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An analysis of the relationship between probation caseloads and property crime rates in California counties

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Abstract

Each year more offenders are sentenced to probation than to any other sanction in the criminal justice system. In spite of the saliency of probation programs, the evidence is mixed concerning their effectiveness. In order to address this important gap in the research, the authors sought to determine if probation was effective in achieving one of its stated goals: protecting public safety. In doing so, they conducted a macro-level analysis, exploring the relationship between probation caseloads and property crime rates in each county in California over a nine-year period. Results from a two-way fixed effects regression model suggested that as probation loads increased, so did crime. While additional research into the effectiveness of probation is clearly needed, the results of the analysis nevertheless indicated that improvements in public safety could be expected if probation caseloads were reduced.
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Introduction

Probation is one of the most common sentences in the criminal justice system. It is no longer just reserved for misdemeanants or first-time offenders. Today's probationers include many criminals convicted of serious felonies. Currently, there are almost twice as many people on probation nationally as there are in prisons and jails combined. California's numbers mirror the nation's, with nearly 340,000 offenders on probation, as compared with an average monthly jail booking count of 98,025 (California Board of Corrections, 2002) and a state prison population of 160,901 (California Department of Corrections, 2002). What's more, the probation population has been increasing over time. Between 1995 and 2000, for example, California's probation population increased by nearly 20 percent (Criminal Justice Statistics Center, 2000).

While the severity of probation offenses and the numbers of probationers may have increased over time, so too has the range of probation programs or "treatment" approaches (e.g., McCarthy, 1987). For example, many probation departments provide intensive supervision of offenders and make available social services such as drug treatment, job training, and counseling programs (e.g., Petersilia & Turner, 1993). At the other extreme, many jurisdictions have even implemented "banked" probation caseloads, where low-level offenders are tracked by computer or via mail, as opposed to more traditional in-person meetings or visits with probation officers (Los Angeles County Planning Committee, 1996; Petersilia, 1995).

Probation is relied upon extensively because it offers a relatively cost-effective alternative to more traditional sanctions such as jail or prison terms. While it costs an average of US$26,690 to keep an

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inmate in jail (California Department of Corrections, 2002; see also Haynes & Larsen, 1984), probation costs substantially less. Camp and Camp (1995) reported that it cost approximately US$584 per probationer per year to maintain adequate supervision. Unfortunately, because of recent increases in probation caseloads (e.g., Guynes, 1988), whether probation is effective in terms of protecting public safety remains unknown. It is conceivable that increases in probation caseloads, which are invariably accompanied by less supervision, have consequences in terms of public safety.

The concern that probation caseloads may be linked to public safety is not isolated. Petersilia (1997, pp. 179–183) summarized several studies focusing on this particular issue. Not surprisingly, the evidence was somewhat inconclusive about whether high probation caseloads encouraged recidivism and thereby threatened public safety. A problem with the existing research, however, was that it was almost exclusively micro-level in orientation. The authors of this article were unable to identify a single macro-level analysis concerning the link between probation caseloads and public safety. This is a significant oversight.

Macro-level research is used extensively throughout criminology and elsewhere in criminal justice. It has the benefit of adding robustness to micro-level research findings. It also helps flesh out the relationships between important variables by moving from a lower level of analysis (such as individuals) to a higher level of analysis (such as on cities, counties, states, or nations). The authors of this article adopted a macro-level approach in order to fill this critical void in probation research.

Specifically, the research reported in this article sought to answer the following question: Are probation caseloads linked to overall crime rates? In pursuit of an answer, the authors relied on both a review of the existing literature on probation's effectiveness in this regard and an empirical analysis. First, the authors sought to determine what the current research suggested concerning the effectiveness of probation programs, with particular attention to public safety. Next, the authors conducted an empirical analysis of the effects of probation caseloads on public safety.

Research focus

Probation departments play a dual role in the criminal justice system. Probation officers are charged both with protecting public safety and rehabilitating offenders. As protectors of public safety, probation officers act as law enforcement officers, responsible for monitoring probationers' activities and ensuring that probationers comply with court-ordered conditions of probation (Latessa & Allen, 1997). In many states, probation officers are sworn peace officers and, as such, possess much the same authority as police officers. Of interest in this article is the public safety role played by probation officers.

