them to the limit remain commonplace in some police precincts. Without Miranda's legal ground rules firmly in place, the instincts of the moment would too often prove irresistible.

Miranda's detectable social costs are vanishingly small and its benefits are substantial -- for anyone who may someday be arrested and for police departments themselves. Yet we cannot afford to treat compliance with Miranda's safeguards as an optional matter.

*563 III. Conclusion

No thoughtful person can study American crime statistics or observe contemporary urban life without seeing the urgent need to do something about our unacceptably high crime rates. We have our differences about how this goal can be achieved. Those who think the answer lies in stronger deterrence support stringent sentences and more expenditure for prison construction, police, crime labs, prosecutors, and trial courts (to reduce pressure for lenient pleas). Others put their faith in elementary and secondary education, job training, drug treatment, community policing, full employment policies, community redevelopment, or scattered-site housing for the urban underclass. All the promising options cost money -- lots of it.

It is natural, but also risky, to seek solutions in quick-fix, "inexpensive" doctrinal change. If new procedures can improve efficiency, save money, and raise conviction rates -- without sacrificing other important values -- they are obviously worth implementing. But if the expected benefits are chimerical, then efforts to sell procedural change as a crime control strategy not only yield nothing but backfire, by furthering the illusion that constitutional rights are to blame for our predicament and by diverting attention from the real, hard, and expensive choices that must be made. The victims of crime, who should be uppermost in our minds, will be the first to suffer from this politically expedient but cruelly deceptive tactic.

Overruling Miranda, or replacing it with a watered-down set of interrogation rules, is an idea whose time has come -- and gone. [\[FN267\]](#) The Supreme Court's most recent decisions continue to reaffirm and enforce the Miranda safeguards. The

Court's reasons for doing so -- its sense of Fifth Amendment imperatives and its assessment of law enforcement realities -- are always fair targets for academic examination, but they hold up eminently well to such scrutiny. Miranda is -- and deserves to be -- here to stay.

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[FN1]. [384 U.S. 436 \(1966\)](#).

[FN2]. *Id.* at 467, 478 -79.

[FN3]. E.g., Tom Gibbons & Jim Casey, Ed Meese's War on Miranda Draws Scant Support, *Chi. Sun-Times*, Feb. 17, 1987, at 41. For a dissenting view, see Joseph D. Grano, *Confessions, Truth, and the Law* 27-58 (1993).

[FN4]. E.g., ABA Special Comm. on Criminal Justice in a Free Soc'y, *Criminal Justice in Crisis* 28 (1988) [hereinafter ABA Special Comm.] (noting results of survey in which "very strong majority" of judges, prosecutors, and police officers agreed that Miranda does not pose serious problems); Eduardo Paz-Martinez, *Police Chiefs Defend Miranda Against Meese Threats*, *Boston Globe*, Feb. 5, 1987, at 25, 29.

[FN5]. Paul G. Cassell, *Miranda's Social Costs: An Empirical Reassessment*, [90 Nw. U. L. Rev. 387 \(1996\)](#).

[FN6]. See *infra* text accompanying notes 61, 133 (claims of District Attorneys Hogan and Koota).

[FN7]. Welsh S. White, [Defending Miranda : A Reply to Professor Caplan](#), [39 Vand. L. Rev. 1, 20 \(1986\)](#); see also sources cited in Cassell, *supra* note 5, at 389 nn. 4 & 5.

[FN8]. See Liva Baker, *Miranda : Crime, Law and Politics* 180 - 81, 403 - 05 (1983); Gerald N. Rosenberg, *The Hollow Hope: Can Courts Bring About Social Change?* 325 -29 (1991); Otis H. Stephens, *The Supreme Court and Confessions of Guilt* 165 -200 (1973); see also sources collected in Yale Kamisar, *Police Interrogation and Confessions* 47- 49 & n.11 (1980).

[FN9]. See, e.g., ABA Special Comm., *supra* note 4; Gibbons & Casey, *supra* note 3; Wayne E. Green, *Police vs. 'Miranda' : Has the Supreme Court Really Hampered Law Enforcement?*, *Wall. St. J.*, Dec. 15, 1966, at 16 (reporting views of Kansas City police chief -- later FBI Director -- Clarence Kelley); Paz-Martinez, *supra* note 4; see also [Rhode Island v. Innis](#), [446 U.S. 291, 304 \(1980\)](#) (Burger, C.J., concurring) (noting that "law enforcement practices have adjusted to [Miranda's] strictures"); Tom C. Clark, *Observations: Criminal Justice in America*, 46 *Tex. L. Rev.* 742, 745 (former Justice, who had dissented in *Miranda*, recognized "error" in his "appraisal of [its] effect [[[s] upon the successful detection and prosecution of crime"); Yale Kamisar, *Landmark Ruling's Had No Detrimental Effect*, *Boston Globe*, Feb. 1, 1987, at A27 (quoting former Philadelphia District Attorney -- now Republican Senator -- Arlen Specter's conclusion that "law enforcement has become accommodated to *Miranda*, and therefore I see no reason to turn the clock back").

[FN10]. Office of Legal Policy, U.S. Dep't of Justice, *Report to the Attorney General on the Law of Pre-trial Interrogation*

(1986) [hereinafter Pre-trial Interrogation Report].

[FN11]. See, e.g., Gibbons & Casey, *supra* note 3; Paz-Martinez, *supra* note 4.

[FN12]. See sources collected in Cassell, *supra* note 5, at 389 nn. 4 & 5.

[FN13]. ABA Special Comm., *supra* note 4, at 1-2, 28. The Committee consisted of a federal judge, several academics, the Washington, D.C. chief of police, two defense attorneys, California's Attorney General, and the State's Attorney for Dade County, Florida (Janet Reno).

[FN14]. Cassell, *supra* note 5, at 395-437.

[FN15]. *Id.* at 416-17.

[FN16]. *Id.* at 433, 437-38.

[FN17]. Following Cassell's terminology, I refer to a change from 50% to 40% in the rate of suspects who confess as a 10% drop in the confession rate. Likewise, general references to "confessions" or the "confession" rate include not only outright confessions but all statements that contain incriminating admissions.

[FN18]. See Richard Angelo Leo, *Police Interrogation in America: A Study of Violence, Civility and Social Change* 332-33 (1994) (unpublished Ph.D. dissertation, University of California, Berkeley).

[FN19]. *Id.*

[FN20]. See *infra* text accompanying notes 164 -65 (Table 1).

[FN21]. See *infra* text accompanying notes 23-33.

[FN22]. See sources cited in *supra* notes 9 & 13.

[FN23]. See sources cited in *supra* notes 9 & 13; see also Richard H. Seeburger & R. Stanton Wettick, Jr., *Miranda in Pittsburgh -- A Statistical Study*, 29 U. Pitt. L. Rev. 1, 26 (1967) (noting that in 1961 Detroit police judged confessions to be necessary in 23.6% of their cases but that "improved investigation and preparation of the cases" in the wake of Miranda's precursor, [Escobedo v. Illinois](#), 378 U.S. 478 (1964), caused the necessity rate to drop to 15.2%).

[In a forthcoming reply to my analysis, Cassell disparages the comprehensive ABA study, see *supra* note 13, and offers as a more reliable source an unpublished letter written to (and apparently at the request of) the Meese Justice Department. Paul G. Cassell, *All Benefits, No Costs: The Grand Illusion of Miranda 's Defenders*, 90 Nw. U. L. Rev. (forthcoming Spring 1996). The letter describes a telephone survey in which the Director of the Police Executive Research Forum (PERF) polled its members to determine their views on Miranda . Cassell claims that the group favored modification of Miranda. A close look at the letter (which Cassell generously supplied to me) reveals that Cassell's claims for this study are untenable. First, the survey question was phrased in blatantly loaded terms: "Do you support the efforts of the Justice Department to have the Supreme Court reverse the Miranda decision[?]" Second, even with this spin on the question, only five of the twenty-one respondents did support the overruling of Miranda ; ten respondents said they "prefer[red] modification to reversal" and six opposed any modification of Miranda . Though Cassell (along with PERF's Director) characterizes the plurality as "favor[ing] 'reconsideration of Miranda and some modification,'" the view that these ten respondents actually expressed ("prefer modification to reversal ") (emphasis added) in context amounted to a rejection of the Justice Department's anti-

Miranda crusade. Third, the letter itself cautions that its report is based on a cursory phone survey" (emphasis added), and that the small group polled was not necessarily representative. In this regard, it is of interest that the survey also asked whether the police chiefs supported the U.S. Sentencing Commission's effort to develop guidelines for imposing the death penalty in federal cases. On this issue, the respondents split eighteen to one in favor (with only three giving qualified or conditional answers); the same group split six to five against overruling Miranda (with ten others preferring modification to overruling). Once its tenor is fully described and its findings presented in context, the unpublished letter actually provides further evidence of how few police officers remain seriously opposed to Miranda .]

