American Law and Economics Review Advance Access published May 7, 2014

Indigent Defense Counsel, Attorney Quality, and Defendant Outcomes

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County governments typically provide legal defense services for the indigent through one of two methods: public defenders and assigned counsel. I measure differences in defendant outcomes between these two types of counsel, finding that assigned counsel generate significantly less favorable outcomes for defendants than public defenders. Since assigned counsel work involves attorneys selecting into it, outside labor market options could affect attorney selection decisions. With that in mind, I analyze how attorneys of different quality levels respond to exogenous changes in their respective outside options, finding a significant impact on the performance of assigned counsel relative to public defenders. (*JEL*: K14, J22)

1. Introduction

Ever since the Supreme Court's 1963 ruling in *Gideon v. Wainwright* required state and local governments to provide defense counsel for defendants who could not afford it, the question of how to provide these services has been important as a practical policy matter. In fact, in order to comply with this requirement, counties in the United States spend in excess of $1.5 billion on more than four million indigent defendants each year

American Law and Economics Review
doi:10.1093/aler/ahu003

(Farole and Langton, 2010).[1] While the systems of providing defense counsel for the indigent vary widely between states and even between counties within a state, the attorneys handling these cases can generally fall into two categories: public defenders and assigned counsel. Public defenders are typically salaried workers whose full-time job is representing the indigent. Assigned counsel are typically private attorneys who, subject to standards set within their county, can select onto a panel of attorneys available to represent indigent defendants. Unlike public defenders, assigned counsel are usually paid on a case-by-case basis, and handling indigent matters is not their full-time occupation. Since assigned counsel make some tradeoff between indigent defense work and working in the outside labor market, they are potentially more heavily influenced than public defenders by changes in the attorney labor market. This paper focuses first on measuring differences in outcomes between public defenders and assigned counsel and then analyzes the mechanism through which outside labor market options affect the relative quality of these two groups.

To assess the differences in outcomes between public defenders and assigned counsel, I use data on felony prosecutions in large counties to estimate the baseline differences in case outcomes that exist between these groups. The second major piece of the analysis—testing the extent to which exogenous changes in labor market conditions affect the outcome gaps—involves assuming one point in the attorney wage distribution represents the outside option of a low quality type of attorney and a second point in the attorney wage distribution represents the outside option of a high quality type. Using a multi-county data set and data on the composition of an assigned counsel panel from a single county (Franklin County, OH), I test whether changes in these outside options affect (i) the outcome gaps between the two types of court-appointed attorneys and (ii) the composition of the assigned counsel panel itself. I show that in state courts, assigned counsel generate significantly less favorable defendant outcomes than public defenders. These differences are sensitive to variation in outside options in a way that is consistent with attorneys of varying quality levels responding to these changing incentives. These findings indicate that local labor

1. For the sake of comparison, this is larger than the combined 2010 budget requests from the Federal Trade Commission (2010) ($287 million) and the U.S. Securities and Exchange Commission (2009) ($1.026 billion).

market conditions are relevant considerations for those making policies related to assigned counsel systems.

The rest of the paper is organized as follows: in Section 2, I provide background relevant to the analysis, explaining the institutional details related to the public defender and assigned counsel systems and framing the literature into which this analysis fits. In Section 3, I describe the data. In Section 4, I discuss identification and the general empirical strategy of the analysis. The models and results are presented in Section 5. Section 6 concludes.

2. Background

2.1. Institutional Details of Indigent Defense Systems

In the United States, defendants who can show that they cannot afford a private attorney are provided one by the government. In many cases, this attorney comes from the state or county public defender office.[2] These attorneys exclusively handle indigent matters, and they are typically paid a salary rather than at an hourly rate. When a public defender is not used, some form of assigned counsel is employed. Assigned counsel are private attorneys who choose to supplement their other work by representing indigent defendants. There is almost always some minimum level of experience and qualification that attorneys must possess in order to serve as an assigned counsel, and that minimum level usually increases with the severity of the offense. This minimum level generally varies by county. Assigned counsel are generally paid by the local government on a case-by-case basis after their services are rendered.[3] The methods of compensation for assigned counsel differ by jurisdiction,[4] as do the actual rates paid to these attorneys. For

2. This includes some organizations, like the Legal Aid Society in New York City, that contract with the local government to provide indigent defense services, since lawyers for these organizations are full-time employees.

3. A third type of defense counsel for the indigent is attorneys doing pro bono work (private attorneys doing this type of defense work free of charge). While I could not obtain precise figures on the extent to which felony indigent matters are handled pro bono (from the American Bar Association or elsewhere), conversations with people who handle these cases lead me to believe it is a small proportion of cases, and thus I ignore it.

4. These methods fall into a number of categories: reasonable compensation (county or local judge determines rate at which assigned counsel are paid), statewide hourly rate (either set by state statute, administrative rule, or court rule), flat fee (either per case or under some contractual arrangement), annual contract, rates recommended

instance, in 2002, New Jersey paid $25/h out-of-court and $30/h in-court while Indiana paid $60/h for both in-court and out-of-court (The Spangenberg Group, 2003).[5]

This analysis focuses on counties that use both public defenders and assigned counsel to represent indigent defendants, and so the institutional details surrounding the mechanism by which cases are assigned between these groups is important, particularly as it relates to measuring differences in outcomes between these groups. These details inform the plausibility of assuming random assignment between the two groups as well as any caveats to the results. While these assignment mechanisms certainly differ between counties in some respects, a county's mechanism for assigning cases between these two groups has several important features: how the set of attorneys eligible for assigned counsel work is determined, rules for when assigned counsel are to be used, the rules by which that assignment decision is made, and the entity that actually assigns counsel to a defendant. The set of eligible attorneys is most often determined by way of an application and approval process, where individual attorneys submit applications to be eligible for this work and, once approved by the appropriate committee, are made eligible for specific types of cases based on their experience. Examples of such procedures are in Dade County, FL (where the Private Court Appointed Counsel Screening Committee reviews applications)[6] and Hamilton County, OH (where the Hamilton County Public Defender Commission reviews applications).[7] The rules for when this set of available assigned counsel are used differ by jurisdiction, but in the counties considered here, it typically involves conflicts of interest cases and instances where public defender workloads are high. An example is spelled out in the

by a non-binding commission on indigent defense, or some combination of these (The Spangenberg Group, 1999).

5. For the sake of comparison with outside labor market options, The consumer price index (CPI)-adjusted out-of-court hourly rate for assigned counsel in the subsample for which I have data on those rates varies between 13.4 and 39.4. The average tenth-percentile hourly wage for all attorney work is 23.8 in the subset of data I primarily use, and the average median hourly wage is 43.4 in that same subset.

6. The role of this committee is made clear in materials from the Eleventh Judicial Circuit of Florida's website: http://www.jud11.flcourts.org/docs/APPLICATION%20FOR%20SCREENING%20COMMITTEE%20fin.pdf.

7. The attorney registration form can be found on the webpage for Hamilton County Public Defender: http://www.hamiltoncountypd.org/uploads/Registration%20form%20Rev.%208-30-12.pdf.

procedures in Pima County, AZ, which calls for the use of assigned counsel where conflicts exist, workload limits have been reached, or the accused has an open case with the assigned counsel (Pima County Office of Court Appointed Counsel, 2005). When the conflict of interest centers on the fact that there are multiple defendants facing related charges, this means the first case filed will typically be handled by the public defender, with subsequent cases going to assigned counsel (unless there is an additional conflict). The actual assignment rules typically involve a rotation among the attorneys eligible for a particular class of case, such as described in the King County, Washington Assigned Counsel Plan, where "cases will be assigned according to a rotation procedure, in order to maintain an equitable and consistent basis of case assignment" (King County Office of the Public Defender Assigned Counsel Panel, 2008). This often involves either an attorney being assigned directly from a list to a particular case (as in King County) or an attorney being assigned a particular calendar day and handling the cases that originate on that day, as is done in San Francisco (San Francisco Superior Court, 2010). The assignment itself is done either by judges themselves, as can be done in Harris County, TX (Harris County District Courts Trying Criminal Cases, 2011), or by a separate entity within the court system (such as the Lawyer Referral and Information Service Conflicts Panel in San Francisco, CA, or the Office of the Public Defender's Adult Case Coordinator in King County, Washington). Virginia Code § 19.2-159 summarizes these relatively common institutional features:

> Except in jurisdictions having a public defender, or unless (i) the public defender is unable to represent the defendant by reason of conflict of interest or (ii) the court finds that appointment of other counsel is necessary to attain the ends of justice, counsel appointed by the court for representation of the accused shall be selected by a fair system of rotation among members of the bar practicing before the court whose names are on the list maintained by the Indigent Defense Commission.

I revisit the implications of these assignment mechanisms, specifically how they relate to comparing public defender case outcomes with assigned counsel case outcomes, in Section 4.1.

While public defender offices may not operate in the exact same way in every county, they tend to have a more standardized structure than assigned counsel programs. The key feature that allows me to group public defenders

together is the fact that there is no selection decision in a given period (since defending the indigent is a public defender's full-time job, I am implicitly assuming that if a person were a public defender yesterday, that person is a public defender today).[8] This leaves public defenders less susceptible to selection decisions based on changing labor market conditions. With that in mind, I consider changes in the outside options of attorneys in the context of how they affect attorneys' decisions to select into assigned counsel work (the value of which stays fixed as outside options change) and the case outcomes of assigned counsel. Their effects on assigned counsel decisions determine their overall effect on the gap between public defenders and assigned counsel.