In spite of increases in the probation population and caseloads, the recent changes in probation programs, and the heightened public and media attention to criminal justice sanctions in general, there was not a great deal of research concerning probation's link to public safety. Indeed, one of California's foremost probation experts recently commented that probation was the least studied component of the corrections system (Petersilia, 1998).

Studies examining the links between probation and public safety focused overwhelmingly on recidivism rates for individual probationers. That is, most of the extant research, which is reviewed in the next section, consisted of relatively localized studies of a sample of probationers and their offending and rearrest experiences during the probation period. Some of these studies attributed recidivism patterns to probation caseloads; others did not. To the authors' knowledge, however, not a single probation study focused on probation caseloads and *overall* crime rates. In other words, macro-level research on the links between probation caseloads and crime was all but non-existent.

Previous studies

Over the years, probation officers, policymakers, and some probation researchers argued that smaller caseloads would result in more contact between probation officers and probationers. Increasing this contact would in turn decrease the likelihood of recidivism by increasing the level of supervision and access to rehabilitative services (Carter, Robinson, & Wilkins, 1967; Carter & Wilkins, 1984). To date, however, researchers were unable to decisively prove whether smaller caseloads resulted in improvements in public safety and rehabilitation of offenders. For example, Allen, Eskridge, Latessa, and Vito (1985) found that probationers "failed" between 16 and 55 percent of the time.

Of course, the term "failure" can have different meanings. Researchers who defined failure as reconvictions found that probation was quite effective (e.g., Sutherland, Cressey, & Luckenbill, 1992, p. 460). Probation looks more effective still for reincarcerations. If, by contrast, arrests or technical violations are used as measures of probation's effectiveness, the picture is somewhat more disheartening. For example, RAND found that approximately two-thirds of

probationers are rearrested during their probation term (Petersilia, 1985a, 1985b; Petersilia, Turner, Kahan, & Peterson, 1985).

Studies by Langan and Cunniff (1992) and Whitehead (1991), however, reported that less than 50 percent of probationers were rearrested (see Geerken & Hayes, 1993, p. 555 for an impressive pre-1993 summary of probationer failure rates). Indeed, Geerken and Hayes (1993) found that when focusing on the percentage of crime committed by persons on probation (instead of arrests, convictions, etc.), probation appeared surprisingly effective. In their words, "...the complete elimination of probation and parole would have a very negligible effect on the burglary and armed robbery rates since more than 90 percent of all burglaries and armed robberies were committed by persons not on probation or parole at the time of their arrest" (p. 557).

The link between caseloads and crime

Studies seeking to link caseload size and recidivism date back more than thirty years. One such study, dubbed the "San Francisco Project," was conducted in 1967 (Carter et al., 1967). In this study, federal probation authorities designated offenders into one of four supervision levels: "ideal" (caseloads of forty to fifty offenders per officer), "normal" (caseloads of seventy to 130), "intensive" (caseloads of twenty to twenty-five); and "minimum" (caseloads of several hundred). At the end of two years, the study revealed that there were no significant differences in the number of violations among the probationers placed in the minimum, normal, and ideal caseloads. Each group had violation rates of approximately 23 percent. Those probationers in the intensive caseload, by contrast, had a violation rate in an unexpected direction. This group had a higher violation rate of 38 percent. When technical violations were removed from the analysis, however, there were no significant differences between the four caseload types.

Ten years later, in 1977, researchers were still unable to ascertain with certainty whether probation caseloads were in fact effective in reducing recidivism (Banks, Porter, Rardin, Silver, & Unger, 1977). After reviewing the previous work on probation, the researchers concluded that "the studies reviewed contained such poor research designs, and such unclear operational definitions of key variables, that the effect of reduced caseloads on offender recidivism remains unknown" (p. 7).

Since the late 1970s, at least five major studies seeking to identify the impact of caseloads on recidivism were conducted. Among the five studies, three found evidence to support the contention that smaller caseloads were related to lower recidivism rates. Two, however, failed to decisively demonstrate that smaller caseloads were effective in reducing recidivism rates. An important element shared by these studies was a focus on intensive supervision probation (see Harland & Rosen, 1987 for an introduction to Intensive Supervision Program (ISP)). Among other objectives, they sought to determine whether the low caseloads characteristic of ISP programs were linked to recidivism.