[FN24]. Cassell, *supra* note 5, at 450-54.

[FN25]. *Id.* at 453-54.

[FN26]. See *id.* at 406 - 09 (excluding most of New Haven data); *id.* at 409 -12 (excluding Washington, D.C. data).

[FN27]. See *id.* at 454 n.384 (citing anecdotal evidence that some police departments initially violated many Miranda requirements, but offering no evidence that subsequent increases in rates of compliance were associated with decreasing confession rates).

[FN28]. See *id.* at 459 (Table 3) (post-1970 confession rates of 40.3% for Six City sample 1977, 51.3% for Jacksonville 1979, 36.6% for San Diego 1979, and 63.8% for Bay Area 1993 -- a range roughly comparable to the range pre-Miranda, which includes such estimates as 49% for New York County 1966, 45% for Philadelphia 1964, 21.5% for the District of Columbia 1966, 45% for Kings County 1966, 40% for New Orleans 1966, 40.4% for Los Angeles 1965, 30.9% for Baltimore 1961, 47% for Atlanta 1961, and 42.4% for Kings County 1961).

Cassell claims that his own study of Salt Lake City shows a 1994 confession rate of only 33% in that city. See *id.* at 457, 459 (Table 3); Paul G. Cassell & Bret S. Hayman, *Police Interrogation in the 1990s: An Empirical Study of the Effects of Miranda*, 43 *UCLA L. Rev.* (forthcoming 1996). But this 33% figure results from a good deal of unjustified juggling of the numbers. Cassell and Hayman expand their pool (and thus enlarge the denominator of their calculation) to include cases in which defendants were never questioned at all and cases in the low-success setting of noncustodial questioning (which is not governed by Miranda). In addition, they exclude from their count of successful custodial interrogations two categories of cases that are included in most pre- Miranda tallies of incriminating statements -- cases in which the confession was "volunteered" and cases in which the suspect gave a "denial with explanation." The authors included in the latter category (and thus excluded from the count of successful interrogations) such useful and highly incriminating statements as the statement of an assault suspect who admitted hitting the victim but claimed self-defense. Cassell & Hayman, *supra* .

On Cassell's own data, the Salt Lake confession rate in custodial interrogations was 48%. *Id.* If we add the "volunteered" confessions and the "denial[s] with explanation" that were incriminating, the confession rate rises to at least 54% -- a total that easily equals or exceeds comparable pre- Miranda figures. See George C. Thomas III, *Plain Talk About the Miranda Empirical Debate: A "Steady" State Hypothesis About Confessions*, 43 *UCLA L. Rev.* (forthcoming 1996).

[FN29]. See *supra* text accompanying notes 9 -13, 23.

[FN30]. Leo, *supra* note 18.

[FN31]. *Id.* at 126 - 67.

[FN32]. Cassell tries to deflect the force of the Leo data by a strained and self-contradictory attempt to shave Leo's reported confession rate from 64% down to 42%. See Cassell, *supra* note 5, at 456-57 & n.408. Even a 42% confession rate remains

on a par with many reported confession rates for the pre- Miranda period. See supra note 28. But in any case it is clear that Leo's 64% figure is the correct number. Cassell arrives at the 42% rate by counting only full confessions and statements that admit to some elements of the offense. Contrary to his own approach elsewhere and to the approach in most pre- Miranda studies used for comparison, Cassell here chooses not to count in the confession rate the 22.5% of the cases in which the suspect "provided some incriminating information ... but did not directly admit to any of the elements of the crime." Leo, supra note 18, at 268. Compare Cassell, supra note 5, at 395 n.29 (treating a "confession" as "encompassing not only outright admissions of guilt but also incriminating statements") with Cassell & Hayman, supra note 28 (defining "incriminating statement" to include statements that make a suspect appear suspicious, such as "stammering and sometimes contradictory answers," and defining "successful" interrogations to include not only those that produce "incriminating statements" but also those that lock the defendant into a false alibi).

[FN33]. Leo, supra note 18, at 367. There are other indications as well that Miranda, now that police have assimilated it and learned to use it, may actually generate a net increase in the rate of successful interrogations. Miranda's assurance that the suspect can cut off interrogation at any point encourages some suspects to talk, by lulling them into a false sense of security about the risks they face in offering an exculpatory tale. Cf. [Smith v. Illinois, 469 U.S. 91, 93 \(1984\)](#) (investigator told suspect, "[I]f you do agree to talk with me without a lawyer being present you can stop at any time you want to"; suspect responded, "All right. I'll talk to you then."). George Thomas suggests that Miranda may have a "double effect," simultaneously discouraging outright confessions but encouraging suspects to begin answering questions, and he argues that some of the available data indicate that "[a] reduction in admissions was ... more than offset by more cases in which suspects incriminated themselves without admitting guilt." George C. Thomas III, *Is Miranda A Real-World Failure?: A Plea for More (and Better) Empirical Evidence*, 43 UCLA L. Rev. (forthcoming 1996).

[FN34]. See infra text accompanying notes 55 - 164.

[FN35]. See Donald T. Campbell, *Reforms as Experiments*, 24 Am. Psychol. 409, 411 (1969) (listing five threats to validity of causal inference from time-series data); H. Laurence Ross et al., *Determining the Social Effects of a Legal Reform*, 13 Am. Behav. Scientist 493, 494 - 95 (1970) (discussing six reasons why interpretation of before-after data is difficult).

[FN36]. Project, *Interrogations in New Haven: The Impact of Miranda*, 76 Yale L.J. 1519, 1574 (1967) [hereinafter Project] (noting decline in confession rates as a result of new police administration and pre- Miranda court decisions).

[FN37]. See [Developments in the Law: Confessions, 79 Harv. L. Rev. 938 \(1966\)](#).

[FN38]. Project, supra note 36, at 1574 (noting suspects' expanded awareness of their rights).

[FN39]. Id. at 1573.

[FN40]. Cassell, supra note 5, at 450.

[FN41]. Project, supra note 36, at 1573.

[FN42]. Cassell, supra note 5, at 407-08.

[FN43]. Id.

[FN44]. [367 U.S. 643 \(1961\)](#).

[FN45]. [372 U.S. 335 \(1963\)](#).

[FN46]. Cassell does not argue for overruling either *Gideon* or *Mapp*.

[FN47]. See Stephen J. Schulhofer, Is [Plea Bargaining Inevitable?](#), [97 Harv. L. Rev. 1037, 1098 n.200 \(1984\)](#) (describing indigent defense services in Philadelphia prior to *Gideon*).

[FN48]. See Donald T. Campbell, Measuring the Effects of Social Innovations by Means of Time Series, in *Statistics: A Guide to the Unknown* 93, 95 (Judith M. Tanur et al. eds., 1989).

[FN49]. But see text accompanying notes 114, 117 *infra* (changes reported for Seaside City are not statistically significant).

[FN50]. Campbell, *supra* note 48, at 97- 99 (discussing "variety of ways in which statisticians are attempting to get appropriate tests of significance for the real-life time series").

[FN51]. See Stephen J. Schulhofer, Confessions and the Court, *79 Mich. L. Rev.* 865, 867- 69 (1981) (discussing vagueness and uncertainty of the voluntariness approach).