2.2. Literature Review

In general terms, this work is certainly related to the literature on the labor supply choices of workers. Worker responses to variation in implied wage rates has been analyzed in a number of settings, from stadium vending (Oettinger, 1999) to driving a taxicab (Camerer et al., 1997) to fruit packing (Chang and Gross, 2014). Since this analysis is concerned with those labor market supply choices for workers of varying quality levels, it is connected to the efficiency wage literature, where a key explanation for firms paying wages above the market clearing level is that it allows them to attract a higher quality labor force (Katz, 1986). A number of analyses within the efficiency wage literature address this labor supply issue in a way that explicitly accounts for the value of a worker's outside work option (and possible variation in it). This is particularly important here because in the context of the decision to perform assigned counsel work, the compensation rates for assigned counsel in a given county often remain unchanged for many years, but the outside options for attorneys can vary substantially over that time period. Several studies have shown that relatively high outside options reduce worker quality and performance within industries. Cappelli and Chauvin (1991) show these effects in automobile manufacturing, and

8. There is a selection decision made in becoming a public defender, but it was a decision made long before the observations of interest (the cases in the data), and therefore, I treat that selection decision as exogenously determined and take it as given for the purposes of the analysis. This is supported by the fact that the attrition rate for attorneys in public defender offices is $<1\%$ (Farole and Langton, 2010).

Propper and Van Reenen (2010) demonstrate such effects in the nursing industry.

With regard to attorneys in particular, there have been numerous studies that have empirically examined attorney characteristics and case outcomes, most of which come from the law literature. Some of the earlier papers in this vein of research faced constraints in the available data and, as such, their conclusions are at best suggestive.[9] More recently, Abrams and Yoon (2007) use the random case assignment of the Clark County Nevada Public Defender's Office in Nevada to make inferences about attorney ability and the magnitude of its effect on sentences. That paper uses data on observable attorney characteristics, such as experience level and race, in order to determine which ones explain case outcomes. Other paper compare outcomes across broad categories of attorneys different from those analyzed here. One such paper is Hartley et al. (2010), which compares public defender outcomes to private attorney outcomes and finds little difference between them.

There is relatively little in the economics literature examining the differences in outcomes resulting from different methods of indigent defense. While there has been a recent study examining the question in Taiwan (Huang et al., 2010),[10] Iyengar (2007) is the only instance in which this question is directly examined in the United States in a manner that accounts for the case assignment mechanism between types of indigent defense counsel.[11] Iyengar looks at federal court cases and examines the differences in outcomes between defendants represented by federal public defenders and

9. Nagel (1973) and Holden and Balkin (1985) are two such papers from this literature. In comparing private attorneys to court-appointed counsel, Nagel finds that private attorneys are superior at both having their clients bailed out and negotiating probation sentences for their clients, though the lack of attention to selection issues and control variables in this study call into question the credibility of the findings. Houlden and Balkin compare two particular types of assigned counsel, ad hoc counsel and contract attorneys, but the fact that the data include fewer than 400 total observations limits the value of this particular comparison.

10. Huang et al. find that public defenders and private attorneys contracted for indigent defense are equally effective in rough terms, with public defenders having higher conviction rates but shorter sentences conditional on conviction.

11. Cohen (2014), for instance, uses regression analysis to examine differences between the outcomes of private attorneys, public defenders and assigned counsel, finding assigned counsel outcomes to be worse than public defender and private attorney outcomes. This paper acknowledges that selection bias stemming from the fact that it

those represented by panel attorneys (i.e., assigned counsel compensated at a fixed hourly rate). She finds that defendants represented by panel attorneys have longer average sentences and are more likely to be found guilty. She also finds that, for a subset of the data for which she has data on individual attorneys, the outcome gap between public defenders and panel attorneys could be explained by those observables, including things such as years of experience and law school attended.

Just as in Abrams and Yoon (2007), these results of Iyengar (2007) indicate that observable attorney characteristics, like experience, are correlated with attorney quality in a way that can be traced through to case outcomes. With those connections in mind, this paper extends this research on indigent defense methods into state courts. Such an extension matters because there are many more indigent criminal cases in state court than in federal court (for the sake of comparison, there were over four million cases involving indigent defendants in state courts in 1999 (DeFrances and Litras, 2000),[12] as compared to 68,000 federal representations of indigent defendants in 1998 (Harlow, 2000).[13] Because of these differences in case volume, the presence of persistent, systemic issues causing disparate defendant outcomes in state courts can affect many more individuals.

3. Data

The main data set I use is "State Court Processing Statistics, 1990–2004: Felony Defendants in Large Urban Counties." This data set was compiled by the Bureau of Justice Statistics and contains samples of data at the case level from a set of large counties in the United States. The observations in the data are cases in which the defendant is initially charged with a felony.[14] The data contain a sample of felony cases originating in the month of May

does not account for the case assignment mechanism is an issue that makes it difficult to infer causation.

12. This count of four million cases is based on the one hundred most populous counties in the United States, so it understates national total. Most of these four million cases were felonies.

13. Like the state cases, these federal cases are predominantly felonies.

14. The charged offense in the data is the initial felony charge and does not include amendments made during the legal process.

in even-numbered years beginning in 1990 and ending in 2004.[15] In each county, the sample includes all felony cases filed on a set of either 5, 10, or 20 business days within the month of May. In each year data are collected, forty counties are sampled, but since the same forty counties are not sampled each year, a total of sixty-five counties appear in the data. Cases are monitored through the disposition of the case or May 31 of the following year, whichever comes first.[16]

The data set details the progression of cases through the various phases of the legal process. This includes information on the offense with which the defendant was charged, the type of attorney representing the defendant (there is no other information on attorney characteristics, such as years of experience or law school attended, in this data set), bail and other pretrial information, plea information, adjudication outcome, and sentencing information, as well as the demographic characteristics of the defendant (including information on prior arrests and convictions). This level of detail allows me to consider a number of different outcome variables in the hope of providing a more complete picture of the differences between the two systems. The four outcome variables I use are *guilty* (a binary variable equaling one if the defendant was convicted), *most serious* (a binary variable equaling one when the defendant was found guilty of the most serious charge), *sentence* (the sentence length in months), and *duration* (the time between arrest and adjudication in days). The first three dependent variables have a very clear interpretations as to how they affect the defendant (defendants prefer lower values), and while these measures are certainly correlated with one another to an extent, they address different dimensions of the case outcome that may tell a more complete story.[17] The last variable, *duration*, has clear implications for the efficiency of the court system (since cases with a longer duration are likely more resource-intensive from the perspective of

15. Data on attorney type are not collected in 1990, so for the purposes of this paper, the relevant observations in the data are from 1992 to 2004.

16. Over 93% of the cases identified as being handled by assigned counsel or public defenders involves the case being disposed by a guilty verdict, an acquittal, or the case being dismissed.

17. This has shown to be an important feature in papers such as Huang et al. (2010), which show that in Taiwan, the probability of conviction differs between groups comparable with the ones I consider, but the unconditional expected sentences are not significantly different.

the court system), though it can be seen as ambiguous from the defendant's perspective.[18]

Some relevant variables from the primary subset of data used in the paper (that is, those counties for which I do not reject the hypothesis that cases are randomly assigned between public defenders and assigned counsel) are summarized in Table 1. Relevant variables from the attorney wage distribution are also included in the table (the tenth-percentile and median attorney wages are the ones primarily used in the analysis as the outside options for low-quality types and high quality types, respectively; the twenty-fifth percentile wage is also used for the low quality type's outside option as a robustness check). The first panel summarizes the main subset of data used in the analysis.[19] The second panel summarizes a larger subset of the data of which I make use as a robustness check.[20]

Table 2 shows that in the primary subset of counties, public defenders handle 47% of the cases for which either a guilty or not guilty outcome is observed, and assigned counsel handle 12% of the cases. One key limitation of these data is the large number of cases for which attorney type (i.e., whether the attorney can be identified as a public defender, assigned counsel, or some other type of attorney) is missing.[21] Roughly, 25% of the

18. If the defendant is out on bail and a positive discount rate of time, then all things equal, that defendant would prefer the longer time span. If the defendant is in jail during the trial, maybe a speedy trial is preferable. It is also worth noting that attorneys certainly do not have complete control of the duration of the case, as the judge has final say as to when things are actually resolved. To the extent the presiding judge is willing to accommodate attorney requests affecting the timing of the cases resolution (e.g., asking for additional time to investigate a particular aspect of the case), defense attorneys can marginally affect this variable.

19. The main subset of data I use here removes observations where neither a public defender nor an assigned counsel represents the defendant, observations where I do not observe a guilty or non-guilty outcome, observations from counties where I reject the hypothesis that cases are randomly assigned between public defenders and assigned counsel, observations involving county-year-offense combinations where either assigned counsel or public defenders did not handle any cases, and all murder cases.

20. The only difference between this subset and the main subset is that it does not exclude observations involving county-year-offense combinations where either assigned counsel or public defenders did not handle any cases.

21. According to the BJS statisticians who work with these data, collecting information on attorney type from the case management systems of different courts has been difficult over the years, though it has improved over time.

