A Report on “Do Progressive Prosecutors Increase Crime? A Quasi-Experimental Analysis of Crime Rates in the 100 Largest Counties, 2000–2020” by Petersen et al. (2024)

Reviewer 2

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> I am wiser than this person; for it is likely that neither of us knows anything fine and good, but he thinks he knows something when he does not know it, whereas I, just as I do not know, do not think I know, either. I seem, then, to be wiser than him in this small way, at least: that what I do not know, I do not think I know, either.
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> Plato, *The Apology of Socrates*, 21d

> To err is human. All human knowledge is fallible and therefore uncertain. It follows that we must distinguish sharply between truth and certainty. That to err is human means not only that we must constantly struggle against error, but also that, even when we have taken the greatest care, we cannot be completely certain that we have not made a mistake.
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> Karl Popper, 'Knowledge and the Shaping of Reality'

Overview

Citation: Petersen, N., Mitchell, O., & Yan, S. (2024). Do progressive prosecutors increase crime? A quasi-experimental analysis of crime rates in the 100 largest counties, 2000–2020. *Criminology & Public Policy*, 23, 459–490.

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Abstract Summary: This study examines the impact of progressive prosecutors taking office in the 100 most populous counties between 2000 and 2020 on crime rates, finding that their inauguration led to statistically higher index property and total crime rates, but generally not higher violent crime rates.

Key Methodology: Heterogeneous difference-in-differences regressions using an original database of progressive prosecutors and county-level crime data from 2000 to 2020.

Research Question: Do progressive prosecutors increase crime?

Summary

Is It Credible?

This study by Petersen et al. attempts to bring empirical rigor to the contentious debate surrounding "progressive prosecutors" and public safety. By analyzing 21 years of data across the 100 largest U.S. counties, the authors aim to overcome the limitations of previous city-level research. Their headline findings are that the inauguration of progressive prosecutors led to a relative increase in property crime of approximately 7% and a similar increase in total index crime, but had "no reliable overall effect" on violent crime (p. 482). They frame these results as evidence of a policy tradeoff, suggesting that higher property crime may be the "price" for the social justice advancements associated with progressive prosecution (p. 460). While the study's scope and methodological sophistication are commendable, the credibility of these causal claims is strained by significant data limitations and fragility in the research design.

The claim regarding increased property crime is the study's most robust statistical finding, yet it faces challenges regarding data integrity and causal attribution. The authors constructed their dataset by manually substituting approximately 11% of the county-year observations where they deemed the national data to be "irregular" or "abnormally large" (Supporting Information, Appendix text). While transparent, this reliance on subjective reconstruction—including replacing data for major jurisdictions like Cook County and New York City for the entire study period—introduces a non-trivial risk of measurement error. Furthermore, the causal link between the prosecutor and property crime is complicated by jurisdictional realities. The "progressive" designation is based on the policies of the county-level District Attorney, who typically handles felonies (p. 472). However, the property crime index is dominated by larceny-theft, an offense often prosecuted as a misdemeanor by separate city attorneys or municipal courts. If the "treatment" (the DA's policy) does not apply to the bulk of the "outcome" (misdemeanor theft), the observed increase may be driven by unmeasured confounders, such as concurrent changes in policing strategies or corporate reporting practices, rather than the prosecutor's actions.

The finding of a null effect on violent crime is analytically weaker. The authors acknowledge that their research design failed the "parallel trends" assumption for violent crime—meaning that counties electing progressive prosecutors were already on a different violence trajectory than traditional counties before any policy change occurred (p. 476). To salvage the analysis, they employed a conditional model that adjusts for county characteristics. While a standard econometric technique, this adjustment implies that the "no effect" finding is contingent on the model correctly accounting for all pre-existing differences. Given that progressive counties had significantly higher baseline violent crime rates in 2000 (p. 474), it remains ambiguous whether the lack of a significant effect reflects the success of progressive policies or the persistence of underlying structural differences that the model could not fully neutralize.