[FN52]. Grano, *supra* note 3, at 218 -22.

[FN53]. Pre-trial Interrogation Report, *supra* note 10, at 106 (1986) (proposing a system of revised warnings to replace those required by *Miranda*).

[FN54]. Cassell, *supra* note 5, at 496-97.

[FN55]. Seeburger & Wettick, *supra* note 23.

[FN56]. Cassell, *supra* note 5, at 396.

[FN57]. *Id.* at 496-97 (recommending that *Miranda* rules be replaced by warning of right to silence and right to appointed counsel when defendant is taken before a judge).

[FN58]. See Seeburger & Wettick, *supra* note 23, at 8 (noting that "within a short time after the *Miranda* decision there was almost total compliance with [its] requirements").

[FN59]. See *id.* at 12-13. In discussing data from Los Angeles, Cassell speculates that a similar but less significant change in questionnaire forms might have occurred there, and he cites the supposed change as a ground for dismissing the results of that study, which showed no negative impact post-*Miranda*. See *infra* text accompanying notes 149 - 54. The same concern applies with even greater force to Pittsburgh's 1967 post- *Miranda* rate, which Cassell nonetheless chooses to include.

[FN60]. See Seeburger & Wettick, *supra* note 23, at 13. Table 3, showing that the five major felonies used in the first post-*Miranda* sample (and in the pre- *Miranda* sample) comprised only 63% of the cases in the second post-*Miranda* sample. If we recalculate the second post- *Miranda* rate, using only the set of offenses from which the pre- *Miranda* and immediate post-*Miranda* rates were derived, the second post- *Miranda* confession rate rises to 28.7%; the drop from the pre- *Miranda* baseline thus becomes only 17.3% (not 21.4%) for the 1967 sample. Thus, even if the 1967 estimate were methodologically comparable to that for 1966, the slightly greater decline (1.2%) for the latter year would be much less than the 2% decline that could be expected from long-term trends and causes independent of *Miranda*. See *supra* text accompanying notes 36 - 47.

[FN61]. Controlling Crime Through More Effective Law Enforcement: Hearings Before the Subcomm. on Criminal Laws and Procedures of the Senate Comm. on the Judiciary, 90th Cong., 1st Sess. 1120 (1967) [hereinafter Controlling Crime Hearings].

[FN62]. [Johnson v. New Jersey, 384 U.S. 719 \(1966\)](#).

[FN63]. See [Stephen J. Schulhofer, The Fifth Amendment at Justice: A Reply, 54 U. Chi. L. Rev. 950, 955 n.21 \(1987\)](#).

[FN64]. Cassell, *supra* note 5, at 418 (Table 1).

[FN65]. *Id.* (Table 1).

[FN66]. *Id.* (Table 1).

[FN67]. *Id.* at 400.

[FN68]. *Id.* at 400 n.65.

[FN69]. *Id.* at 400 n.67. The news article Cassell cites reveals that the suspect was arrested in April 1966, questioned prior to Miranda, and not indicted until November 1966.

[FN70]. *Id.* at 400.

[FN71]. See *infra* note 78 for further evidence that Hogan's 14.5% figure necessarily excludes some of the confessions that were given by suspects in the pool of July-December cases.

[FN72]. Cassell, *supra* note 5, at 400 (quoting Controlling Crime Hearings, *supra* note 61, at 1120).

[FN73]. *Id.* (quoting Stephen J. Markman, [The Fifth Amendment and Custodial Questioning: A Response to "Reconsidering Miranda," 54 U. Chi. L. Rev. 938, 946 n.19 \(1987\)](#)).

[FN74]. Controlling Crime Hearings, *supra* note 61, at 1120.

[FN75]. Cassell, *supra* note 5, at 400 (quoting Markman, *supra* note 73, at 946 n.19).

[FN76]. See *supra* text accompanying notes 68 - 69.

[FN77]. Markman, *supra* note 73, at 946 n.19.

[FN78]. The data make clear that the New York prosecutors could not possibly have been acting in the way that Cassell and Markman suppose. If pre- Miranda confessions were presented to grand juries in the July-December period (as Cassell wants to assume), and if the confession rate in these pre- Miranda cases was 49% (as Hogan reports), the confession rate for the post- Miranda segment of the July-December pool would necessarily be even lower than the 15% rate recorded for that pool as a whole. A numerical example will make clear the implausibility of the resulting scenario that Cassell's argument requires. Assume that one-quarter of the July-December cases involved pre- Miranda interrogations. If pre- Miranda confessions were presented to grand juries, and if the confession rate for that pre- Miranda segment of the pool was 49%, the confession rate for the post- Miranda segment of the pool would be only 3.7% $[(.49 \times .25) + (.037 \times .75) = .15]$. And if more than one-quarter of the July-December cases involved pre- Miranda confessions, the confession rate for the post- Miranda cases in

the pool would have dropped from 49% virtually to zero.

These calculations would appear useful for those attempting to find a large post-Miranda effect, except that the results are clearly too good (from that perspective) to be true. The largest Miranda effect reported in the studies Cassell credits (other than New York) is Philadelphia's 24.6% drop, and the average drop in the seven studies he credits (other than New York) is 13.5%. Cassell, *supra* note 5, at 418 (Table 1). Since the conjectures needed to salvage the New York study imply a drop of 40 - 45% or more (with a post-Miranda confession rate well under 5%), those conjectures clearly cannot be correct.

[FN79]. Cassell, *supra* note 5, at 400 (quoting Controlling Crime Hearings, *supra* note 61, at 1120).

[FN80]. Controlling Crime Hearings, *supra* note 61, at 1122.

[FN81]. *Id.* (emphasis added).

[FN82]. See Cassell, *supra* note 5, at 401.

[FN83]. Controlling Crime Hearings, *supra* note 61, at 1122.

[FN84]. *Id.*

[FN85]. Vera Inst. of Justice, Monitored Interrogations Project -- Final Report: Statistical Analysis (1967), cited in Cassell, *supra* note 5, at 401 & n.71.

[FN86]. *Id.* at 24, 45.

[FN87]. Although the Vera tables mostly do not indicate which offenses were classified as misdemeanors, they do specify that there were 141 drug misdemeanors (out of 1460 total cases) in one sample and 154 drug misdemeanors (out of 768 total cases) in the other sample. *Id.* at 24, 45.

[FN88]. *Id.* at 22-23 (emphasis added); see also *id.* at 44.

[FN89]. [In a forthcoming reply to the above criticism, Cassell attempts to render Vera's case pool comparable to Hogan's by focusing on the principal felony cases in the Vera study. See Cassell, *supra* note 23. He calculates that the confession rate in these cases (aggravated assault, robbery, rape, burglary, grand larceny, felony drugs, and arson) was 16.1% and 24.8% in the two Vera samples. *Id.* But this winnowing of the Vera pool is not nearly sufficient to render it comparable to Hogan's: only 40% of these serious felony cases proceeded to the grand jury stage! (The 20th Precinct sample had 447 such cases in a six-month period; the remaining Manhattan precincts had 946 such cases in a one-month period -- the equivalent of 5676 cases over six months. See Vera Inst. of Justice, *supra* note 85, at 2, 26, 47. Thus, for Manhattan as a whole, the serious felony arrests Vera studied would represent a pool of 6123 cases over a six-month period. Yet Hogan reports that over an equivalent six-month period, only 2448 nonhomicide felony cases were presented to the grand jury.)

Which kinds of cases would be among the 40% selected for presentation to the grand jury? Hogan described them as "the more solid and serious felony cases." Controlling Crime Hearings, *supra* note 61, at 1120. To obtain a sample comparable to Hogan's, we therefore must exclude the least serious of these felony cases -- probably grand larceny (confession rates of 12% and 24%) and (as viewed in 1967) the felony drug cases (confession rates of 13% and 7%). Above all, we must exclude the cases with weak evidence -- a pool in which the confession rate was by definition well below average. Inevitably, in the segment of the Vera cases that remain -- the only subset that would be comparable to Hogan's -- the confession rate will be much higher than the 16.6% and 25.6% figures Cassell reconstructs, and far out of line with the 14.5% figure Hogan

reported.]