Table 1. Summary Statistics for Multi-County Data Set

Variable	County-year-offense combinations with both assigned counsel and public defender cases in relevant counties	All county-year-offense combinations in relevant counties
Guilty case outcome (*guilty*)	0.793	0.779
Guilty plea	0.761	0.737
Conviction on most serious charge (*most serious*)	0.565	0.571
Sentence (in months) (*sentence*)	10.601	11.802
Case duration (in days) (*duration*)	93.626	97.138
Public defender	0.756	0.797
Assigned counsel	0.244	0.203
White defendant	0.400	0.376
Male defendant	0.833	0.829
Defendant with active criminal justice status	0.400	0.374
Number of prior felony convictions	1.354	1.407
Number of prior prison sentences	0.445	0.502
Tenth percentile attorney wage	13.389	13.413
Twenty-fifth percentile attorney wage	17.338	17.853
Median attorney wage	24.424	25.506
Random case assignment validated	1.000	1.000
Case has adjudication outcome	1.000	1.000
County-year-offense combination where both public defenders and assigned counsel handle cases	1.000	0.554
Year dummies:		
1992	0.098	0.114
1994	0.141	0.133
1996	0.194	0.162
1998	0.146	0.151

(*Continued*)

Table 1. Continued

Variable	County-year-offense combinations with both assigned counsel and public defender cases in relevant counties	All county-year-offense combinations in relevant counties
2000	0.101	0.135
2002	0.148	0.146
2004	0.172	0.158
Charge dummies:		
Rape	0.009	0.013
Robbery	0.067	0.059
Assault	0.143	0.115
Other violent crime	0.033	0.037
Burglary	0.106	0.098
Larceny/theft	0.082	0.096
Motor vehicle theft	0.033	0.041
Forgery	0.019	0.031
Fraud	0.021	0.026
Other property crime	0.038	0.044
Drug sale	0.205	0.166
Other drug crime	0.176	0.188
Weapons	0.021	0.028
Driving-related crime	0.025	0.027
Other public order	0.021	0.031

All figures were computed from Bureau of Justice Statistics data from "State Court Processing Statistic" files and Bureau of Labor Statistics data on wages. Excludes cases from counties where random assignment cannot be empirically validated, murder cases, and cases with no adjudication outcome. Some county-year-offense combinations are excluded in the first column because they have no cases handled by assigned counsel or no case cases handled by public defenders. Median, tenth percentile and twenty-fifth percentile attorney wages are all CPI-adjusted.

relevant cases from the sample have missing data on attorney type.[22] The extent to which this information is missing at random cannot be determined with certainty from the available information, though it should be noted that within the sample of cases for which attorney type is observed, the proportion of cases handled by public defenders and assigned counsel are very much in line with their respective levels nationally.[23] This suggests that

22. Cases with missing adjudication outcomes are even more likely to have missing data on attorney type, but they are excluded from the sample.

23. In 1996, 82% of felony defendants in state courts in large counties were represented by publicly finance attorneys (Harlow, 2000), and in 1999 in large counties, 82% of indigent cases were handled by public defenders (DeFrances and Litras, 2000). Therefore, the expectation is roughly 67% of cases handled by public defenders and 15%

Table 2. Percentages of Cases Handled by Various Attorney Types by Year in Multi-County Data Set

	Public defender (%)	Private attorney (%)	Assigned counsel (%)	Pro Se (%)	Other (%)	Missing (%)
1992	40.4	11.9	13.1	0.5	0.1	33.9
1994	43.0	14.2	13.1	0.4	0.2	29.1
1996	51.2	14.3	11.0	0.3	0.1	23.1
1998	42.6	15.0	12.4	0.4	0.0	29.6
2000	51.1	15.4	9.9	0.2	0.0	23.3
2002	50.8	15.8	12.5	0.5	0.2	20.3
2004	51.7	15.9	12.1	0.0	1.7	18.6
1992–2004	47.3	14.7	12.0	0.3	0.3	25.4

All figures were computed from Bureau of Justice Statistics data from "State Court Processing Statistics" files. Excludes cases from counties where random assignment cannot be empirically validated, murder cases, and cases with no adjudication outcome.

disproportionate numbers of missing data cases are not coming from either of these groups. I further address this missing data concern by running some specifications of the models on the subset of counties for which the percentage of cases with missing data on attorney types is <30%. The results of the analysis generally hold when this restriction is made, bolstering the assumption that the missing data are not a substantial factor in the results.

Table 3 summarizes the offense categories of the cases handled by each type of attorney, showing that the proportions of the four crime categories in the data (violent, property, drug, and public order crimes) are similar for assigned counsel and public defenders (though violent crimes make up a slightly larger percentage of assigned counsel cases).

To measure the outside options in the counties of interest, I utilize data from the Bureau of Labor Statistics (BLS) on various attorney wage percentiles in a given metropolitan statistical area (MSA) (since the counties for which I have data are large cities, it is a relatively simple task to link them to a given MSA). This information on attorney wages provides a useful estimate of the outside options for attorneys in a particular county in a given year, but it certainly has limitations. Since it includes all types of attorneys (rather than just criminal attorneys) and excludes self-employed

handled by assigned counsel. For the entire sample, the proportions are close to that (over 63% of observed attorney types are public defenders, and 16% are assigned counsel. In 1996, it is even closer (67 and 14%, respectively).

Table 3. Proportions of Indigent Cases Handled by Public Defenders and Assigned Counsel,Respectively, by Charge, in Multi-County Data Set

Type of crime	Charge	PD (%)	AC (%)
Violent	Rape	52.0	48.0
	Robbery	69.2	30.8
	Assault	74.1	25.9
	Other violent	72.1	27.9
	Total violent	71.7	28.3
Property	Burglary	78.8	21.2
	Larceny-theft	77.3	22.7
	Motor vehicle theft	74.0	26.0
	Forgery	71.9	28.1
	Fraud	75.4	24.6
	Other property	73.4	26.6
	Total property	76.5	23.5
Drug	Drug sales	74.5	25.5
	Other drug	80.4	19.6
	Total drug	77.2	22.8
Public order	Weapons	76.2	23.8
	Driving-related	81.1	18.9
	Other	74.3	25.7
	Total public order	77.5	22.5
All crime		75.6	24.4

All figures were computed from Bureau of Justice Statistics Data from "State Court Processing Statistics" files. Excludes cases from counties where random assignment cannot be empirically validated, murder cases, and cases with no adjudication outcome.

attorneys (this includes solo practitioners and partners in law firms[24]) from its calculations it is a noisy proxy for the outside options of the subset of attorneys who would be candidates for assigned counsel work.[25]

24. These two groups account for 26% of attorneys according to the BLS 2010–11. Occupational Outlook Handbook, and due to the nature of the work, the pool of assigned counsel attorneys is likely self-employed at a higher rate than the general population of attorneys.

25. A key aspect of the analysis of Franklin County, OH is to show that the data on outside options used in this analysis, though noisy, contains the relevant information that is driving attorney selection decisions (that is, these measures are sufficiently correlated with the true outside options of the relevant sets of attorneys, and changes in these outside options affect selection decisions). I do this by showing that the relative quality of the panel is significantly affected by changes in these measured outside options in a way consistent with economic theory.

Consumer price index data (using the 1982–84 base year) are also used to appropriately deflate wages.[26]

In order to analyze the attorney selection dynamics more carefully, I use data on the assigned counsel panel in Franklin County, OH. This data set includes the cases handled by individual attorneys in the years 2000 through 2008 and dates on which the attorneys were paid for the cases they handled. This information provides a picture of the continually changing composition of the assigned counsel panel over this time period. I have supplemented the Franklin County data with data have attorney registration data from the Ohio Supreme Court. Attorneys must register with the Supreme Court in order to practice in Ohio, and they must update their registration every 2 years. When they register, they provide information on their county of employment and law school attended, among other things. These records provide a series of snapshots of what the pool of attorneys in Ohio (or any subset of those attorneys) looks like at various points in time. It also allows me to match the attorneys on the assigned counsel roster to the law school they attended and the date on which they began practicing law in Ohio. Thus, it is possible to measure how attorney characteristics, such as years of experience, change (in the aggregate) over time, both in the assigned counsel panel within Franklin County and in Franklin County as a whole.[27] I also use *US News and World Report's Law School Rankings* from 2004 (since it is in the middle of the time period where I observe the panel) to measure the quality of law school attended, both within the panel and within Franklin County as a whole.

4. Identification

4.1. Outcome Gap

The key identifying assumption required to measure the outcome gap between public defenders and assigned counsel is that there are no

26. Because of this deflation, the estimated marginal effects related to one-dollar changes in hourly attorney wages actually correspond to a nominal wage changes that are greater than one dollar.

27. Since I do not have specific information on the outcomes these attorneys generate, I cannot directly link these attorney characteristics to the outcome of a particular case. I am rather simply looking at whether aggregate panel characteristics change in such a way that would be consistent with the theoretical prediction.

systematic unobserved differences between cases handled by public defenders and cases handled by assigned counsel. This means that, conditional on observables, the pool of assigned counsel cases does not systematically differ from the pool of public defender cases. This assumption is plausible when cases within a given category are randomly assigned between these two groups. By assuming case assignment is random, I can attribute observed differences in outcomes between public defenders and assigned counsel (i.e., the outcome gap) to the type of attorney handling the case.

In order to understand the plausibility of assuming random assignment, consider two of the primary reasons assigned counsel handle cases: case overflow and conflict of interest. If assigned counsel handle overflow cases (cases that come in when public defenders have full caseloads), assignment is purely a function of the timing of the charges within a given month in a particular county. Assignment is not based on an evaluation of the characteristics of a particular case or defendant. Therefore, the overflow cases likely mirror the non-overflow cases within a given category. If assigned counsel handle cases where the public defender office has a conflict of interest (such as a case with multiple defendants), rules exist to assign cases between groups, and the attorneys who are potentially handling the cases are not making those allocations. While certain parties might have some discretion in the assignment process (for instance, a judge or the case assignment coordinator[28]), if for some reason they attempt to select cases for assigned counsel that systematically differ from the cases going to public defenders, it is assumed that those systematic differences would likely have observable qualities, and the validation mechanism proposed below would identify those differences.