For example, probation researchers Erwin and Bennett (1987; see also Erwin, 1986) published an evaluation of a Georgia Department of Corrections program which allowed offenders the option of being placed in an ISP, in lieu of a prison sentence. To determine program effectiveness, the researchers sampled ISP offenders, regular probationers, and prison releasees. After an eighteen-month follow-up period, the results revealed that ISP offenders committed fewer and less serious crimes than regular probationers and prison releasees, although they did commit more technical violations than regular probationers.

The second study evaluated New Jersey's ISPs (Pearson & Harper, 1990). One facet of the study was to compare the recidivism rates between two groups: (1) ISP cases, and (2) a matched sample of about one hundred felony offenders who were sentenced for ISP-eligible crimes (prior to ISP implementation) and who were subsequently released on parole. The results revealed that 12 percent of the ISP offenders were convicted of a new crime compared to 23 percent of the offenders in the matched group. Pearson and Harper (1990) noted, however, that since the study lacked random assignment, it was difficult to determine if the results were due to participation in the ISP program.

In 1989, Byrne and Kelly conducted an evaluation of Massachusetts' ISP (Byrne & Kelly, 1989; see also Byrne, Lurigio, & Baird, 1989). One major focus of the study was to examine and to compare recidivism rates between courts with ISPs and courts without ISPs both before and after the implementation of the program. The results revealed there were no overall differences in recidivism rates between the experimental and control courts. The authors of that study did find, however, that as the level of supervision increased, recidivism rates significantly decreased in both courts.

In 1986, RAND conducted a multi-site demonstration project for ISPs in California (Petersilia & Turner, 1990a). Three sites were selected: Contra Costa County, Ventura County, and Los Angeles County. Each site identified those offenders eligible to participate. Subsequently, RAND randomly assigned each offender into the ISP program or the control probation program. The study revealed that at

the end of the one-year follow-up period, about 40 percent of the ISP offenders in each site had technical violations and approximately one-third had new arrests. In this vein, the only significant difference between the experimental and control programs was in Ventura County. The offenders in the ISP program were less likely to be arrested than the offenders in the control program. When the average number of arrests per year of street time was used, however, there was no significant difference between these two groups.[1]

Cunniff and Shilton (1991) conducted a study on various issues related to felony probation. One focus of this study was to examine caseloads and case outcomes. The study consisted of over 12,000 cases in thirty-two large metropolitan and suburban jurisdictions. Caseloads were associated with supervision levels–higher supervision levels were indicative of smaller caseloads. The results revealed that offenders who were supervised by probation officers with caseloads of more than 150 had the highest absconding rates. There were no consistent variations, however, between those probationers who were supervised by probation officers with caseloads below 150. The researchers noted that 12 percent of the probationers who were supervised by probation officers whose caseloads ranged from fifty-one to one hundred absconded. In contrast, the absconding rate was actually lower for those who were supervised by probation officers whose caseload was between 101 and 150 (4 percent). Lower caseloads by themselves would appear to have no direct impact on lowering the absconding rate (p. 64).

What is the verdict from this body of research? After more than three decades of study, the effectiveness of lower caseloads in reducing recidivism remains uncertain. While the majority of the evidence supports the contention that more probation supervision results in lower rates of recidivism, the fact that several key studies failed to find such a connection cannot be ignored.

This article continues in the footsteps of the studies just cited by beginning to explore the relationship between probation caseloads and the overall crime rate, not just recidivism rates among a sample of probationers. The analysis reported here differed considerably from that conducted in the past. First, the analysis moved from the micro- to macro-level, focusing on countywide probation caseloads. This new focus permitted a fairly sophisticated modeling strategy because, in California, demographic and economic data are available for all counties in yearly increments. The authors incorporated such data into the analysis to isolate the effect of probation caseloads on crime rates after controlling for other factors known to affect the crime rate.

Data and methods

Rather than look at recidivism rates for individual offenders as previous research had done, the authors examined the link between probation caseloads and public safety (the latter being measured by the overall property crime rate). Although there are myriad factors that influence the crime rate, one would nonetheless expect to find a relationship between probation programs and crime rates, if probation is effective in protecting public safety.