[FN90]. Controlling Crime Hearings, *supra* note 61, at 199 -219.

[FN91]. [Escobedo v. Illinois, 378 U.S. 478 \(1964\)](#).

[FN92]. [United States ex rel. Russo v. New Jersey, 351 F.2d 429 \(3d Cir. 1965\)](#).

[FN93]. Controlling Crime Hearings, *supra* note 61, at 201 (statement of Arlen Specter).

[FN94]. Cassell, *supra* note 5, at 403. In addition to the studies Cassell cites, which report that about 50% of statements given are incriminating, *id.* at 403 n.89, the Hogan homicide study, see *supra* text accompanying notes 83 - 84, likewise found that half the statements given were incriminating.

[FN95]. See Harold E. Pepinsky, A Theory of Police Reaction to *Miranda v. Arizona*, 16 *Crime & Delinq.* 379, 382 (1970).

[FN96]. Cassell, *supra* note 5, at 404.

[FN97]. See *supra* text accompanying notes 23 -33.

[FN98]. See Kamisar, *supra* note 9 (quoting Specter to this effect).

[FN99]. See *supra* text accompanying notes 82 - 84.

[FN100]. In *United States ex rel. Russo v. New Jersey*, the Third Circuit held a confession inadmissible where the suspect had not previously been warned of his right to remain silent and his right to consult an attorney. [351 F.2d 429, 440 \(3d Cir. 1965\)](#). Thereafter the Philadelphia police began advising suspects of their right to consult counsel. Russo did not, however, require (and Philadelphia police did not at that point adopt) *Miranda's* other main safeguards -- the suspect's right to cut off questioning and the prosecution's heavy burden of establishing an express waiver. (On the latter point, Russo did stress the need for proof that "an accused was offered counsel but intelligently and understandingly rejected the offer." *Id.* at 440 (quoting [Carnley v. Cochran, 369 U.S. 506, 576 \(1962\)](#)). But the court also referred approvingly to cases in which waiver had been inferred because the suspect confessed after being warned of his rights. *Id.* Russo did not lay down any clear *Miranda*-like guidelines for waiver, and Specter does not refer to any such guidelines having been imposed.

In sum, Russo did not anticipate the parts of *Miranda* that Cassell wants to jettison, and the warning requirements it did adopt were close to those Cassell himself advocates. The only significant difference is that Cassell's warning would not include the right to consult counsel immediately, but as a practical matter it seems unlikely that the slightly stronger wording of the Russo warning would by itself produce a significant difference in the confession rate, and Cassell offers no evidence to the contrary. Although the Russo rules are not perfectly congruent with Cassell's approach, they are far closer to it than was the regime followed in Philadelphia prior to Russo or Escobedo.

[FN101]. See Controlling Crime Hearings, *supra* note 61, at 200 (statement of Arlen Specter indicating that police began giving warnings after Russo). Similarly, in Russo itself (a Newark, N.J. case) the suspect, who had been interrogated in 1961, received no warnings at all. See [Russo, 351 F.2d at 431, 433](#).

[FN102]. See [Commonwealth ex rel. Kern v. Banmiller, 187 A.2d 185, 190 \(Pa. Super. Ct. 1962\)](#). For contemporary readers who understandably will find cases like Kern hard to believe, the court's discussion of the interrogation issue is worth quoting in its entirety:

It is true that the F.B.I. officers stripped Kern of his clothes in making a search of him immediately after being taken to the police station. This was testified to be standard practice and the reasons given therefor. It is not denied that after this took place, Kern was permitted to put on his underwear, and it was in this condition that the statement was obtained. Counsel for Kern now contends that this was psychological coercion. Amazingly, Kern himself never referred to this as being a cause for giving or signing any alleged confession. The officers testified that there was no beating and abuse Id. Kern simply ignores [Malinsky v. New York, 324 U.S. 401, 407 \(1945\)](#), where the Court had held, 17 years earlier, that a confession obtained under such circumstances violates the due-process voluntariness requirement.

[\[FN103\]](#). See supra note 100.

[\[FN104\]](#). These figures reflect Specter's finding that the statement rate dropped by 27.6% (from 68.3% post- Russo, pre-Miranda to 40.7% post-Miranda), together with Cassell's assumption that in both periods, 50% of the statements were incriminating. However, as discussed above, see supra text accompanying note 99, we cannot be sure that the proportion of statements that were incriminating remained constant during the two periods. This concern is especially important in comparing periods with and without warnings because Cassell's sources provide specific indications that warnings tend to increase the proportion of statements that are incriminating. See supra text accompanying notes 82-84. Thus, we cannot convert a change in the statement rate before and after warnings into a reliable estimate of the change in the confessions rate. See supra text accompanying note 99.

The comparison in the text above (statement rates under Miranda to those in a regime with partial warnings) poses a similar problem: the proportion of statements that were incriminating might not remain constant. A cautious analysis would therefore discard the comparison of statement rates in the post- Russo, pre- Miranda period to those in the post- Miranda period. But the problem of potentially shifting rates of incriminating statements is less acute in this instance: when both regimes warn suspects of their right to silence, there is less basis for expecting shifts in the proportion of incriminating statements. Cassell therefore might fairly claim that rejecting the last phase of the Philadelphia study would be unduly technical and speculative. Since the proper treatment of that part of the study seems a reasonable judgment call, I follow Cassell in assuming that the 27.6% drop in Philadelphia's statement rate during the last phase can be converted into an estimated 13.8% drop in its confession rate. If we were instead to exclude the 13.8% figure, as a more cautious approach would require, the average confession-rate drop for the remaining cities would be even smaller than the 9.7% and 6.4% figures that result when Philadelphia is included. See infra text accompanying notes 164-65 (Table 1).

[\[FN105\]](#). James W. Witt, Non-Coercive Interrogation and the Administration of Criminal Justice: The Impact of Miranda on Police Effectuality, 64 J. Crim. L. & Criminology 320 (1973).

[\[FN106\]](#). Id. at 322-23.

[\[FN107\]](#). Id. at 323.

[\[FN108\]](#). Id. at 322-23.

[\[FN109\]](#). [People v. Dorado, 398 P.2d 361 \(Cal. 1965\)](#).

[\[FN110\]](#). Cassell, supra note 5, at 405.

[\[FN111\]](#). Id.

[\[FN112\]](#). In Dorado, the California Supreme Court held a confession inadmissible because the suspect had not been informed

of his right to remain silent and his right to counsel. [Dorado, 398 P.2d at 371](#). The California court imposed no interrogation cut-off rules and no special requirements for proving waiver. Indeed, Dorado appears to assume that waiver can be inferred if the suspect already knows his rights or is properly informed of them. *Id.* ("Obviously, defendant could not waive the right to remain silent unless he knew of that right.... [I]n the absence of knowledge such waiver requires warning, but the officials gave no warning here."). Thus Dorado, like the Third Circuit's decision in Russo, see *supra* note 94, did not anticipate the parts of Miranda to which Cassell objects and imposed only a warning requirement similar to the one Cassell would retain as a replacement for Miranda.

[\[FN113\]](#). In a purely historical inquiry, designed to compare Miranda to the 1960s voluntariness regime, the proper treatment of the Seaside data is debatable, just as it was for Philadelphia. See *supra* text accompanying notes 103 - 05. One could discard the Seaside figure as irrelevant or treat it as a useful but possibly low estimate of the difference between Miranda and a regime without any warnings. Cassell follows the latter course and includes Seaside in his tabulations. On balance, this is a reasonable approach, since there appears to be little difference in confession rates between a regime with partial warnings and one with no warnings at all. As Cassell acknowledges, there was a "clear consensus" among prosecutors "that confession rates remained constant even in those states where Escobedo had been extended [as Dorado did] to require the police to warn suspects of their rights," Cassell, *supra* note 5, at 493, and "[t]he available empirical evidence confirms that warnings have comparatively little effect on confession rates." *Id.* at 494.