Since non-random assignment of cases introduces the possibility that selection bias is driving these differences (rather than any fundamental difference in the nature of the representation), I only use observations from counties for which this assumption of random assignment has been empirically validated. It is worth noting that non-random assignment could

28. You could even argue that a prosecutor or law enforcement officer could potentially attempt to affect the assignment by the timing of their own actions. The point still holds that such manipulation would likely be detected in the observable qualities of the cases going to the two types of attorneys, which is what I empirically validate. For instance, if law enforcement was trying to manipulate assignment, it might be based on the individual's criminal history, which is observed in the data.

conceivably bias the results in either direction (if more difficult cases are going to assigned counsel in greater proportion, then defendant outcomes from assigned counsel would be appear worse from the defendant's perspective relative to public defender outcomes, but if fewer difficult cases go to assigned counsel, the opposite is true). I assume that the random assignment hypothesis is not violated in a county if defendant characteristics such as race, sex, and criminal history do not make the assignment of an indigent defendant to an assigned counsel significant more or significantly less likely, conditional on the time period and offense. The implicit assumption here is that if observable defendant characteristics are not significant factors in the assignment decision, then unobserved characteristics are not significant either. Limiting the analysis to counties where I fail to reject this hypothesis is a form of validation similar to the one used in Iyengar (2007).[29]

This hypothesis might be rejected for a number of reasons. Obviously, the test would be rejected if the assignment mechanism sends systematically different cases to one type of counsel, basing this assignment on observable and unobservable characteristics of the defendant. This most certainly would bias the results of a comparison, and eliminating these counties from the analysis is the central purpose of the testing. Considering these institutional details, and having spoken with individuals in counties for which I have rejected the hypothesis, a more plausible explanation for rejecting the hypothesis is the presence of systematic observable differences between cases involving a conflict of interest and cases not involving a conflict of interest. Given that many conflict of interest cases involve multiple defendants (an example would be conspiracy cases, a category of cases for which Marcus (2002) documents this type of conflict), one might hypothesize that the characteristics of defendants committing crimes by themselves might differ from the characteristics of defendants committing crimes with others.

29. It should be noted that I cannot completely dismiss the possibility that selection on the basis of unobservable factors is a problem. Individuals involved in determining assignment could conceivably have additional information on the specifics of a case that could influence their assignment of the case. This is similar to concerns about medical treatment assignment decisions raised in Dranove et al. (2003). After considering the institutional context and testing for non-random selection on the basis of observables, I make the assumption that this is not an issue in the subset of the data I use in the analysis.

This could lead to a rejection of the random assignment hypothesis.[30] If the only differences between the cases handled by assigned counsel and public defenders were observable in this way, appropriately controlling for these observables would allow for a comparison between groups. However, if there are unobservable differences correlated with the observable differences that come about in this way, a comparison between groups is not appropriate. Since I cannot rule out that unobservable differences accompany these observable differences, I eliminate those counties with significant observable differences from the sample. After reducing the sample in this way, I can use regression models to estimate the outcome gap between the two groups.

4.2. Changes in the Outside Attorney Labor Market

In order to identify the extent to which outside labor market options affect the outcome gap between public defenders and assigned counsel, I focus on two points in the attorney wage distribution: the tenth percentile and the median. I assume that the tenth-percentile attorney wage represents the outside option of low quality attorneys in the pool of potential assigned counsel and that the median attorney wage represents the outside option of high quality attorneys in this pool. Since the value of performing assigned counsel work typically does not change over time in a given county, exogenous variation in these respective outside options can impact attorney selection decisions, thus affecting the relative quality of an assigned counsel panel. Economic theory would predict that, all else equal, an increase in the low quality assigned counsel's outside option results in fewer of these attorneys choosing assigned counsel work. This theoretically improves the

30. For instance, I reject the random assignment hypothesis in San Bernardino, CA, but having spoken with an attorney in their public defender office, their process has no features where the observable characteristics of the defendant do not enter into whether the case goes to assigned counsel or public defender (as in many counties, in the case of multiple defendants, the first case filed goes to the public defender, subsequent cases go to assigned counsel). Systematic observable differences between cases involving a conflict of interest and cases not involving a conflict of interest could explain this. The fact that these tests reject counties where there assignment decisions are not intentionally based on observable characteristics of the defendants bolsters the assumption that the tests reject counties where this is done intentionally (I assume that the observed differences in the cases of the two groups would be more pronounced if the assignment mechanism is intentionally based on such things).

overall quality of the assigned counsel panel, and thus I expect the outcomes for assigned counsel cases to improve (relative to public defender outcomes) and the outcome gap to narrow.[31] Through an analogous mechanism, I expect a ceteris paribus increase in the outside option of high quality assigned counsel to induce fewer of these attorneys to choose assigned counsel work, reducing the overall quality of the panel and causing the outcome gap to widen. I test the extent to which changes in the outside options affect the outcome gap in this way. The emphasis on two points in the attorney wage distribution is important, as it yields a testable prediction of how any changes should affect outcome variables. When heterogeneity of attorney quality is considered in this way, using a single value, like the mean wage (as is done in Iyengar, 2007), to summarize the outside options of attorneys does not yield a theoretical prediction of how the outcome gap will change. Specifically, if the mean rises due to changes in the high type's outside option, one would expect the outcome gap to rise. If the mean rises due to changes in the low type's outside option, one would expect the outcome gap to fall.

This framework implicitly assumes several things. One assumption is that any additional variation in points in the attorney wage distribution not accounted for by the control variables is exogenous (that is, uncorrelated with the relative quality differential between public defenders and assigned counsel in a given time and place). In order to support this assumption, I present some evidence in Section 5.4 indicating that the distribution of the true ability of attorneys in a given area for which I have data is roughly constant over time. This analysis also assumes that the outcomes generated by assigned counsel can be compared with the outcomes generated by public defenders in a way that is consistent across time. Limiting the analysis to cases from counties where random assignment has been empirically validated, as I do in most specifications, allows for this type of comparability. It is worth noting that, while this might lend credibility to the results, this analysis does not directly rely on random assignment in the same way that measuring the baseline outcome gaps does. It only requires that any systematic differences between assigned counsel cases and public defender cases be constant across time; it does not require that no such differences exist.

31. An implicit assumption here is the quality of representation from public defenders is roughly constant over time in a particular county.

More fundamentally, this analysis rests on the assumption that exogenous changes in these particular labor market conditions (the median and tenth-percentile wages) are in fact a driver of the decision to self-select onto the assigned counsel panel. The individual attorney data from Franklin County helps to address this issue and bolster that assumption. The assigned counsel payment records allow me to construct panel-level measures of attorney quality. I can then empirically test whether measures of assigned counsel quality are sensitive to exogenous changes in outside options and whether that response is consistent with the theory.

5. Models and Results

5.1. Case Assignment Randomization

I assume that unobservable differences do not exist between the public defender cases and assigned counsel cases in counties where no significant observable differences between these cases exist. Testing for observable differences involves regressing a dummy variable indicating whether or not an assigned counsel handled the case on characteristics that could be the basis of discrimination and a set of control variables. I run this regression separately for each county, using the set of cases within that county handled by either public defenders or assigned counsel:

$$\text{assigned counsel}_{ijot} = \beta_0 + \boldsymbol{\beta_1}(\mathbf{X_{ijot}}) + \boldsymbol{\theta_t} + \boldsymbol{\eta_0} + \boldsymbol{\varepsilon}_{ijot}. \qquad (1)$$

Here, and throughout the paper, "i" indexes the variable by defendant, "j" indexes the variable by type of attorney handling the case, "o" indexes the variable by offense category, "t" indexes the variable by year, and "c" indexes the variable by county (since the model specified in Equation (1) is run separately for each county, none of the variables in this equation are indexed by county). The control variables, $\boldsymbol{\theta_t}$ and $\boldsymbol{\eta_0}$, are vectors of year fixed effects and offense fixed effects, respectively. The case characteristics comprising $\mathbf{X}$ include a dummy variable indicating whether the defendant is white, a dummy variable indicating whether the defendant is male, and a dummy variable indicating whether the defendant has an active criminal justice status in the county (having an active criminal justice status means the defendant is already under some sort of supervision by the criminal justice

system, such as probation or pre-trial release). Two other components of an individual's observable criminal history, the number of felony convictions and the number of times an individual has been incarcerated, are also part of the test for random assignment and are including in **X**. I include separate dummy variables indicating whether a defendant has no prior felony convictions, a single prior felony conviction, or more than one prior felony conviction. Similarly, I include separate dummy variables indicating whether a defendant has no prior incarcerations, a single prior incarceration, or more than one prior incarceration. I test whether the coefficients in $\boldsymbol{\beta}_1$ are jointly significant (at the 5% level) in determining whether an indigent defendant is represented by an assigned counsel or not.[32] The counties for which I fail to reject random assignment account for over 60% of the indigent cases relevant to the analysis, and the results of the empirical tests are provided in Appendix A. It is worth noting that the F-test I use here tends to over-reject the null hypothesis of random assignment (Abrams et al., 2012), so there is an element of conservatism in using this test. Furthermore, the power of the tests in the counties where the null is rejected does not differ substantially from the counties where the null is not rejected, even those counties that have relatively few observations.[33] For the sake of being conservative, I conduct the majority of this analysis using only those counties for which I fail to reject this hypothesis (though I do use the all-county sample as a robustness check).[34]

5.2. Outcome Gap

Having identified a set of counties on which the issue can be addressed, I estimate the difference in outcomes between public defenders and assigned

32. In addition to being used in Iyengar (2007), similar empirical validation strategies using the observable characteristics of the units being randomly assigned can be found in Anderson et al. (1999) and Kling (2006).

33. All but two of these counties have denominator degrees of freedom well in excess of 120, and for any given non-centrality parameter, the power of a test with denominator degrees of freedom of 120 is within 0.03 of the power of a test with an arbitrarily large denominator degrees of freedom (see Cohen, 1988, p. 421). In other words, the power curves for counties with fewer observations are quite close to the power curves for counties with more observations.