Clearly the property crime rate is not a perfect measure of public safety. Public safety is a complex concept that consists of multiple dimensions, only one of which is, or should be, the crime rate (e.g., Greene, Collins, & Kane, 2000). Individuals' perceptions of neighborhood problems and safety are relevant factors as well. It is therefore acknowledged that the measure of public safety used in the analysis reported here is not perfect. The authors encourage future researchers to explore the effects of probation caseloads on other dimensions of public safety besides the property crime rate.

Variables

There are so many factors that influence the crime rate, thus it was necessary to develop an analytical method for determining the influence of probation programs on crime, while controlling for these other factors. Accordingly, a regression model was developed in order to examine the influence of probation caseloads on the crime rate while controlling for other factors known to influence crime rates.

As the first step in developing the regression model, the authors reviewed the previous research on the causes of crime to determine which variables—other than the level of probation services—were thought to influence the crime rate. Numerous studies examined the link between crime rates and various social and economic factors, such as demographic characteristics of the population, economic conditions, and local law enforcement activities. Based on these earlier studies, a regression model was estimated to explore the impact of probation services on crime rates, while controlling for these other factors.

The dependent variable was the property crime rate itself.[2] The property crime rate was calculated as the number of property crimes reported to law enforcement agencies divided by county population. The analysis focused on property crime instead of violent crime because people who were placed on probation were generally less serious offenders and, therefore, were more likely to commit property rather that violent crimes. Another reason for this focus was that,

in California, *violent* crime rates fluctuated wildly from county to county; of the fifty-eight counties in California, more than half had extremely low violent crime rates relatively to property crime rates.[3]

The key explanatory variable was the average probation caseload, calculated as the number of adult and juvenile offenders on probation divided by the number of probation officers in the county.[4] In addition to the key explanatory variable, the authors also included six variables that, based on previous research, were thought to be associated with the property crime rate. These control variables can be placed into two different categories: (1) deterrent variables, and (2) socioeconomic variables.

The deterrent variables were the property crime clearance rate (defined as the number of crimes "cleared" by arrest divided by county population),[5] per capita law enforcement expenditures for each county (including both city and county expenditures for law enforcement), and the local jail population per capita. Each of these variables was expected to be inversely associated with the property crime rate (i.e., as enforcement activities increase, crime decreases).[6] An inverse relationship was expected because these three factors—law enforcement's effectiveness, law enforcement spending, and people held in custody—were thought to have a deterrent effect on crime (e.g., Marvell & Moody, 1996; Shepherd, 2002).

The socioeconomic variables included in the model, all of which had a basis in macro-level criminological theory, were as follows: (1) the percentage of males between the ages of thirteen and twenty-five; (2) the unemployment rate; (3) the per capita welfare rate, calculated as the number of families receiving welfare divided by county population (as a proxy for the extent of poverty in each county); and (4) per capita personal income (also lagged by one year).[7] Based on previous research, it was likely that a high percentage of young males was associated with a rise in the crime rate (Hirschi & Gottfredson, 1983; Jolin & Gibbons, 1987; Steffensmeier & Streifel, 1991), as were unemployment and poverty levels (e.g., Grant & Martinez, 1997; Land, McCall, & Cohen, 1990). In a similar vein, it was likely that higher incomes were associated with lower crime rates (e.g., Sampson, 1987).[8]

The data used for this project were provided by a range of government sources, including the state attorney general's office, the state controller's office, and the department of finance. Data were gathered for all fifty-eight counties in California for the years 1990–98. This yielded a total of 522 observations with which to estimate the statistical model (fifty-eight counties multiplied by nine years).[9] Thus, each observation in the data set was one of fifty-eight counties in one of the nine years under study.

In defense of a macro-level analysis

As indicated, most research into the effectiveness of probation was micro-level in nature. That is, it used individual probationers as the units of analysis. The authors of this article were unable to locate any macro-level studies concerning the effects of probation caseloads on crime rates. This was a strange shortcoming in the literature because macro-level research found something of a home elsewhere throughout the criminal justice literature. Countless macro-level criminological studies could be identified as well.