[\[FN114\]](#). For the 2% decrease in confessions as a percentage of suspects incarcerated, chi-square = 0.18 and p = 0.671.

[\[FN115\]](#). Witt, *supra* note 105, at 325.

[\[FN116\]](#). *Id.*

[\[FN117\]](#). For the 2% increase in confessions as a percentage of suspects interrogated, chi-square = 0.22 and p = 0.638.

[\[FN118\]](#). Project, *supra* note 36, at 1573.

[\[FN119\]](#). See *supra* text accompanying notes 36 - 47.

[\[FN120\]](#). Project, *supra* note 36, at 1571, 1578.

[\[FN121\]](#). Cassell, *supra* note 5, at 408-09.

[\[FN122\]](#). Richard J. Medalie et al., Custodial Police Interrogation in Our Nation's Capital: The Attempt to Implement Miranda, 66 Mich. L. Rev. 1347, 1414 (Table E-1) (1968) (reporting 3% drop in statement rate); see also Cassell, *supra* note 5, at 410 (converting 3% drop in statement rate to a 1.5% drop in confession rate).

[\[FN123\]](#). Cassell, *supra* note 5, at 410.

[\[FN124\]](#). Seeburger & Wettick, *supra* note 23, at 26 n.51.

[\[FN125\]](#). *Id.* (emphasis added).

[\[FN126\]](#). Cassell, *supra* note 5, at 412-13.

[\[FN127\]](#). Cassell attempts to buttress the reasonableness of the 40% pre-Miranda estimate by referring to New Orleans data

that he believes indicate a 76.3% confession rate in a sample of 1961 cases terminated by guilty plea or trial. *Id.* at 413 n.157. There are several glaring problems here. First, New Orleans' confession rate for 1961 is a poor proxy for the relevant pre-Miranda baseline, because it includes confessions generated under a legal regime pre-dating Gideon, Mapp, and important refinements of the due-process voluntariness test. See *supra* text accompanying notes 36-47. In addition, Cassell's seemingly precise 76.3% figure for 1961 is based in part on his purely speculative guess about what the confession rate was in cases that went to trial. Finally, the supposed 76.3% confession rate in completed cases is not a measure of the confession rate in interrogations, since the confession rate is presumably very low in interrogations that lead to dismissal rather than guilty plea or trial. A 76% confession rate in completed cases thus gives no basis for corroborating whether the confession rate in pre-Miranda interrogations was 40% (as Cassell assumes) or, say, only 35% -- a figure that would put the confession-rate drop not at Cassell's 11.8% figure but instead at only 6.8%.

[FN128]. Green, *supra* note 9 (emphasis added).

[FN129]. Cassell, *supra* note 5, at 413.

[FN130]. On the reasonableness of this 50% figure, see *supra* note 94 and text accompanying note 99.

[FN131]. Cassell, *supra* note 5, at 413 n.158.

[FN132]. Green, *supra* note 9.

[FN133]. Controlling Crime Hearings, *supra* note 61, at 223.

[FN134]. See *supra* note 94 and accompanying text.

[FN135]. See *supra* text accompanying notes 82 - 84.

[FN136]. See *supra* text accompanying note 99.

[FN137]. See Cassell, *supra* note 5, at 414 (discussing newspaper account of Thompson's remarks).

[FN138]. [*Escobedo v. Illinois*, 378 U.S. 478 \(1964\)](#).

[FN139]. Cassell, *supra* note 5, at 414.

[FN140]. Evelle J. Younger, *Interrogation of Criminal Defendants -- Some Views on Miranda v. Arizona*, 35 *Fordham L. Rev.* 255, 256 (1966).

[FN141]. *Id.* at 259 - 60 (Table 1).

[FN142]. Cassell, *supra* note 5, at 415.

[FN143]. See *supra* text accompanying note 33.

[FN144]. See *supra* text accompanying notes 34 -50.

[FN145]. [*People v. Dorado*, 398 P.2d 361 \(Cal. 1965\)](#).

[\[FN146\]](#). See supra note 112.

[\[FN147\]](#). Cassell, supra note 5, at 416.

[\[FN148\]](#). See supra note 112. Indeed, the Los Angeles official in charge of compiling the survey data stressed "the increased scope of the admonitions required by Miranda over Dorado." Controlling Crime Hearings, supra note 61, at 344 (memo from Earl Osadchey to Lynn D. Compton, Assistant District Attorney).

[\[FN149\]](#). Cassell, supra note 5, at 415.

[\[FN150\]](#). Id. at 415-16 (quoting Controlling Crime Hearings, supra note 61, at 347, 350).

[\[FN151\]](#). Id. at 416.

[\[FN152\]](#). Controlling Crime Hearings, supra note 61, at 344, 347.

[\[FN153\]](#). Id. at 349, 350.

[\[FN154\]](#). The survey questionnaire for the post- Miranda period repeatedly uses the term "admission" as a synonym for the category "admission or other statement," see id. at 348, and the published law review version of the Los Angeles study simply uses the term "confession or admission" to identify cases that had been tabulated under the rubric "confession, admission or other statement" on the survey questionnaire. See Younger, supra note 140, at 259. Thus, there is no reason to suspect that the slight difference in the terminology of the worksheet categories had any substantive significance.

[In a reply to my comments, to be published in a forthcoming issue of the Northwestern University Law Review, Professor Cassell claims to have discovered "definitive proof " that the Los Angeles survey forms were materially redesigned. Cassell, supra note 23. In fact, his "definitive" document provides no new information, and it suffers from the same flaw as the evidence Cassell used in his original article. The document -- Younger's report of his Dorado- Miranda study -- was published almost verbatim in the Senate Hearings on which both Professor Cassell and I previously relied. The only unpublished portion of the report is its appendix containing the questionnaire forms used for the June 1966 (post- Miranda) survey. But, as I indicate in the text above, the published version of the report makes clear the exact wording that was used ("confession, admission or other statement") in the actual June 1966 survey questionnaire. See supra text accompanying note 150. What was not previously available was the December 1965 questionnaire form needed to corroborate Professor Cassell's speculation that it could have used narrower wording to describe the kinds of statements that were counted. The new evidence does nothing to fill this gap, since the only questionnaire form it provides is the June 1966 version, not the missing and supposedly narrower December 1965 version. If we are to speculate here, it would be more plausible to assume that the December 1965 forms were not materially different, since the Los Angeles District Attorney's Office did not bother to include them in the full version of its report that it lodged with the University of Utah Law Library.]

[\[FN155\]](#). See Younger, supra note 140, at 260 ("Confessions and admissions were present in 50% of the requests for felony complaints received from police agencies during [the post- Miranda] survey period.").

[\[FN156\]](#). That this was the understanding at the time is confirmed by Younger's use of the simple rubric "confession or admission" to describe the statements category in the published law review version of the study. See supra notes 154-55.

[\[FN157\]](#). Younger, supra note 140, at 260.

[\[FN158\]](#). See supra text accompanying notes 72-74.

(Cite as: 90 Nw. U. L. Rev. 500)

[FN159]. Cassell's forthcoming reply to my comments carries his reinterpretation of the Los Angeles results even further, arguing that with his adjustments, "the post- Miranda confession/admission rate would be 25% (one-half of 50%)," Cassell, *supra* note 23. This is simply not a defensible reading of what Younger meant when he wrote that "[c]onfessions and admissions were present in 50% of the [cases]." Younger, *supra* note 140, at 260. (To underscore the obvious, my own argument is not that Younger's words must be read literally, regardless of their context, but simply that, taken in context, his report clearly indicates that there were incriminating statements in 50% (not 25%) of the cases.)

[FN160]. See Brief, Amicus Curiae of Americans for Effective Law Enforcement et al., [Brewer v. Williams, 430 U.S. 387 \(1997\)](#) (No. 74-1263). In addition to joining this amicus brief, Younger served as a leading advisor on crime control issues to the 1968 presidential campaign of Richard Nixon, and after the election Nixon appointed him to head the President's Advisory Council on Crime and Law Enforcement. See Yale Kamisar, *How to Use, Abuse -- and Fight Back with -- Crime Statistics*, 25 Okla. L. Rev. 239, 256 (1972).