34. I also eliminate counties for which collinearity issues reduced the numerator degrees of freedom of the F-test.

counsel. The four dependent variables considered (*guilty*, *most serious*, *sentence*, and *duration*) reflect different facets of the outcome of a particular case.[35] The basic strategy for measuring this outcome gap is linear regression analysis, conditioning on observables, specifically some accounting for the effects of the county, year, and offense related to a case. County effects are important to account for since laws and sentencing guidelines may differ across states, and both the likelihood of conviction and the level of criminal sanctions may be endogenous with regard to institutional details that vary by county. Similarly, sentence length and the difficulty of obtaining a conviction vary across offense categories, so controlling for the charged offense is necessary. Year effects may be important because the prevalence of certain crimes and the overall crime level may change over time. One can imagine scenarios where high crime levels induce stricter sentences (judges trying to deter future crime) or more lenient sentences (systematically overcrowded prisons).[36] The main specification of the regression is given below (y refers to the four outcome variables listed above that are each separately used as

35. The longer case duration could certainly be influenced by institutional factors over which the assigned counsel has no control. It may be due to the fact that there are more coordination problems involved in actually assigning a case to a given assigned counsel than there are in delegating a case within the public defender office. Also, if assigned counsel are being used for overflow cases, the court system itself may be crowded at that time, and so cases take longer to adjudicate (although public defenders at that time would have the same problem). Furthermore, if assigned counsel are being used in cases where there are multiple defendants, there may be coordination issues among the officers of the court involved in those cases that lengthen the legal process. Again, this may be beyond the control of a given assigned counsel. This concern is less of an issue in the section dealing with attorney responses to changes in outside options. In that section, I need only assume these institutional factors are consistent over time relative to the extent those factors affecting public defenders.

36. I am comparing outcomes from public defender cases originating in May of a particular year to outcomes for assigned counsel cases originating in that same month. These cases are not resolved at the same time, so to the extent that there are month or quarter-specific shocks in outcomes, sentence patterns may vary over time (e.g., more severe sentences may occur when judges are fatigued as suggested in Danziger et al., 2011, so such shocks may be related to a judge's caseload, which changes over time). I am implicitly assuming that if such a shock term exists, the average value of this shock is the same for assigned counsel cases originating in May (whenever they may be resolved) and public defender cases originating in May (whenever they may be resolved).

the dependent variable in these models):[37]

$$y_{\text{ijoct}} = \alpha + \beta_1(\text{assigned counsel}_j) + \boldsymbol{\beta}_2(\mathbf{X_{ijoct}}) + \boldsymbol{\chi}_{\mathbf{ct}} + \boldsymbol{\eta}_0 + \varepsilon_{\mathbf{ijoct}}. \quad (2)$$

The variables in the vector **X** are the same defendant characteristics used in the random assignment regressions. I control for these factors in all of the regressions in the analysis. I also include a fixed effect for each county-year combination ($\boldsymbol{\chi}_{\mathbf{ct}}$) and each offense ($\boldsymbol{\eta}_0$). It is important to control for county-year effects in light of the other findings of this paper, which show that local wage changes (a variable measured at the county-year level) affect the selection decisions of attorneys considering assigned counsel work.

In the main specification, I restrict the sample to all public defender and assigned counsel observations from counties where I fail to reject the random assignment hypothesis, excluding murder cases,[38] cases without an adjudication outcome, and observations corresponding to county-offense-year combinations for which either assigned counsel or public defenders handled no cases. This last restriction is done so as to remove the possibility that any measured outcome gap was generated by extrapolating from observations handled by only one type of counsel. Standard errors are clustered at the county level. I then vary some of these restrictions as a means of testing robustness. Robustness checks include clustering standard errors at the state level rather than the county level (specification 2 in Table 4), having county fixed effects, year fixed effects, and offense fixed effects each enter the model separately, so as to reduce the number of parameters to be estimated (specification 3 in Table 4), and eliminating counties with missing data on attorney type in at least 30% of cases (specification 7 in Table 4). Specification 7 is done to bolster the assumption that high levels of missing

37. It should be noted that in these (and all subsequent) regressions where *sentence* is the dependent variable, I limit attention to observations where *sentence* is <180 months (this eliminates <1% of observations). When *duration* is the dependent variable, I limit the analysis to observations where *duration* exists in the data. I also utilize the sampling weights given in the data.

38. The complexity and seriousness of these cases leads to counties often having special procedures for assigning counsel for indigent defendants and for compensating them. Since murders make up <1% of the adjudicated cases, this is a relatively minor restriction.

Table 4. Regression Results Showing Outcome Gap between Public Defenders and Assigned Counsel using Different Outcome Variables

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable: *guilty*			E[*guilty*] = 0.7926			
assigned counsel	0.0284	0.0284	0.0341*	0.0250	0.0153	0.0359
	(0.0176)	(0.0218)	(0.0200)	(0.0182)	(0.0158)	(0.0243)
n	16,619	16,619	16,619	30,732	45,054	12,462
Dependent variable: *most serious*			E[*most serious*] = 0.5657			
assigned counsel	0.0522***	0.0522*	0.0666***	0.0507***	0.0420**	0.0641***
	(0.0165)	(0.0258)	(0.0213)	(0.0177)	(0.0171)	(0.0231)
n	16,619	16,619	16,619	30,732	45,054	12,462
Dependent variable: *sentence*			E[*sentence*] = 10.4423			
assigned counsel	3.358***	3.358***	3.793***	3.091***	2.410***	3.563***
	(0.849)	(0.903)	(0.834)	(0.840)	(0.653)	(1.178)
n	16,410	16,410	16,410	30,217	44,370	12,303
Dependent variable: *duration*			E[*duration*] = 93.5109			
assigned counsel	27.03***	27.03***	27.73***	26.10***	22.33***	26.16***
	(5.301)	(5.668)	(5.319)	(5.169)	(3.925)	(6.868)
n	16,565	16,565	16,565	30,654	44,949	12,430

County dummies			X			
Year dummies			X			
Offense dummies	X	X	X	X	X	X
County-year dummies	X	X		X	X	X
Attorney type missing <30% of time						X
County-year-offense categories with assigned counsel and public defender cases	X	X	X			X
Only random assignment counties	X	X	X			X
Standard errors clustered by county	X		X	X	X	X
Standard errors clustered by state		X				

The variable guilty is a dummy variable equaling one when the defendant is convicted of a crime. The variable *most serious* is a dummy variable equaling one when the defendant is convicted of the most serious offense with which he or she was charged. The variable *sentence* is the length of time a defendant is set to be incarcerated, measured in months. The variable duration is the amount of time between arrest and adjudication in a case, measured in days. The variable *assigned counsel* is a dummy equaling one when the attorney handling the case is assigned counsel. In addition to controls described at the bottom of the table, each regression controls for defendant race, sex, status within the criminal justice system, prior convictions, and prior incarcerations. Murder cases and cases with no adjudication outcome are excluded from the analysis. Robust errors are in parentheses. Standard errors are clustered at the county level unless otherwise indicated. Significance levels: $^{*}P < 0.1$, $^{**}P < 0.05$, $^{***}P < 0.01$.

data on attorney type are not causing a systematic bias in the results. Additionally, I run a specification (specification 6 in Table 4) where I use all counties in the data set, rather than simply the counties for which random assignment has been validated. While the results of this specification certainly could be in part driven by the very issues related to case selection I am trying to avoid, they do give a sense of how well the control variables help to account for any such selection issues. All regressions results are given in Table 4, with the nature of the controls and any restrictions on the data indicated beneath the results.

The results clearly show that, across a range of outcome variables, assigned counsel generate less favorable outcomes for their clients than do public defenders. The results are significant in each specification for three of the four dependent variables (*most serious*, *sentence*, and *duration*). Results for the other dependent variable, *guilty*, have the same sign as the other variables but have lower significance levels, with one specification significant at the 10% level, and several others that are significant at the 20% level. The main specification generates a point estimate of a 2.8% point difference in the likelihood of generating a guilty outcome, and this specification has a P-value of 0.11. Assigned counsel are significantly more likely to generate a conviction on the most serious offense category (the point estimate of the difference is 5.2% points in the main specification), they generate longer expected sentences (the point estimate is 3.36 months, and all specifications are significant at the 1% level), and their cases take a longer time from arrest to adjudication (the point estimate is 27 days, and all specifications are significant at the 1% level).[39] Furthermore, in results not reported here, I find that guilty pleas from assigned counsel cases result in significantly less favorable outcomes for defendants than guilty pleas from public defender cases (as measured by sentence length and likelihood of being convicted of the most serious charged offense), so differences in patterns of plea activity between groups do not account for these results.

These results are certainly comparable with the estimates generated in Iyengar (2007). In that paper, the estimate of the effect of assigned counsel

39. Note that these results are not being driven by differences in outcomes for one particular type of crime, as I find a robust and significant pattern holds for violent crimes, property crimes, and drug crimes.

on the likelihood of conviction is roughly 0.28% points, smaller in absolute terms than my estimate of roughly 3% points. However, the relative magnitudes are similar after accounting for the differences between the data in that paper, which involves federal cases, and the state court outcomes analyzed here. In the federal data, the overall likelihood of conviction was roughly 97% as compared to 79% in the relevant subset of state court cases used in this paper. Therefore, each estimate corresponds to roughly 10–15% of the cases for which there is a no conviction. Iyengar's results indicate that assigned counsel generate an average expected sentence nearly 5 months longer than public defenders in federal court (which corresponds to roughly 16% of an expected sentence). My point estimate of 3.36 months accounts for 32% of an expected sentence. This comparison further suggests that the relative magnitudes of these effects are similar for state and federal courts, and if anything, marginally more pronounced in state courts.