For example, the policing literature contained a wealth of macro-level studies focusing on the relationship between the police presence and crime rates. Recent studies showed that the ratio of police officers to citizens had an inverse effect on the crime rate (e.g., Kovandzic, Sloan, & Vieraitis, 2002; Marvell & Moody, 1996). As many, if not more, studies showed that police levels and the crime rate operated independently of one another (e.g., Loftin & McDowall, 1982). Cameron (1988) found that of twenty-two studies reviewed, only four found an inverse relationship between police levels and crime.

It was not difficult to analogize the foregoing studies (and others like them) to the probation-crime connection. In this article, however, the focus is on the ratio of probation officers to probationers instead of to some more abstract measure such as population. It was perplexing that probation researchers ignored possible macro-level relationships between probation caseloads and crime.

Not only does a macro-level analysis add another "layer" of information to the topic and, perhaps, increase the robustness of what some researchers found, but it also improves generalizability. Most research into probation's effectiveness looked at relatively small samples of probationers. Analysis on a larger scale may help to understand the probation-crime connection across multiple jurisdictions.

In defense of counties as the units of analysis

There is some debate among macro-level researchers concerning the appropriate units of analysis. Some favor neighborhoods, others cities, and still others counties, states, and even nations. In the end, the appropriate level of analysis should boil down to researchers' goals. Counties were the appropriate units for the analysis reported here, for three reasons.

First, by selecting counties as the unit of analysis, this article extends what is already a long history of county-level studies of criminal justice and criminology topics (see, e.g., Baller, Anselin, Messner, Deane,

& Hawkins, 2001; Gillis, 1996; Guthrie, 1995; Hannon & Defronzo, 1998; Kowalski & Duffield, 1990; Kposowa & Breault, 1993; Kposowa, Breault, & Hamilton, 1995; Petee & Kowalski, 1993; Phillips & Votey, 1975).

Second, data for the control variables included in the analysis were only available at the county-level. Census data permitted lower levels of analysis, but because this study contained data collected in yearly increments (as opposed to every ten years as in the census), the authors were forced to "move up" to the county-level. Some researchers used a technique called "linear interpolation" to "fill in" the annual gaps in census data (e.g., Kovandzic et al., 2002), but the authors of this article did not favor such an approach.

Third, and most important, probation in California is a county-level function. That is, probation officers are employees of counties. Thus, to promote consistency, the authors felt that counties were more appropriate than lower levels of analysis. It is likely that there is little probation "variation" from city to city, which would complicate statistical analysis. Counties as the units of analysis help ensure, in the present context, that there is sufficient variation from one jurisdiction to the next.

Estimation technique

A dynamic two-way fixed effects regression model was estimated. A dynamic two-way fixed effects regression model is simply an extension of ordinary least squares (OLS) regression. It extends the simple OLS equation

$$y = \alpha + \beta \mathrm{x} + \varepsilon, \tag{1}$$

to

$$y_{\mathrm{i,t}} = \varphi y_{\mathrm{i,t-1}} + \delta_{\mathrm{i}} + \gamma_{\mathrm{t}} + \beta \mathrm{x}_{\mathrm{i,t}} + \varepsilon_{\mathrm{i,t}} \tag{2}$$

where the subscripts i and t refer to unit (county) i and time t. The coefficient φ represents the lagged dependent variable. Note that the subscript t−1 refers to the fact that values on y are substituted with y values from the previous time period. The coefficient δ is a unit-specific effect represented by fifty-seven dummy variables for each individual county (fifty-eight counties minus one). The coefficient γ is a time-specific effect represented by eight dummy variables for time (nine years minus one). Finally, β and ε have all the usual properties. The intercept, α, in Equation 1 is not estimated in Equation 2 because the dummy variables for each county and year assign specific intercepts to each unit and time period. With the exception of the lagged dependent variable, Equation 2 is commonly referred to as the least squared dummy variable (LSDV) model.

Why separate dummy variables for unit and time? The act of including dummy variables for unit and time controls for unobserved heterogeneity. In the case of the units (counties), for example, the addition of separate dummy variables for each county removes all between-individual difference in y, leaving only within-individual variation to be explained by the x variables. The addition of dummy variables for each time period then controls for within-individual variation over time, so the remaining coefficients represented by β in Equation 2 facilitate explanation of y regardless of time and location. That is to say, the inclusion of time and unit dummies ensures that changes in y are explained only by β. This assumes, of course, that the effects of β are the same across all counties and time periods.