[FN161]. *Id.* at 255.

[FN162]. Cassell's double standard for assessing changes in methodology is also evident in his treatment of the Pittsburgh data, where he accepts a 1967 confession rate derived from a survey form that clearly differed in critical respects from the forms used in preceding periods. See *supra* note 59 and accompanying text.

[FN163]. Cassell, *supra* note 5, at 416.

[FN164]. See Controlling Crime Hearings, *supra* note 61, at 347. Cassell's claim to the contrary is based on a misquotation from the Younger report: Younger did state, as Cassell indicates, Cassell, *supra* note 5, at 416 n.179, that "many of the defendants in the preliminary [hearing] stage ... were arrested prior to Miranda" (emphasis and bracketed word added), but Younger raises no such caveat with regard to his complaint stage data, from which the relevant confession rates are drawn. Compare Younger, *supra* note 140, at 259 (Table IV) (data for "Complaint Stage") with *id.* at 260 (Table V) (data for "Preliminary Stage").

Cassell raises two other, similarly strained and inconsistent objections to the Los Angeles study. (1) He notes that the D.A.'s office had concerns about the accuracy of the December 1965 survey because "many of the replies received were incomplete or inconsistent." Cassell, *supra* note 5, at 415 n.175 (quoting Younger, *supra* note 140, at 256 n.7). But missing or incomplete data is a problem in virtually all social science empirical research; it does not invalidate conclusions drawn from the remaining data when (as here) there is no reason to believe that there is systematic bias in the distribution of the incomplete or invalid responses.

(2) Cassell objects that the complaint-stage data exclude cases in which police "were unable to obtain a vital confession and therefore did not request a complaint." Cassell, *supra* note 5, at 416. Again, this objection, if valid, should give Cassell identical doubts about the New York study of cases presented to the grand jury. In this instance, however, the objection is not valid (for either New York or Los Angeles). While we would like to know about changes in the confession rate in the missing segment of the cases, that gap in our knowledge does not systematically skew before-after comparisons for the segment that was studied. At the arrest stage, cases are of two types: those in which a confession is or is not needed to proceed. Absent a confession, cases in the former category drop from the sample when we proceed to the complaint stage. As a result, the confession rate for cases at the arrest stage will always be lower than the confession rate in cases that survive to the complaint stage, and a drop in the confession rate at the arrest stage will often translate into a larger drop in the confession rate as measured at the complaint stage. Only under extremely artificial assumptions can we construct examples in which a confession-rate increase at the complaint stage (as occurred in Los Angeles) will result from a confession-rate drop in the pool of all arrest cases.

[FN165]. Cassell, *supra* note 5, at 448.

[FN166]. *Id.* at 450.

[FN167]. See *supra* Table 1.

[FN168]. Cassell, *supra* note 5, at 450.

[FN169]. See *supra* text accompanying notes 36 - 47.

[FN170]. The time between the midpoints of the before and after periods was 21 months for Pittsburgh, 8 months for New York, 36 months for Philadelphia, 18 months for New Orleans, 12 months for Kings County, 30 months for Seaside City, and an undeterminate interval (which I assume to be zero) for New Haven and Kansas City. The 16 -month average interval cited in text is the weighted mean, as adjusted to give proper weight (68%) to the smaller cities.

[FN171]. For the reanalysis the time intervals are identical for Pittsburgh, Kansas City, Seaside City, and New Haven; the adjusted intervals are 9 months for Philadelphia and 7 months for Los Angeles. The 13 -month average is a weighted mean, calculated in the manner described in note 164 above. When the Los Angeles data are discarded (in order to compare Miranda to a regime with no warnings), the 5 remaining cities produce the same 13 -month weighted mean.

[FN172]. Cassell's 24% necessity figure is derived by averaging the necessity rates estimated for Pittsburgh (20.2%), New York County (27.5%), and Seaside City (23.6%). Cassell, *supra* note 5, at 433- 37. The Pittsburgh and Seaside estimates are reasonable, but the New York estimate should be excluded. The New York figure is based on an examination of homicide cases awaiting disposition in the D.A.'s office; the study specifically cautioned that necessity rates vary across offenses and that the 27.5% figure cannot be generalized to other crimes. Controlling Crime Hearings, *supra* note 61, at 1121 (statement of Frank S. Hogan, New York County District Attorney). In addition, the necessity judgment was based on the status of the file as it stood with the defendant's statement and did not make any attempt to consider whether the case could have been bolstered by further investigation. The 27.5% figure accordingly cannot serve as a reliable estimate of the confession necessity rate.

Conversely, Cassell unjustifiably excludes a New Haven study that estimated a 13.3% necessity rate. Cassell, *supra* note 5, at 426 -27. Cassell believes that the New Haven test of necessity was too stringent because the study treated a confession as unnecessary whenever (as he puts it) "the evidence (besides the confession) was sufficient only to take the case to trial but not to obtain a conviction." *Id.* at 427. This would indeed be an inappropriate test for a confession's necessity, but it is not the one the New Haven study employed; the study's authors state that they counted a confession as unnecessary when "the evidence would get the case to the jury but a guilty verdict did not appear certain." Project, *supra* note 36, at 1582 (emphasis added). This seems a roughly appropriate test, especially since a guilty verdict at trial is almost never "certain," even with a confession. In addition, the New Haven study cautioned that the 13.3% figure probably overstated the necessity rate because the 12 cases counted in the confession-necessary group included seven that the authors considered borderline, including two in which the authors believed that the suspect was innocent. *Id.* at 1587. The New Haven figures are no more vulnerable than the Pittsburgh and Seaside estimates, and they therefore deserve to be included. Averaging the three usable studies (Pittsburgh, Seaside, and New Haven) produces a necessity figure of 19%. To parallel Cassell's analysis, I use this figure for subsequent computations, even though there is reason to believe that it actually overstates the rate at which the loss of a confession damages law enforcement.

[FN173]. Cassell, *supra* note 5, at 440 -46.

[FN174]. See, e.g., Seeburger & Wettick, *supra* note 23, at 14 ("[A] confession was deemed necessary if we believed that a conviction would not have been likely without the use of a confession In making this determination we assumed ... average presentation of the case by counsel for both sides, decisions based on the merits of the case at the arraignment and trial levels and cooperation by witnesses"). Cassell himself has in mind a trial model for making necessity judgments: He interprets the New Haven authors as putting a confession in the unnecessary category where "the evidence (besides the confession) was sufficient only to take the case to trial but not to obtain a conviction." Cassell, *supra* note 5, at 427. Cassell calls this classification (precisely the one that his plea bargaining analysis presupposes) "rather curious" and concludes that the New Haven data understate the true confession necessity rate. *Id.*

[FN175]. It is disappointing in this connection to see Cassell repeat, as an example of Miranda's cost, the Office of Legal Policy's emotionally inflammatory but misleading example of Ronnie Gaspard, a Texan accused of a brutal murder, who was set free because of what Cassell calls "a Miranda technicality." Cassell, *supra* note 5, at 485. In fact, Miranda was irrelevant to Gaspard's release. Gaspard requested counsel at his arraignment, and counsel was formally appointed ten days later; the questioning that produced Gaspard's confession occurred three days afterward, i.e., 13 days after the formal request for counsel at arraignment. See [Stephen J. Schulhofer, Reconsidering Miranda, 54 U. Chi. L. Rev. 435, 458 -59 n.60 \(1987\)](#). Thus, Gaspard's release, though highly regrettable, was required by the violation of his Sixth Amendment right to counsel. See [Brewer v. Williams, 430 U.S. 387 \(1977\)](#). Cassell does not advocate repeal of the Sixth Amendment, and his own proposal expressly contemplates a right to appointed counsel as of the suspect's first judicial appearance. Cassell, *supra* note 5, at 496-97.