5.3. Effects of Outside Options on the Outcome Gap

The decision to pursue assigned counsel work boils down to a comparison of the value of performing assigned counsel work and the value of a given attorney's outside option.[40] I consider two types of attorneys that can apply for assigned counsel work—high quality types and low quality types—and they each have different outside options. In my preferred specification, I use the median attorney wage as the outside option of the high quality type and the tenth-percentile attorney wage as the outside option of the low quality type. The expectation is that assigned counsel outcomes will improve relative to public defender outcomes if the low quality type's outside option increases or if the high quality type's outside option decreases. I test whether outcomes change in this way using regression models analogous to those I used to measure various outcome gaps:

$$y_{\text{ijoct}} = \beta_0 + \beta_1(\text{assigned counsel*10th percentile attorney wage}_{jct}) + \beta_2(\text{assigned counsel*median attorney wage}_{jct})$$

40. While I use different points in the attorney wage distribution to approximate the changing outside options of high quality and low quality attorneys, the actual outside option is known only to the attorney.

$$+ \beta_3(\text{assigned counsel}_j) + \boldsymbol{\beta_4}(\mathbf{X_{ijoct}}) + \boldsymbol{\chi}_{ct} + \boldsymbol{\eta}_o + \varepsilon_{\text{ijoct}}. \quad (3)$$

The variables assigned counsel*10th percentile attorney wage and assigned counsel*median attorney wage are, respectively, the median and tenth-percentile attorney wage in a particular county and year, each interacted with the assigned counsel dummy.[41] I do not include the wage variable itself in most specifications because it is a county-year level variable and would thus be collinear with the controls in Equation (3) (which account for county-year effects). In the specification where I use separate county effects and year effects rather than county-year effects (specification 3 in Table 5), I do include the CPI-adjusted tenth-percentile and median attorney wages as controls. Table 5 includes robustness checks that are analogous to those in Table 4, as well as a specification (specification 7) where the twenty-fifth percentile attorney wage is used as the outside option of the low quality type.

While potential endogeneity is always a concern in this type of analysis (here, it would take the form of an unobserved variable affecting defendant outcomes of assigned counsel as compared to public defenders being correlated with attorney wages), it is not likely to be a major problem in this analysis. There are still many control variables in these specifications. County-specific institutions and unobservable characteristics are accounted for in either a single county fixed effect or a series of county-year or county-year-offense fixed effects. Furthermore, when considering the major potential source of endogeneity, namely a changing pattern of criminal activity (and how that might affect the legal process in a particular place), it is clear that such changes would affect cases going to both public defenders and assigned counsel. Since I am judging assigned counsel outcomes against public defender outcomes and focusing on changes in the observed gaps between them, these changing patterns pose less of an empirical problem than if I were comparing case outcomes to some absolute standard.[42] As

41. When running these regressions using the log of the wage variables, the significance levels are quite similar. Therefore, I am only reporting those tables for which I use the CPI-adjusted wages in the wage variables rather than variables that have been logarithmically transformed.

42. Another potential endogeneity problem with these specifications could occur if the absolute size of the wages were correlated with some unobserved factor affecting assigned counsel outcomes relative to public defender outcomes in a county. In order to

Table 5. Regression Results Showing Effect of Outside Options on Defendant Outcomes

	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Dependent variable: *guilty*				E[guilty] = 0.8069			
(assigned counsel)*(10th percentile attorney wage)	−0.0169	−0.0169	−0.0175	−0.0155	−0.0113	−0.0271*	
	(0.0141)	(0.0135)	(0.0110)	(0.0136)	(0.0132)	(0.0150)	
(assigned counsel)*(25th percentile attorney wage)							−0.0261**
							(0.00974)
(assigned counsel)*(median attorney wage)	0.00521	0.00521	0.00756	0.00502	0.00531	0.00848	0.0147*
	(0.00831)	(0.00812)	(0.00799)	(0.00788)	(0.00840)	(0.0135)	(0.00870)
n	9,391	9,391	9,391	18,283	19,210	6,742	9,391
Dependent variable: *most serious*				E(most serious) = 0.5901			
(assigned counsel)*(10th percentile attorney wage)	−0.0236**	−0.0236**	−0.0257***	−0.0225**	−0.0185*	−0.0350***	
	(0.0106)	(0.0106)	(0.00811)	(0.0103)	(0.00994)	(0.00847)	
(assigned counsel)*(25th percentile attorney wage)							−0.0294***
							(0.00560)
(assigned counsel)*(median attorney wage)	0.00686	0.00686	0.00976*	0.00828	0.00760	0.0111	0.0158***
	(0.00606)	(0.00532)	(0.00545)	(0.00568)	(0.00605)	(0.00667)	(0.00535)
n	9,391	9,391	9,391	18,283	19,210	6,742	9,391
Dependent variable: *sentence*				E(sentence) = 9.6839			
(assigned counsel)*(10th percentile attorney wage)	−0.926**	−0.926*	−1.109***	−1.003**	−0.946***	−1.186**	
	(0.417)	(0.440)	(0.403)	(0.371)	(0.341)	(0.554)	
(assigned counsel)*(25th percentile attorney wage)							−0.997***
							(0.330)
(assigned counsel)*(median attorney wage)	0.523*	0.523**	0.559**	0.589**	0.562**	0.541	0.776**
	(0.267)	(0.220)	(0.217)	(0.238)	(0.247)	(0.455)	(0.300)
n	9,318	9,318	9,318	18,071	18,986	6,700	9,318

(*Continued*)

Table 5. Continued

	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Dependent variable: *duration*				E(*duration*) = 92.5123			
(assigned counsel)*(10th percentile attorney wage)	−5.790**	−5.790*	−4.936*	−5.169*	−4.749*	−5.801	
	(2.806)	(3.261)	(2.584)	(2.646)	(2.466)	(3.943)	
(assigned counsel)*(25th percentile attorney wage)							−6.959***
							(2.036)
(assigned counsel)*(median attorney wage)	2.811**	2.811*	3.483**	2.307*	2.109	2.124	4.846***
	(1.345)	(1.377)	(1.296)	(1.286)	(1.325)	(2.181)	(1.578)
n	9,359	9,359	9,359	18,234	19,156	6,722	9,359
County dummies			X				
Year dummies			X				
Offense dummies	X	X	X	X	X	X	X
County-year dummies	X	X		X	X	X	X
Attorney type missing < 30% of time						X	
County-year-offense categories with assigned counsel and public defender cases	X	X	X			X	X
Only random assignment counties	X	X	X	X		X	X
Standard errors clustered by county	X		X	X	X	X	X
Standard errors clustered by state		X					

The variable *guilty* is a dummy variable equaling one when the defendant is convicted of a crime. The variable *most serious* is a dummy variable equaling one when the defendant is convicted of the most serious offense with which he or she was charged. The variable *sentence* is the length of time a defendant is set to be incarcerated, measured in months. The variable *duration* is the amount of time between arrest and adjudication in a case, measured in days. The variable *assigned* counsel is a dummy equaling one when the attorney handling the case is assigned counsel. In addition to controls described at the bottom of the table, each regression controls for defendant race, sex, status within the criminal justice system, prior convictions, and prior incarcerations. In specification 3, the CPI-adjusted tenth-percentile attorney wage and median attorney wage are also included as controls. Murder cases and cases with no adjudication outcome are excluded from the analysis. In all specifications, the median attorney wage in the area is taken to be the outside option for high ability types. In specifications 1 through 6, the tenth-percentile attorney wage in the area is taken to be the outside option of the low-ability type. In the final specification, the twenty-fifth percentile attorney wage in the area is taken to be the outside option of the low-ability type. Robust errors are in parentheses. Standard errors are clustered at the county level unless otherwise indicated. Significance levels: * $P < 0.1$, ** $P < 0.05$, *** $P < 0.01$.

far as the CPI-adjusted attorney wage distribution, it does not change dramatically over the course of the years for which I have data. Each point I consider (the tenth percentile, twenty-fifth percentile, and median) sees a modest (less than a two-dollar increase in the hourly rate) rise between 1998 and 2000, but by 2004, each value is within one dollar of its respective level from 1998. Nothing in these changes suggests a fundamental endogeneity problem associated with considering these points in the distribution.

If changes in the outside labor market for attorneys are affecting the quality of the assigned counsel panel, I expect positive coefficients on variables involving the high quality type's outside option (assigned counsel*median attorney wage) and negative coefficients on those involving the low quality type's outside option (assigned counsel*10th percentile attorney wage in specifications 1 through 6, assigned counsel*25th percentile attorney wage in specification 7). The results are given in Table 5, and it is clear that the signs of the coefficients are in line with this theory, and most of these coefficients are statistically significant, especially in those models where the dependent variable is *most serious*, *sentence*, or *duration*. Indeed, the outcome gap between public defenders and assigned counsel is sensitive to changes in the outside option of either type. The results of the main specification indicate that a one-dollar increase in the outside options of high quality types (which corresponds to roughly a 4% increase in the outside option) increases the expected sentence length by roughly 0.5 months; a one-dollar increase in the low quality type's outside option (which corresponds to a 7.5% increase in the outside option) reduces expected sentence by roughly 0.9 months. These values represent roughly 5–10% of an average sentence. For the sake of comparison, Abrams and Yoon (2007) find that an additional year of attorney experience reduces the defendant's expected sentence by 1.7%. Iyengar's estimate of the effect of experience is significantly larger, with a 1 year increase in experience reducing expected sentence by 5 months (or 24% of an expected sentence). Thus, if such a change in the outside options reduces the average experience on the assigned counsel panel by a single year, then these results are not out-of-line with previous estimates.

account for this possibility, I have tested analogous specifications where I demean the wage data within a given county (reducing the endogeneity concerns since unobserved factors correlated with the absolute size of the wages are differenced out). This transformation does not materially affect the results.

Changes in all the outcome measures seem especially sensitive to changes in the outside option of the low quality type attorneys (even accounting for the fact that a one-dollar change in a low quality type's outside option is bigger in percentage terms than a similar change for a high quality type). The magnitudes of the coefficients related to the low quality attorney types are often two to three times greater than those related to the high quality attorney types. They indicate that a one-dollar increase in the low quality type's outside option reduces the probability that an assigned counsel case results in a conviction on the most serious charge by more than 2% points (relative to public defenders), and it reduces the expected duration of the case by more than 5 days (relative to public defenders).