A one-year lagged dependent variable (hence the term "dynamic") was added on the right-hand side of the regression equation to control for unobserved time-varying variables. This did nothing to improve the causal direction of the model; however, it helped the authors to be assured that they were controlling (albeit indirectly and imperfectly) for variables not specified in the model. The lagged dependent variable also helps to control for serial autocorrelation, a problem frequently arising when data are collected on the same units over time. Finally, the regressions were weighted by the square root of population in order to control for heteroskedasticity.

An alternative approach to Equation 2 is the so-called random effects model. The simple random effects model, not taking the time dimension into account, looks something like

$$y_{\mathrm{i,t}} = \alpha + \beta \mathrm{x}_{\mathrm{i,t}} + u_{\mathrm{i}} + w_{\mathrm{i}} + \varepsilon_{\mathrm{i,t}} \tag{3}$$

where specific error terms are estimated for both unit and time. This model assumes that unobserved differences between units and time are random variables, compared with the assumption included in Equation 2 that they are fixed. An important assumption underlying Equation 3 is that that the u_{i} and w_{i} are uncorrelated with the x variables. This is an assumption the authors were unwilling to make. There are steps to overcome this assumption, but they are not perfect by any means.

The authors therefore settled on the two-way fixed effects regression model. There were two additional reasons for selecting fixed effects. First, there were no time-invariant (i.e., dummy) variables in the model. A fixed effects model cannot be estimated with a dummy variable because of perfect collinearity between the county dummies and any other dummy included in the specification.

Second, the authors chose the fixed effects model over a random effects model because a random

effects model assumes the model is correctly specified (i.e., that it contains no omitted variables). The authors did not feel comfortable with this assumption. Indeed, a Hausman chi-square test for random coefficients can be estimated in order to determine which is appropriate, random or fixed effects (Greene, 1993; Hsiao, 1986). It is a test of the overall difference between the coefficients in the random and fixed effects model. If the coefficients differ significantly between models, specification errors are likely and the fixed effects approach should be chosen. The Hausman statistic was highly significant, which led the authors to conclude that the fixed effects model was more appropriate.

Results

The results of the statistical analysis indicated that probation caseloads and crime rates were associated with one another. As hypothesized, the variable probation caseload was positively and significantly associated with the property crime rate. In other words, as probation caseloads increased, crime appeared to increase as well. The results of the statistical analysis are presented in Table 2 (first, however, summary statistics are presented in Table 1).

The findings lend support to certain previous studies finding a link between probation caseloads and crime (e.g., Cunniff & Shilton, 1991; Erwin, 1986; Erwin & Bennett, 1987; Pearson & Harper, 1990). To be fair, though, some past research also suggested that caseload differences did not affect probationer recidivism (e.g., Banks et al., 1977; Carter et al., 1967). The focus of all past research, however, was on individual probationers—with particular reference to intensive supervision probation. No previous studies examined the probation caseloads-crime connection at the macro level, as this article has. Thus, it is risky to pit the results presented here against previous research in this area. The authors believe that further macro-level analysis is necessary before meaningful comparisons can be made.

The results also suggested that, in addition to probation, other law enforcement activities were also effective in reducing crime.[10] Per capita law enforcement expenditures were negatively associated with crime rates, indicating that as total law enforcement expenditures increased, crime decreased. Similarly, the property crime clearance rate was negatively associated with the crime rate, so that as crimes were cleared by arrest, the crime rate went down. Finally, the jail population per capita was negatively associated with the crime rate; this indicated that as the jail population increased, crime decreased, although the coefficient on jail population was not statistically significant. These findings were consistent with past research (e.g., Marvell & Moody, 1996; Shepherd, 2002).

Table 1
Summary statistics

Variables	Mean	SD	Min	Max
Dependent				
Property crime rate	.0439	.0172	.0137	.1706
Independent				
Probation caseload	60.3635	61.4910	0	834.5
Per capita law enforcement exp.	.1548	.0803	.0429	.7855
Property crime clearance rate	.3946	.2958	.1005	3.7770
Jail population per capita	.0024	.0009	1.00e−06	.0079
Unemployment rate	.0944	.0460	.0013	.2990
Percent males from 13 to 25	.0959	.0163	.0603	.1627
Welfare recipients per capita	.3063	1.2122	.0007	11.2768
Income per capita	21.7791	14.1161	5.0436	340.3475

Notes: Summary statistics are for untransformed variables. Table does not include summary statistics for lagged dependent variable. Income per capita is reported in thousands of dollars.