Finally, the study's conclusion that higher property crime is a "tradeoff" for reduced incarceration and racial inequality is an extrapolation rather than a direct empirical finding. The analysis measures the "cost" (crime) but assumes the "benefit" (decarceration) based on external literature (p. 484). Because the study does not measure incarceration rates or racial disparities within the same specific sample and timeframe, it cannot empirically confirm that the jurisdictions experiencing crime increases were the same ones achieving social justice gains. Additionally, the classification of prosecutors relies entirely on "stated prosecution policies" found on websites and campaign materials, not actual implementation (p. 472). Consequently, the study measures the impact of political branding, leaving open the question of whether the observed crime trends result from substantive policy changes or the signal that such branding sends to police and the public.

The Bottom Line

The study provides plausible evidence that jurisdictions electing progressive prosecutors experienced a relative rise in property crime, though this finding is clouded by subjective data reconstruction and the potential influence of unmeasured factors like policing changes. The claim that these prosecutors had no impact on violent crime is statistically fragile, as the analysis required significant adjustments to account for pre-existing differences in crime trends. Ultimately, the framing of a “tradeoff” between safety and justice is a theoretical interpretation rather than a proven empirical fact of this specific analysis.

Potential Issues

Failure of the parallel trends assumption for violent crime: The study's central causal claims about violent crime may not be credible because the research design fails to meet a foundational requirement. The difference-in-differences (DiD) method requires that counties with and without progressive prosecutors would have followed similar crime trends in the absence of the policy change. The authors' own data show this assumption is violated for violent crime. Baseline data from 2000, before any of the studied prosecutors took office, reveals that counties that would eventually elect progressive prosecutors already had statistically significantly higher violent crime rates (704.02 per 100,000) than counties that would not (526.15 per 100,000) (p. 474). The authors acknowledge this critical issue, stating that for violent crime, "the parallel trends test consistently rejected this assumption in the unconditional models" (p. 476). Their solution was to use a "conditional" DiD model, adding a set of county characteristics as controls to make the assumption "tenable." However, this statistical adjustment rests on the strong and untestable assumption that the selected covariates fully account for all unobserved factors driving the divergent pre-existing trends. This methodological compromise weakens the confidence that can be placed in the finding of "no reliable effect on violent crime," as the result may be an artifact of the statistical adjustment used to salvage a research design that did not meet its core prerequisite.

Omission of concurrent policy changes as potential confounders: The analysis attributes changes in crime rates solely to the inauguration of a progressive prosecutor, but it does not account for other simultaneous policy shifts that could be confounding the results. The election of a progressive prosecutor is often part of a broader political movement within a county that also leads to changes in policing (such as de-policing, consent decrees, or budget reallocations) and social spending. These concurrent changes are plausible alternative explanations for the observed crime trends. The study's DiD design does not control for such time-varying local policies. Therefore, the model may be incorrectly attributing an effect to the prosecutor that is actually the result of simultaneous changes in policing or other local government functions that occurred for the same underlying political reasons. The authors acknowledge that future research should examine the role of policing behaviors, but the absence of controls for these factors in the current model leaves the causal attribution vulnerable to omitted variable bias (p. 473).

Subjective reconstruction of outcome data: The study's crime rate data are not based on a single, consistent source but on a combination of national data and extensive, non-replicable substitutions from state-level reports. The authors replaced national Uniform Crime Reports (UCR) data based on their own judgment of what constituted an "abnormally large drop in crime" or "unusually low or high" statistics (p. 471). This was not a minor adjustment; the appendix reveals that "213 county-year crime measures were substituted," which accounts for approximately 11% of the final dataset's observations (Supporting Information, Appendix text). For some of the nation's largest jurisdictions, such as Cook County (Chicago) and four New York City counties, the authors replaced the national data for all 21 years of the study (Supporting Information, Table A3). While the authors are transparent about this process, the reliance on subjective criteria for data substitution introduces a potential source of systematic measurement error and makes the analysis difficult to replicate. This process could inadvertently create or obscure trends, affecting the validity of the findings.