While these estimated effects are quite substantial in practical terms, there are a number of factors that should be considered as a way of putting these large relative magnitudes in context. First, these are CPI-adjusted dollars, where the base is 1982–84. As a consequence, the increase in wages leading to these changes in outcomes is actually larger than one dollar in nominal terms (since the average CPI value in this subsample is 177.9, the impact of a nominal hourly wage change of $1.78 is what is being measured). Secondly, since these estimates focus on one point in the distribution while holding fixed another point, I am essentially measuring how a particular type of change in the spread between these points affects outside options. A one-dollar change in either outside option represents a percentage change that is larger for the spread (on average 9%) than for either of the outside options themselves. Thus, the wage changes leading to these effects are more substantial than they might initially seem. Thirdly, to the extent that unemployment patterns are correlated with these wage changes, these effects represent the combined effect of the changes in both wages and unemployment. Finally, it is worth noting that the standard errors are relatively large, especially in the regressions where *guilty* is the dependent variable, and less precise point estimates could lead to point estimates that overstate the effect.

5.4. Outside Options and Changes in the Assigned Counsel Panel in Franklin County, OH

I augment the analysis in the previous section by examining the changes over time in the composition of the assigned counsel roster within a single

county. I perform this analysis for Franklin County, Ohio because I have individual-level data, which spans multiple years, on attorney compensation for assigned counsel work in that county. I generate measures of attorney quality by linking this information to Ohio Supreme Court records, which contain information on an attorney's initial date of registration in Ohio and law school attended. These data sources allow me to examine whether the observable characteristics of the attorneys on the assigned counsel roster change in the manner predicted by economic theory when outside labor market options change. Since the analysis in the previous section assumes that attorney selection decisions related to assigned counsel work are affected by exogenous changes in outside options, this analysis bolsters the assumptions underlying these models.[43] It demonstrates that the wages that serve as proxies for the outside options of different types of attorneys contain information that affects these decisions.

In the Franklin County analysis, I examine the extent to which the composition of the assigned counsel panel changes, not the extent to which case outcomes change.[44] I make the assumption that additional years of experience practicing law in Ohio adds to an attorney's quality. The findings in Abrams and Yoon (2007), which link additional experience to more favorable defendant outcomes, certainly support this assumption and make attorney experience an important characteristic to include in the analysis. I also assume attending a law school with a higher ranking is positively correlated

43. It should be noted that it is certainly possible that the selection dynamics in Franklin County, OH might not precisely mirror those in the other counties examined in the paper. The population center in this county, the city of Columbus, is the fifteenth largest city in the United States (U.S. Census Bureau, 2012, p. 34), so there are certainly areas in the multi-county data set that have larger populations as well as areas that have smaller populations. If larger (or smaller) population centers have different dynamics, the Franklin County results may not completely capture those effects. The aim is to show that the proposed mechanism by which variation in outside options ultimately affects defendant outcomes can be supported empirically.

44. Since I do not have data on the resolution of individual cases handled by these assigned counsel, I cannot link case outcome information to specific attorney characteristics that would be correlated with attorney quality (so I am not addressing questions like how much an extra year of experience affects the probability of a conviction). Also, since I do not have data on the resolution of public defender cases in this county, I do not have a baseline against which I can compare assigned counsel outcomes over time.

Table 6. Summary Statistics, Franklin County Data

Variable	Mean
Number of cases handled by AC panel	226.259
Number of attorneys on AC panel	92.454
All county attorneys proportion from first tier law schools	0.430
AC panel proportion from first tier law schools	0.248
All county attorneys years of experience	17.261
AC panel years of experience	16.106
Median attorney wage	19.913
Tenth-percentile attorney wage	11.082
Median wage—tenth-percentile wage	8.831

Data on assigned counsel (referred to in the table as "AC Panel") come from Franklin County Common Pleas Court and the registration records of the Ohio Supreme Court. Data on all attorneys from Franklin County (referred to as "All County Attorney") come from registration records of the Ohio Supreme Court. Law school tiers determined by *US News and World Report*. These summary statistics are calculated based on monthly averages for these variables from 2000 through 2008.

with an attorney's quality.[45] Then I measure how the average quality of the attorneys on the panel, as measured in these ways, changes with the relative value of outside options. This gives a sense of aggregate attorney selection decisions and helps explain why assigned counsel outcomes change with outside options. All data in this section is aggregated to the month level. Summary statistics are given in Table 6.

To test how outside options affect the relative quality of the assigned counsel panel, I create dependent variables that are each a difference between a panel-level quality measure and a county-level quality measure. More specifically, the dependent variables are:

1. (Average experience of attorneys on assigned counsel panel) – (Average experience of active, registered Franklin County attorneys).
2. (Percentage of attorneys on assigned counsel panel from first tier law schools) – (Percentage of active, registered Franklin County attorneys from first tier law schools).

I separately regress these dependent variables on the difference between the CPI-adjusted median attorney wage in the area and the CPI-adjusted

45. This could either be due to the fact that institutions with a higher rank actually do a better job training attorneys or simply because these institutions attract higher quality candidates to begin with (or both).

tenth-percentile attorney wage in the area:[46]

$$y_t = \beta_0 + \beta_1(\text{Median Attorney Wage} - \text{10th PercentileAttorney Wage}_{t-k}) + \varepsilon_t. \quad (4)$$

The expectation is that if these outside options are driving attorney selection onto assigned counsel panels, an increase in the wage differential reduces the aggregate quality of the panel (as it means the high type's outside option is improving relative to the low type). Therefore, I would a negative β_1 in the regressions with either of the dependent variables.

A key consideration is the timing of the incentives. What is of interest is the wage differential that induces an attorney to be active on the panel, but there is certainly a lag between when an attorney makes that decision and when that attorney is compensated for a case that he or she handles. Since I observe attorneys at the time they are paid, the relevant outside options to consider are the ones that occurred some months before the attorney is paid (that is, the outside options as they were at the time the attorney chose to be active on the panel).

The sequence of events that occurs once an attorney decides to apply to join the assigned counsel panel is as follows. Once an attorney applies for the panel in Franklin County Common Pleas Court, he or she may become eligible to receive appointments after the next in a series of standing monthly meetings of judges takes place. The average length of time from arrest to adjudication is just under 6 months. At torneys then have 30 days after the case is resolved to submit their bills, and they receive payment roughly 2 weeks later.[47] So, on average, there is roughly 7.5 months between the submission of an application and being paid, although some of these lags are undoubtedly shorter. Assuming that new assigned counsel are handling multiple cases, I would expect to begin to observe them receiving payments earlier than 7.5 months after applying, since some of their

46. In this county, assigned counsel tend to have less experience and on average attend lower quality institutions than the county average. If wages are correlated positively with experience and prestige of law school attended, then the lower end of the attorney wage distribution would be the relevant portion for the sake of this analysis.

47. Information on the sequence of events and the associated timing was gathered from correspondence with an Assigned Counsel Analyst in Franklin County.

cases would likely be resolved in their first few months of assigned counsel work (the tenth percentile of cases involves a case duration of roughly 2 months, therefore 3.5 months would be the expected lag between application and payment for those cases). Because this range exists, the appropriate lagged wage differential likely varies randomly across attorneys.[48] Therefore, I consider each lagged wage differential from 3 months to 10 months for the sake of completeness and as a robustness check (so, in the indices of Equation (4), $k = 3, 4, \ldots, 10$). Since wage differentials are highly correlated on a month-to-month basis,[49] I use the differential from only 1 month in a given regression. However, it should be noted that when I use months 3 through 10 in the same regression, the eight wage differentials are jointly significant. The results are reported in Table 7.

The gaps between the assigned counsel panel and Franklin County as a whole change in a way that indicate attorney selection decisions are being driven by changes in outside options. Furthermore, the point estimates peak in the specifications where the wage differentials are lagged between 3 and 6 months, which is right in line with the expected amount of time between applying for the panel and being compensated for a completed case (the fact that the point estimates fall beyond this time horizon serves a sort of falsification test). The results for the experience gap border on statistical significance, having P-values that range between 0.104 and 0.150 for specifications between the 3-month lag and the 8-month lag. Regressions using law school attended as the quality measure generate results that are significant at the 5% level for 3-, 4-, and 5-month lags. These results lend support to the assumptions underlying the multi-county analysis in Section 5.3.

Underlying both the multi-county and Franklin County analysis is the assumption that the distribution of attorney quality is relatively constant so that changes in the distribution of wages are not solely a reflection of

48. I am using the differential between the median attorney wage and the tenth-percentile attorney wage because I believe, as describe in the earlier discussion of outside options, it is the most appropriate one.

49. Data on attorney wages are collected by the BLS at one or two points in a given year during the time frame I consider. In order to have monthly measures of these wages, I have linearly interpolated between the data points found in the Occupational Employment Statistics (OES) reports produced by the BLS when an actual estimate is not available for a given month. When I do not interpolate and simply use the temporally closest OES estimate for a given month's wages, my results are similar.