Table 2
Two-way fixed effects regression results

Variables	Coefficients	Standard Error	T-Statistic
Probation caseload	.0008	.0003	2.51 *
Per capita law enforcement exp.	− 1.7046	0.6342	− 2.50**
Property crime clearance rate	− .0078	.0017	− 2.69**
Jail population per capita	− 15.4665	11.3547	− 1.36
Percent unemployed (1 year lag)	.9441	.7916	1.19
Percent males from 13 to 25	4.4997	1.1127	4.04**
Welfare recipients per capita	.00002	.00001	1.59
Income per capita (1 year lag)	− 1.65 (e) − 6	6.11 e − 6	− .25
Log of crime rate (1 year lag)	.4475	.0420	10.65**

* Significant at the 95 percent level.
** Significant at the 99 percent level.

The social factors identified by previous research as influencing the crime rate were also generally confirmed by this research. The percentage of males between the ages of thirteen and twenty-five was found to be a positive, statistically significant predictor of the crime rate, indicating that as the number of young males increased, so did the crime rate (Hirschi & Gottfredson, 1983; Jolin & Gibbons, 1987; Steffensmeier & Streifel, 1991). The unemployment rate was also positively associated with the crime rate as was the number of welfare recipients per capita, although these coefficients were not significant in the model. Finally, per capita income was negatively associated with the crime rate, although it was not statistically significant.[11]

Next, the lagged dependent variable was highly significant. This was almost always the case in models such as that presented here; the crime rate in the previous year was highly correlated with the crime rate in the present year. As indicated earlier, this variable was included for two reasons: (1) to help control for omitted variables, and (2) to help control for autocorrelation, a problem that frequently presents itself in time-series—cross-section analysis.

Conclusion and future directions for research

Probation is among the most important components of the criminal justice system. More offenders are sentenced to probation than to any other sanction. Recently, probation caseloads have been growing even faster than jail or prison populations. Coincident with the increase in the use of probation have come a series of new probation programs, from specialized caseload programs for offenders convicted of particular types of crimes to cost-saving banked caseload programs in which offenders are subject to little or no direct supervision from probation officers.

Unfortunately, key questions about the efficacy of probation remain unanswered. What research was done largely focused on the link between supervision levels and recidivism. This previous research indicated that increased supervision (lower caseloads) was associated with reduced recidivism, although there was by no means a consensus among the researchers that such a relationship existed.

In response to the importance of probation and the relative lack of consensus on the effectiveness of probation programs, the authors of this article sought to determine if probation succeeded in one of its stated goals of protecting public safety. The crime rate was the most widely recognized measure of public safety (e.g., Jones, 1991; LeClair & Guarino-Ghezzi, 1991). Therefore, the authors sought to measure the link between the level of probation services and the crime rate—particularly the property crime rate. In a departure from previous research, the analysis moved from the micro-level to the macro-level, consistent with a large body of criminal justice and criminological research found outside the probation context.

The findings indicate that higher caseloads appear to be associated with an increase in the property crime rate. In addition, very high caseloads are likely associated with diminished access to other probation related services, such as drug treatment and job training services. Of course, it is not possible to conclude, given the aggregate nature of the data, that any particular probationers will be more likely to offend relative to others. Nevertheless, when controlling for many other factors known to affect crime rates, caseloads and crime are positively related to one another.

There were other limitations associated with the analysis. First, the analysis was cross-sectional. This did not put the authors in a position to show that increases in caseloads *caused* crime, only that both variables were associated with one another. Next, given certain data limitations, fewer than all possible control variables likely to influence the property crime rate were included in the analysis. Even so, the inclusion of unit and year dummies did control for some such factors that were stable over time, as did the lagged dependent variable. Finally, and perhaps most important, the measure of probation caseloads was imperfect. To lay the caseload-crime debate to rest, perhaps more attention to the definition of probation caseload is necessary (see Note 6).