Equally misleading is Cassell's use of [Edwards v. Arizona, 451 U.S. 477 \(1981\)](#) as an example of a defendant who received a favorable plea bargain because of Miranda. See Cassell, *supra* note 5, at 442 n. 322. Edwards's confession was suppressed on Miranda grounds, but Edwards was then convicted by a jury on retrial and sentenced to life imprisonment. See Schulhofer, *supra* at 459 - 60. It was only when that subsequent conviction was reversed -- for improper admission of hearsay -- that Edwards pleaded guilty in return for a reduced (15 -year) sentence. *Id.* at 460 n.62. Since Cassell does not advocate repeal of the Confrontation Clause or the hearsay rule, his own approach would produce the identical result in Edwards.

[FN176]. See Stephen J. Schulhofer, *Due Process of Sentencing*, 128 U. Pa. L. Rev. 733 (1980).

[FN177]. *Id.* at 757; Kevin P. Reitz, [Sentencing Facts: Travesties of Real-Offense Sentencing, 45 Stan. L. Rev. 523 \(1993\)](#). Cassell acknowledges this qualification. Cassell, *supra* note 5, at 445 & n.339.

[FN178]. See United States Sentencing Commission, *Guidelines Manual* s 1B1.3 (Nov. 1994) [hereinafter *Guidelines Manual*] (defining "relevant conduct" that must be included in sentence computation).

[FN179]. In [United States v. Kikumura, 918 F.2d 1084 \(3d Cir. 1990\)](#), the court held that a substantial upward sentencing departure sometimes will require proof of the alleged conduct by clear and convincing evidence. But most circuits have rejected this approach and accepted the proof-by-a-preponderance standard even for upward departures that substantially enhance the otherwise applicable sentence. See, e.g., [United States v. Masters, 978 F.2d 281 \(7th Cir. 1992\)](#); [United States v. Restrepo, 946 F.2d 654 \(9th Cir. 1991\)](#).

[FN180]. E.g., [Masters, 978 F.2d 281](#); [United States v. Bailey, 892 F. Supp. 997 \(N.D. Ill. 1995\)](#).

[FN181]. *Guidelines Manual*, *supra* note 178, s 3E1.1 (authorizing downward departure of up to three levels for defendants who demonstrate acceptance of responsibility).

[FN182]. Cassell, *supra* note 5, at 440. Cassell's figure represents 3.8% of the 754,000 arrests for violent crime in 1993. But

the 3.8% figure is an estimate of the confession rate for "suspects questioned ," id. at 437, and roughly 20% of arrested suspects are never interrogated. See Cassell & Hayman, supra note 28. Thus, the population of violent offenders affected by Miranda consists of roughly 603,000 suspects (754,000 x 0.8), and Cassell's 3.8% figure implies an attrition of 23,000 cases.

[FN183]. Cassell, supra note 5, at 492-96.

[FN184]. Id. at 492-94.

[FN185]. Id. at 494 ("warnings have comparatively little effect on confession rates") (emphasis added).

[FN186]. Again, I assume that 80% of the arrested suspects would be interrogated, so that the number of lost convictions would be $(.8 \times 754,000) \times .0032 = 1930$.

[FN187]. See Cassell & Hayman, supra note 28. In the Cassell-Hayman sample, prosecutors dropped 44 of the 216 cases, and 9 of these (20.4% of the cases dropped) were dropped in spite of a successful interrogation.

[FN188]. See supra text accompanying notes 50-54; see also supra Table 1. n.**.

[FN189]. See supra text accompanying notes 171-81.

[FN190]. See supra text accompanying notes 23-33.

[FN191]. Cassell, supra note 5, at 486-92, 496-97.

[FN192]. See id. at 497 n.634 ("Continued persistence to convince a suspect to change his mind will, at some point, render a confession involuntary") (emphasis added).

[FN193]. See [Oregon v. Elstad, 470 U.S. 298, 312 \(1985\)](#) (stating this as the governing test of voluntariness).

[FN194]. Cassell, supra note 5, at 473.

[FN195]. Id.

[FN196]. Id.

[FN197]. Id. at 473-78.

[FN198]. Id. at 478.

[FN199]. Id. at 478 (quoting O. John Rogge, *Why Men Confess* 198 (1959)).

[FN200]. Id. at 437-40.

[FN201]. Id. at 476 (emphasis added).

[FN202]. Id. at 477 (emphasis added).

[FN203]. See infra text accompanying notes 231-32.

[FN204]. [380 U.S. 609 \(1965\)](#).

[FN205]. See, e.g., [James v. Kentucky, 466 U.S. 341 \(1984\)](#); [Carter v. Kentucky, 450 U.S. 288 \(1981\)](#); [Brooks v. Tennessee, 406 U.S. 605 \(1972\)](#).

[FN206]. [Carter, 450 U.S. 288](#). Only Justice Rehnquist dissented in Carter.

[FN207]. [431 U.S. 801 \(1977\)](#). Justice Rehnquist did not participate in Lefkowitz. Justice Stevens was the lone dissenter.

[FN208]. [Id. at 807](#).

[FN209]. See, e.g., [Withrow v. Williams, 113 S. Ct. 1745, 1759, 1761 \(1993\)](#) (O'Connor, J., dissenting) (appearing to treat "compelled or involuntary" statements as synonymous).

[FN210]. See Schulhofer, *supra* note 175, at 444 - 45.

[FN211]. [168 U.S. 532 \(1897\)](#).

[FN212]. [Id. at 543, 565](#) (emphasis added) (quoting 3 Russell on Crimes 478 (6th ed.)). Bram held that the Fifth Amendment rendered a prisoner's statement compelled and therefore inadmissible where, without other coercion, the interrogator told him, "If you had an accomplice, you should say so, and not have the blame of this horrible crime on your own shoulders." [Id. at 539](#).

Critics of Miranda sometimes suggest that this conception of Fifth Amendment compulsion is unworkable because "zero-value pressure simply is impossible Thus, unless the issue is seen as one of degree, either all statements are coerced or none are." Joseph D. Grano, [Selling the Idea to Tell the Truth: The Professional Interrogator and Modern Confessions Law, 84 Mich. L. Rev. 662, 685 n.101 \(1986\)](#). But Bram and other Fifth Amendment cases bar only those pressures that government exerts for the purpose of getting the suspect to talk. See Schulhofer, *supra* note 175, at 444 - 45. When the prosecution introduces incriminating evidence at trial, for example, its actions put pressure on the defendant to respond, but here the prosecution has acted not for the purpose of compelling the defendant to speak but for the legitimate purpose of meeting its burden of proof and obtaining a conviction regardless of whether the defendant speaks.

[FN213]. See, e.g., Grano, *supra* note 3, at 136 - 41.

[FN214]. See Schulhofer, *supra* note 175, at 437. It is therefore historically inaccurate to assume that "[l]ong before Miranda was decided, it was well established that the Fifth Amendment prohibited the introduction of compelled or involuntary confessions at trial." [Withrow v. Williams, 113 S. Ct. 1745, 1761 \(1993\)](#) (O'Connor, J., dissenting). On the contrary, the Fifth Amendment was wholly inapplicable to state criminal proceedings until [Malloy v. Hogan, 378 U.S. 1 \(1964\)](#); in federal cases, the Bram precedent had been quickly forgotten, and the sole test of admissibility for confessions given in police custody was the voluntariness test under the Due Process Clause, not the compulsion test under the Self-Incrimination Clause. See [Withrow, 113 S. Ct. at 1751](#); Schulhofer, *supra* note 175, at 436 -37. The part of Miranda that holds police interrogation subject to Fifth Amendment compulsion principles seems so thoroughly uncontroversial today that it is difficult for some judges and scholars to recall what a radical departure this was from the assumptions of the time, including not only the interrogation cases of the 1950s and early 1960s but even the Warren Court's own recent decision in [Escobedo v. Illinois, 378 U.S. 478 \(1964\)](#). The Fifth Amendment compulsion approach seemed so contrary to the weight of then-prevailing precedent that Miranda's lawyers decided not even to argue the Fifth Amendment claim. See Yale Kamisar et al., *Modern Criminal Procedure* 476 (8th ed. 1995) (quoting remarks of John J. Flynn).