Measurement of stated policy instead of implementation: The study's central independent variable—whether a prosecutor is "progressive"—is based on a coding of publicly stated policies from "official prosecutor websites, media searches, and campaign materials," not on their actual implementation in terms of charging, plea bargaining, or sentencing recommendations (p. 466). The authors acknowledge this limitation, stating, "our coding scheme focuses on stated prosecution policies, not actual practice" (p. 472). They justify this by arguing that public messaging is relevant for deterrence theory. However, other theoretical mechanisms discussed in the article, such as incapacitation, depend entirely on actual changes in prosecutorial behavior, not just rhetoric (p. 462). Without data on changes in prosecutorial actions, the study cannot distinguish between the effects of a prosecutor's political branding and the effects of their substantive reforms, limiting the ability to attribute the findings to the specific mechanisms of progressive prosecution.

Extrapolation beyond the study's empirical scope: The article's conclusion frames the findings as evidence of a policy "tradeoff," suggesting that higher property crime may be the "price" for advancements like reduced incarceration and racial inequality (pp. 460, 484). This conclusion is an extrapolation, not a direct finding of the study. The analysis provides evidence for one side of this ledger (a relative increase in property crime) but does not measure the other side (changes in incarceration rates or racial disparities in the same counties). Instead, it relies on citing external studies to establish the "benefit" part of the tradeoff. To empirically demonstrate a tradeoff, a study must show that the same jurisdictions that experienced the relative crime increase also experienced the benefits. By linking its own finding of a "cost" to other studies' findings of a "benefit," the article makes a rhetorical move in its policy discussion that extends beyond its own empirical evidence.

Potential violation of the no-interference assumption: The study's design assumes that crime trends in control group counties are unaffected by the election of progressive prosecutors in treatment group counties. This assumption, known as the Stable Unit Treatment Value Assumption (SUTVA), may not hold if there are spillover effects between counties, particularly those within the same metropolitan area. For instance, criminal activity could be displaced from a county with a traditional prosecutor to a neighboring county with a progressive prosecutor, or vice-versa. Such spillovers would contaminate the control group, biasing the estimated effect of the policy in an unknown direction. While this is a standard theoretical limitation for many DiD studies, the article does not discuss or test for this possibility.

Failure to consider alternative mechanisms for property crime changes: The analysis attributes the relative increase in property crime to the deterrence or incapacitation effects of prosecutorial policies but does not consider plausible alternative mechanisms. For example, in recent years, many large national retailers, which are disproportionately located in the large urban counties studied, have adopted corporate policies of non-engagement with shoplifters. Such a change is a time-varying factor that could increase recorded property crime (specifically larceny) and is likely correlated with the types of jurisdictions that elect progressive prosecutors. While it is debatable whether such changes would align perfectly with the staggered timing of prosecutor elections, the omission of such alternative explanations means the study may be attributing a trend to prosecutors that is at least partially explained by concurrent shifts in corporate or citizen behavior.

Reliance on police-reported crime data: The study uses UCR data, which captures only crimes reported to and recorded by police. This outcome measure is potentially influenced by the very policies being studied. For example, a progressive prosecutor's policy to de-prioritize arrests for certain low-level offenses could lead police to record fewer of those incidents or discourage citizens from reporting them. This would create an artificial decrease in crime. Conversely, policies aimed at improving community relations could increase crime reporting. The authors acknowledge this is a limitation of UCR data and mention the potential for a "feedback loop" between reporting and arrests (p. 473). This is a standard and often unavoidable limitation in criminological research, but it means the results may reflect changes in reporting and recording behavior rather than solely changes in underlying criminal activity.

Misalignment between prosecutorial jurisdiction and outcome data: There may be a misalignment between the scope of the "treatment" and the "outcome." The study focuses on county-level chief prosecutors, who are typically responsible for felony cases (p. 472). However, the outcome variable, UCR Index Crimes, includes offenses like larceny-theft, which are often charged as misdemeanors. In many large jurisdictions, misdemeanors are handled by a separate City Attorney's office, not the County District Attorney. This means the policies of the measured "progressive prosecutor" may not apply to a portion of the property crimes included in the outcome data. The authors acknowledge this complexity by excluding counties with multiple *felony* prosecutors, but the more common felony/misdemeanor jurisdictional split is not addressed and could introduce measurement error that biases the estimated effect.