Though the results take another step toward illustrating the consequences of increased probation caseloads, significant work remains. Researchers still have little information about the cost effectiveness of probation programs relative to other sanctions or rehabilitation programs. Additionally, research is still needed to determine the relative effectiveness of specialized caseload programs and the impact of large banked caseloads. The analysis necessarily (given the nature of the data) relied on average caseloads, but two counties with the same average caseload may have very different probation systems. Research addressing these types of programs would provide probation departments with the tools necessary to allocate resources effectively and improve public safety and more effectively rehabilitate offenders.

Future researchers may also choose to explore the relationship between "types" of probationers and crime. This has already occurred to a minor extent (e.g., Petersilia, 1997), but many additional avenues for research exist. The authors of this article have

implicitly assumed that all categories of probationers have the equal capacity to influence county-level crime rates. Obviously, certain probationers are higher risk than others, so future researchers may wish to study the influence of caseload on crime, controlling for specific types of probationers.

In closing, while it may be true that probation caseloads and crime rates are positively associated, it may be too simplistic to assume that more supervision is better supervision. Instead, one must also take into consideration the nature of the supervision as well as the classification of offenders, probation officers, and treatment programs (Clear & Dammer, 2000). This, the authors believe, is the main direction in which future probation researchers should channel their energies.

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Notes

1. Other publications stemming from RAND's probation research include Petersilia (1989, 1990), Petersilia and Turner (1990b, 1991), and Turner, Petersilia, and Deschenes (1992).

2. The authors transformed the property crime rate into the natural log because the property crime rate is highly skewed in the positive direction.

3. The authors realize that there are underreporting problems with using official crime statistics in an analysis such as this; however, research shows that the reporting rates do not change much over time (Bastian, 1993). Also, official statistics are a fairly good measure of serious crime, which is at focus here (Gove, Hughes, & Geerken, 1986).

4. The caseload measure was calculated as the total number of probationers divided by the total number of probation officers. It is important to note that using this measure of average caseloads can mask important differences across counties. For example, a county that relies extensively on specialized caseload or intensive supervision programs for high risk offenders and banked caseloads for all others could have the same average caseload as another county that simply uses more conventional supervision means. Similarly, the measure does not distinguish the types of services provided by counties. Two counties with the same ratio of probationers to probation officers may deploy resources very differently. Nevertheless, data limitations prevented the authors from using more specific date in their analysis.

5. This measure includes crime clearances for juveniles as well as adults.

6. Deterrent variables are not particularly common in most macro-level models of crime estimated by criminologists. Economists, however, generally include deterrent variables in many of their models (e.g., Cherry, 1999; Cornwell & Trumbull, 1994). The authors believe they are important to include because their independent variable of interest, probation caseload, is tied to county expenditures and apprehension rates.

7. Some of the data included in the model were collected on a fiscal year basis and some were collected on a calendar year basis. Fiscal year variables were law enforcement expenditures and number of welfare recipients. All other variables are collected on a calendar year basis. Population figures are as of January 1 of each year.

8. One independent variable conspicuously absent from the model is race. Usually, race (if measured as the proportion of Black and/or Hispanic people in the population) is positively associated with crime. The authors chose not to include race in the model because of the level of diversity in the state of California. In fact, in preliminary analysis, the race variables included in the models were consistently significant and in the negative direction. That is, as the percentage of Blacks and Hispanics increased, the crime rate declined. The exclusion of race from the model reported below had no significant impact on the other coefficients.

9. The authors restricted the model to counties with probation caseloads below 250. This excluded all observations more than two standard deviations from the mean. This cutoff point was somewhat arbitrary, but was based on the assumption that changes in very high caseloads probably had a negligible impact on the crime rate. The authors believed it was safe to assume, for example, that a caseload change from 40 to 60 was more likely to have an effect on crime than a change from, say, 250 to 270. The 250 cutoff point could also be viewed as a means for excluding "outliers" from the analysis. The authors also excluded observations with law enforcement expenditures more than two standard deviations from the mean on similar grounds.

10. The R-squared for this model was .9866. It was high because of the addition of dummy variables for unit and time.

11. Though income, unemployment, and welfare rates were not significant in the reported specification of the model, it was hypothesized that this was due to the high degree of collinearity among the variables, as well as the inclusion of dummy variables for each cross section and time series period. When models were run excluding one or more of these variables, the result was generally a significant coefficient on the other included variables. All three variables were left in the model as control variables.

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