[FN215]. [Miranda v. Arizona, 384 U.S. 436, 461 \(1966\)](#) ("We are satisfied that all the principles embodied in the privilege [against self-incrimination] apply to informal compulsion exerted by law-enforcement officers during in-custody questioning.") (emphasis added).

[FN216]. *Id.* ("As a practical matter, the compulsion to speak in the isolated setting of the police station may well be greater than in courts or other official investigations, where there are often impartial observers to guard against intimidation or trickery.").

[FN217]. See, e.g., [Gerald M. Caplan, Questioning Miranda, 38 Vand. L. Rev. 1417, 1430 \(1985\)](#) ("In the typical interrogation, however, there is some coercion; the suspect is detained, queried, challenged, and contradicted."); Grano, *supra* note 212, at 674 ("[A]ll such [interrogation] tactics, whether or not 'offensive,' are intended to increase the pressure -- the compulsion -- on the suspect to confess.").

[FN218]. See Grano, *supra* note 212, at 674.

[FN219]. [Miranda, 384 U.S. at 457.](#)

[FN220]. [417 U.S. 433 \(1974\).](#)

[FN221]. *Id.* at 444.

[FN222]. *Id.*

[FN223]. Cassell, *supra* note 5, at 472-73; see also Grano, *supra* note 3, at 175 - 82.

[FN224]. [Edwards v. Arizona, 451 U.S. 477 \(1981\).](#)

[FN225]. [Minnick v. Mississippi, 498 U.S. 146 \(1990\).](#)

[FN226]. [Arizona v. Roberson, 486 U.S. 675 \(1988\).](#)

[FN227]. See also [Smith v. Illinois, 469 U.S. 91 \(1984\)](#); [Estelle v. Smith, 451 U.S. 454 \(1981\).](#)

[FN228]. Edwin Meese III, Square Miranda Rights With Reason, Wall St. J., June 13, 1986, at 22.

[FN229]. [113 S. Ct. 1745, 1752 \(1993\)](#) (emphasis added) (quoting [Miranda v. Arizona, 384 U.S. 436, 467 \(1966\)](#)).

[FN230]. *Id.* at 1752-53 (quoting [United States v. Verdugo-Urquidez, 494 U.S. 259, 264 \(1990\)](#)) (emphasis supplied in Withrow).

[FN231]. See, e.g., Cassell, *supra* note 5, at 477.

[FN232]. As Wigmore noted: "The exercise of the power to extract answers begets a forgetfulness of the just limitations of that power. The simple and peaceful process of questioning breeds a readiness to resort to bullying and to physical force and torture." 8 John Henry Wigmore, Evidence 309 (3d ed. 1940).

[FN233]. [Fare v. Michael C., 439 U.S. 1310, 1314 \(1978\).](#)

[FN234]. [Michigan v. Tucker, 417 U.S. 433, 443 \(1974\)](#).

[FN235]. [Withrow, 113 S. Ct. at 1754](#).

[FN236]. [Id. at 1753](#).

[FN237]. See Cassell, *supra* note 5, at 486-92.

[FN238]. See, e.g., [Miller v. Fenton, 796 F.2d 598 \(3d Cir. 1986\)](#).

[FN239]. A forceful and thoroughly persuasive discussion of the value of videotaping is developed in Leo, *supra* note 18, at 401-14. After canvassing and puncturing various concerns sometimes raised against videotaping, Leo comments: The real reason many police officers and detectives ... oppose video-recording of confessions is that the taping of custodial questioning creates an objective record of the interrogation that exposes police to potential external criticism Moreover, the video-taping of custodial interrogations threatens to shift the balance of advantage between police and suspects in the "swearing contest" when their accounts of the interrogation differ in court With video-taping, no longer can two officers "cleanse" their notes to tell similar accounts that may contradict a suspect's testimony Videotaping custodial questioning thus represents a threat only to those officers who fear either receiving internal or external criticism about the legality of their interrogation methods or who fear losing a "swearing contest" that is adjudicated by an independent and objective record. *Id.* at 408 - 09; see also Kamisar, *supra* note 8, at 113 -37 (discussing need to impose a recording requirement for all custodial interrogation).

[FN240]. See Cassell, *supra* note 5, at 492 & nn.608-09.

[FN241]. *Id.* at 492 & n.610.

[FN242]. See [Craig M. Bradley, The Emerging International Consensus as to Criminal Procedure Rules, 14 Mich. J. Int'l L. 171, 185 \(1993\)](#) ("England's interrogation rules are more stringent than Miranda.") (emphasis added); *id.* at 197- 98 (noting that in Canada, a suspect must be informed upon arrest of his right to counsel, and that police "must refrain from attempting to elicit evidence from the detainee until he has a reasonable opportunity to retain and instruct counsel") (quoting Martin's Annual Criminal Code CH-21 (1991)); *id.* at 214 -15 (Germany requires warning of rights to silence and right " at all times, even before his examination, to consult with defense counsel of his choice") (emphasis added) (quoting German Code of Criminal Procedure s 136); [id. at 217-18](#) (noting that Italian interrogation rules prohibit use of statements made in the absence of counsel).

[FN243]. [451 U.S. 477 \(1981\)](#).

[FN244]. Cassell, *supra* note 5, at 496.

[FN245]. [Davis v. United States, 114 S. Ct. 2350 \(1994\)](#).

[FN246]. [Miranda v. Arizona, 384 U.S. 436, 479 \(1966\)](#).

[FN247]. See [Davis, 114 S. Ct. at 2362](#) (Souter, J., concurring) ("When a suspect understands his (expressed) wishes to have been ignored ..., in contravention of the 'rights' just read to him by his interrogator, he may well see further objection as futile and confession (true or not) as the only way to end his interrogation.").

[FN248]. [465 U.S. 420, 433 \(1984\)](#).

[FN249]. Cassell, supra note 5, at 497 n.634 (emphasis added).

[FN250]. See Brief for the Police Foundation et al. as Amici Curiae (cited in [Withrow v. Williams, 113 S. Ct. 1755 n.6 \(1993\)](#)); see also text accompanying supra notes 9 -13.

[FN251]. [Withrow, 113 S. Ct. at 1755](#).

[FN252]. Leo, supra note 18, at 399.

[FN253]. Cassell, supra note 5, at 498.

[FN254]. Id.

[FN255]. See [Miranda v. Arizona, 384 U.S. 436, 467 \(1966\)](#); see also [Withrow, 113 S. Ct. at 1752](#).

[FN256]. Cassell, supra note 5, at 489-90, 491-92 & n.605.

[FN257]. See, e.g., Kamisar, supra note 8, at 77- 94.

[FN258]. See, e.g., Charles J. Ogletree, Are [Confessions Really Good for the Soul?: A Proposal to Mirandize Miranda](#), 100 [Harv. L. Rev. 1826 \(1987\)](#).

[FN259]. Cassell, supra note 5, at 498.

[FN260]. See Schulhofer, supra note 175, at 460 - 61.

[FN261]. See [Michigan v. Tucker, 417 U.S. 433, 443 \(1974\)](#) (quoted in text accompanying note 234 supra); Schulhofer, supra note 51, at 880 - 81, 883 - 84.

[FN262]. Gerald Rosenberg appears to make just this assumption in concluding that Miranda was a failure. See Rosenberg, supra note 8, at 326 -29; see also David Simon, Homicide 199 (1991) ("[If] the Miranda decision was, in fact, an attempt to 'dispel the compelling atmosphere' of an interrogation, then it failed miserably.").

[FN263]. E.g., Simon, supra note 262, at 197-202; Leo, supra note 18, at 168 -220.

[FN264]. Simon, supra note 262, at 197.

[FN265]. Leo, supra note 18, at 220 -51.

[FN266]. See [Stone v. Powell, 428 U.S. 465, 492 \(1976\)](#); Schulhofer, supra note 51, at 892- 93.

[FN267]. Yale Kamisar, *Overturing Miranda : Idea Whose Time Has Come and Gone*, Legal Advertiser [Detroit], Feb. 26, 1987, at 1, 4.

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