Inclusion of the 2020 pandemic year: The study period ends in 2020, a year marked by the unique social disruptions of the COVID-19 pandemic. The authors acknowledge that the pandemic "likely affected crime rates in unique ways not representative of early time periods" (p. 466). The year-by-year results show that the estimated effect on property crime in 2020 is the largest in the entire 21-year series (p. 477). While the authors do not provide a robustness check excluding 2020, they do present year-by-year estimates showing that the effects on property crime were statistically significant in multiple years prior to the pandemic (e.g., 2013–2017, 2019). This mitigates the concern that the overall finding is driven solely by the anomalous 2020 data, though the inclusion of that year likely inflates the average effect size.

Exclusion of early adopter jurisdictions: To meet the requirements of the DiD model, the authors excluded four counties that had a progressive prosecutor in office before the study's start date in 2000 (p. 472). The authors transparently state this was a methodological necessity. However, this decision systematically removes the "early adopters" of progressive prosecution, which may be different in unobserved ways from the later adopters that constitute the treatment group. This necessary exclusion may limit the generalizability of the findings, as the results apply only to a specific subset of counties that adopted such policies after 2000.

Potential bias from balanced panel requirement: In constructing the crime data, the authors included only law enforcement agencies that reported data in every year from 2000 to 2021, creating a "balanced panel" (Supporting Information, Appendix text). The authors note this is to ensure that changes in crime are not artifacts of agencies entering or leaving the dataset. While this choice enhances internal validity, it means that any new police departments formed after 2000, or any that were consolidated or disbanded, were excluded. This could systematically omit crime data from the most demographically dynamic parts of counties, such as fast-growing suburbs, potentially biasing the overall county crime rate.

Potential bias in prosecutor classification from data availability: The classification of prosecutors as "progressive" is based on publicly available materials like websites and media reports (p. 466). This method may be susceptible to a "streetlight effect," where it is easier to find and code policies for prosecutors who are more adept at public relations or operate in major media markets. This could create a systematic bias where being coded as "progressive" is correlated with unobserved county characteristics like media scrutiny or political engagement, which could themselves be correlated with crime trends. The authors acknowledge their reliance on public, stated policies (p. 472), but this inherent limitation of the data source means the measure may capture media savvy as much as it captures substantive policy orientation.

Presentation and clerical errors: Several minor clerical errors appear in the article's presentation of descriptive statistics. First, the article states that total index crime fell by "45% between 2000 and 2020 in the full sample (4475 vs. 2347 per 100,000)" (p. 473); the correct calculation is a 47.6% decline ((4475 - 2347) / 4475). Second, the text states that "43% (19 out of 44)" of progressive prosecutors took office between 2017 and 2020 (p. 470); the data in Table 3 sum to 20 prosecutors (8+4+6+2), which is 45.5% of 44. Third, the discussion section summarizes the findings as "higher property (~6.7%) and total index (~7.0%) crime rates" (p. 482), but the calculations based on the results (p. 476) show the opposite: a ~7.0% increase for property crime (227.34 / 3255.7) and a ~6.7% increase for total crime (251.72 / 3773.9). These inconsistencies do not affect the main regression analyses but represent a lack of precision in the reporting of key contextual figures.

Future Research

Validation of policy implementation: Future work should move beyond coding prosecutors based on campaign rhetoric and website statements. Research could utilize administrative data to measure actual prosecutorial behavior, such as dismissal rates, diversion usage, and plea bargaining outcomes. This would allow researchers to distinguish between the effects of a prosecutor's political "brand"—which might influence deterrence through public perception—and the effects of their actual courtroom practices.

Integrated analysis of costs and benefits: To empirically validate the "tradeoff" hypothesis, future studies should employ a unified model that measures both crime rates and decarceration/disparity metrics within the same jurisdictions over the same period. By correlating the magnitude of the crime increase directly with the magnitude of the incarceration decrease at the county level, researchers could determine if the "price" of higher property crime actually yields the "purchase" of greater equity, or if these outcomes are uncorrelated.

Control for policing and enforcement mechanisms: Future research must account for the "feedback loop" between prosecutors and police. Models should include time-varying controls for police activity, such as arrest rates and clearance rates, to determine if changes in crime statistics are driven by prosecutorial policies or by a police response (e.g., de-policing) to the election of a reformist prosecutor. Additionally, distinguishing between felony and misdemeanor jurisdictions would clarify whether county DAs are causally responsible for changes in lower-level property crimes.

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