Table 7. Regression Results Showing Effect of Differential in Outside Options Between Quality Types on Panel Quality Measures

Dependent variable	AC panel experience − all country attorneys experience E[AC panel experience − all country attorneys experience] = −1.1552		AC panel first tier proportion − all country attorneys first tier proportion E[AC panel first tier proportion − all country attorneys first tier proportion] = −0.1824	
Months lagged	Coefficient	*P*-value	Coefficient	*P*-value
3	−0.0956	0.1500	−0.0068**	0.0310
4	−0.1036	0.1240	−0.0065**	0.0370
5	−0.1072	0.1130	−0.0062**	0.0450
6	−0.1107	0.1040	−0.0058*	0.0650
7	0.1092	0.1090	−0.0052	0.1030
8	−0.0985	0.1500	−0.0036	0.2610
9	−0.0900	0.1920	−0.0023	0.4740
10	−0.0613	0.3740	−0.0013	0.6790

Data on assigned counsel come from Franklin County Common Pleas Court. Data on attorney characteristics, both for those attorneys on the assigned counsel panel and other attorneys, come from registration records of the Ohio Supreme Court. Law school tiers determined by *US News and World Report*. Experience is measured in years of registration with the Ohio Supreme Court. The first dependent variable is the difference between the average years of attorney experience for the active members of the assigned counsel in a given month and the average years of experience for all attorneys registered in the county in that same month. The second dependent variable is the difference between the proportion of active members of the assigned counsel that graduated from a first tier law school in a given month and the proportion of all attorneys registered in the county that graduated from a first tier law school in that same month. Each regression involves regressing the dependent variable on a single lagged measured of the difference between the CPI-adjusted median attorney wage and the CPI-adjusted tenth-percentile attorney wage in the area. Significance levels: $^{*}P < 0.1$, $^{**}P < 0.05$, $^{***}P < 0.01$.

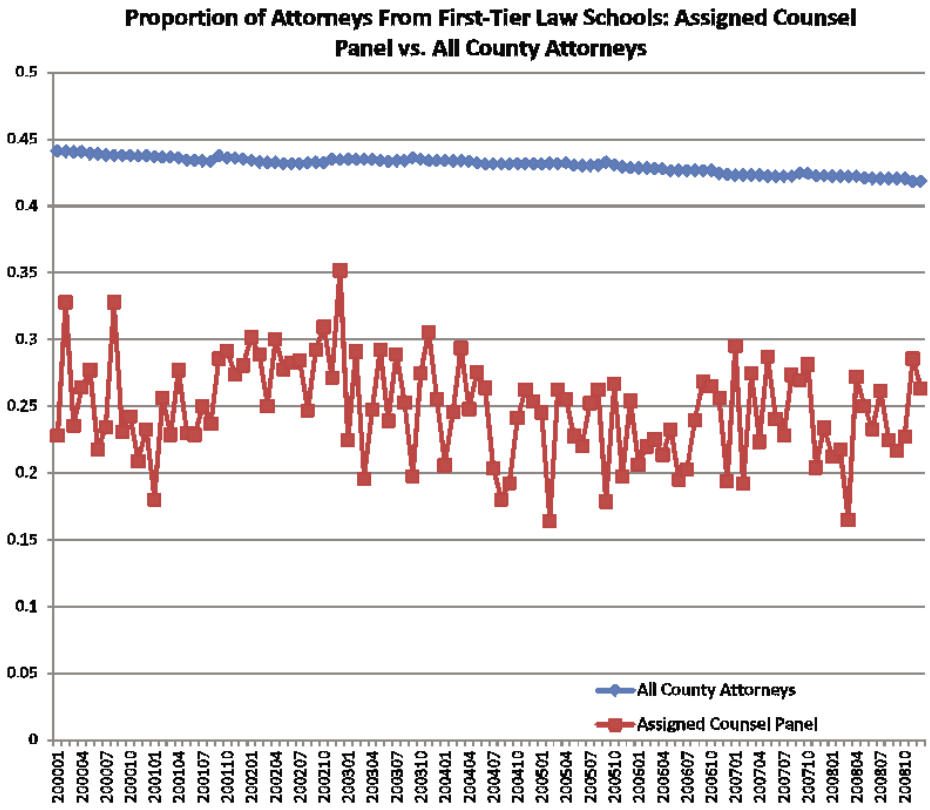

Figure 1. Percentage of Attorneys from Law Schools Ranked in First Tier, Both for All of Franklin County and for Assigned Counsel Panel, by Month
Note: Data on assigned counsel come from Franklin County Common Pleas Court. Data on attorney characteristics, both for those attorneys on the assigned counsel panel and other attorneys, come from registration records of the Ohio Supreme Court. Law school tiers determined by US News and World Report.

a changing labor force. Certain institutional factors in the legal profession make this assumption more likely to be valid. For instance, there are high switching costs associated with an attorney moving to a new state (having to take that state's Bar Examination, developing relationships with new colleagues, etc.). It is also clear from the Franklin County data that changes in the composition of the county's attorney pool are gradual, certainly more gradual than changes in the composition of the assigned counsel panel. Figure 1 contrasts the degree of change in the composition of these two groups over the 9 years for which the data are available. The percentage of attorneys from first tier law schools remains relatively constant in the county as a whole, though this measure of quality is quite volatile in the assigned counsel panel. While this visual evidence from a single county

does not eliminate the possibility that a changing composition of local attorneys could affect the multi-county analysis, it does suggest that change in the attorney population of a particular county is at the very least a slow-moving process.

6. Conclusion

In this paper, I have made quantitative comparisons between the two major systems for representing indigent defendants in state courts: public defenders and assigned counsel. I have shown that assigned counsel generate significantly less favorable outcomes for indigent defendants than public defenders, results quantitatively in line with previous work on indigent defense at the federal level. This is true for a variety of specifications and outcome measures, including likelihood of being convicted of the most serious charged offense, sentence length, and speed with which cases are resolved.

My findings indicate that changes in the outside labor market options of attorneys of different quality levels significantly affect the outcome gap along most of the dimensions considered here. In looking at attorney-level data from one county's assigned counsel panel, I see evidence that changes in outside options affect the overall quality of the assigned counsel panel in a way consistent with these selection incentives. This, at least partially, suggests why across dozens of large counties I see the outcome gap between public defenders and assigned counsel widening further when exogenous fluctuations in the local labor market induce relatively more low quality attorneys to choose assigned counsel work. This widening pattern is robust, holding across a number of specifications. This selection effect is certainly important from a policy perspective. The committees involved in approving candidates for this kind of work could benefit from simply being aware of these connections between outside labor market fluctuations and assigned counsel panel quality and outcomes, understanding that certain circumstances might lead to an increase in low quality applicants for this type of work. With these considerations in mind, perhaps additional institutional controls and standards could be put in place to mitigate the impact of these effects.

Appendix A. Results of Tests of Random Assignment Hypothesis

Joint significance of defendant characteristics				
County id	*F*-stat	*P*-value	Reject randomness	Obs
Alameda CA	1.9	0.0663		1,359
Allegheny PA	0.74	0.6366		194
Baltimore (City) MD	0.76	0.6217		310
Baltimore (County) MD	1.36	0.2198		353
Bronx NY	0.95	0.4689		537
Broward FL	2.76	0.0075	X	1,101
Contra Costa CA	0.14	0.9948		486
Cook IL	0.15	0.9945		2,666
Dade FL	1.05	0.3929		2,578
Dallas TX	1.1	0.3615		834
DuPage IL	0.13	0.9957		217
Duval FL	0.49	0.8404		105
El Paso TX	0.87	0.533		450
Erie NY	0.46	0.8598		223
Essex MA	N/A	N/A	X	115
Essex NJ	0.82	0.5747		926
Fairfax VA	1.45	0.1846		255
Franklin OH	0.41	0.8986		285
Fulton GA	0.74	0.6383		327
Hamilton OH	1.05	0.3964		512
Harris TX	1.16	0.3253		1,720
Hillsborough FL	1.66	0.1162		748
Honolulu HI	1.77	0.0915		557
Jackson MO	0.47	0.8592		370
Jefferson AL	1.08	0.3765		518
Jefferson KY	N/A	N/A	X	54
King WA	1.45	0.1819		477
Kings NY	1.34	0.2278		609
Los Angeles CA	3.18	0.0023	X	5,511
Macomb MI	N/A	N/A	X	3
Maricopa AZ	0.72	0.6541		1,958
Marion IN	0.43	0.8825		1,448
Middlesex MA	1.56	0.1458		387
Milwaukee WI	1.73	0.0988		813
Monroe NY	2.55	0.0143	X	389
Montgomery MD	0.62	0.742		501
Montgomery PA	0.85	0.5607		45
Nassau NY	N/A	N/A	X	19
New Haven CT	N/A	N/A	X	105
New York NY	2.36	0.0227	X	441
Orange CA	0.87	0.5314		1,597
Orange FL	0.64	0.7245		852
Palm Beach FL	0.65	0.716		593
Philadelphia PA	5.42	0	X	2,002

(*Continued*)

Appendix A. Continued

Joint significance of defendant characteristics				
County id	*F*-stat	*P*-value	Reject randomness	Obs
Pima AZ	1.46	0.1765		1,150
Pinellas FL	1.52	0.1557		851
Queens NY	0.87	0.5294		274
Riverside CA	1.14	0.3383		1,037
Sacramento CA	1.18	0.3122		697
Salt Lake UT	0.87	0.531		685
San Bernardino CA	2.86	0.0057	X	1,693
San Diego CA	1.06	0.3892		510
San Francisco CA	1.06	0.3892		580
San Mateo CA	N/A	N/A	X	243
Santa Clara CA	3.51	0.001	X	1,592
Shelby TN	1.79	0.0894		287
St Louis MO	0.19	0.9879		390
Suffolk MA	1.71	0.1082		228
Suffolk NY	N/A	N/A	X	53
Tarrant TX	N/A	N/A	X	530
Travis TX	N/A	N/A	X	265
Ventura CA	0.83	0.5642		309
Washington DC	N/A	N/A	X	N/A
Wayne MI	2.45	0.017	X	1,194
Westchester NY	N/A	N/A	X	N/A

Figures were computed from Bureau of Justice Statistics data from "State Court Processing Statistics" files. The dependent variable in these regressions is a dummy that takes on the value of one when a case is handled by assigned counsel and zero when it is handled by a public defender. Defendant characteristics include the defendant's race, sex, status within the criminal justice system, prior convictions, and prior incarcerations. Random assignment hypothesis is rejected when *P*-value is <0.05.

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