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Abstract

The lack of effective judiciary in post-socialist countries has been a pervasive concern and successful judicial reform an elusive goal. Yet to date, little empirical research exists on the functioning of courts in the post-socialist world. We draw on a new court-level panel dataset from Bulgaria to study the determinants of court case disposition and to evaluate whether judicial decision-making is subject to a quantity-quality tradeoff. Addressing endogeneity concerns, we find that case disposition in Bulgarian courts is largely driven by demand for court services. The number of serving judges, a key court resource, matters to a limited extent only in a subsample of courts, a result suggesting that judges adjust their productivity based on the number of judges serving at a court. We do not find evidence implying that increasing court productivity would decrease adjudicatory quality. We discuss the policy implications of our findings.

Keywords: courts, post-socialist countries, case disposition, quantity-quality tradeoff

JEL Classifications: P37, K40, D02

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1. Introduction

An effective judiciary is essential for the functioning of a market system. Courts play a key role in ensuring secure property rights and in facilitating sustained commercial activity in large-scale anonymous markets where reputation-based relational contracting loses its efficacy (see, e.g., Johnson et al. 2002, Dixit 2003, Stephenson 2007, Dove 2015). In transition countries in particular, evidence suggests that laws on the books must be backed by adequate enforcement by the courts in order for markets to prosper (Pistor et al. 2000, Skosples 2012).

Yet more than two decades after the start of post-socialist transition, little is known about the functioning of the judicial systems in transition economies beyond basic descriptive statistics and qualitative analyses (see, e.g., Dietrich 2000, Anderson et al. 2005, Ng et al. 2008). Rigorous empirical evidence on the performance of post-socialist courts based on original court data (Murrell 2001; Hendley 2004, 2005; Gadiuta 2012, Dimitrova-Grajzl et al. 2012a, 2012b, 2014a, 2014b), as opposed to indirect, survey-based evidence (e.g., Hendley et al. 2000, Johnson et al. 2002, Djankov et al. 2003a, Pyle 2006, Koford and Miller 2006), is particularly scarce.[1]

Systematic, empirically-grounded analysis of courts can provide valuable input into ongoing discourse about the design and reform of judicial systems (see, e.g., Messick 1999, Botero et al. 2003, Stephenson 2007). From the policymaking standpoint, the overall lack of empirical studies on the post-socialist judiciaries is worrisome since by year 2005, in transition countries "less overall progress [had] been made in judicial reform and strengthening than in

[1] Belova (2005) provides insights about the performance of the Soviet-era arbitration system. Shvets (2013) and Lambert-Mogiliansky et al. (2007) draw on data about court quality to study behavior of firms in post-socialist Russia. Gimpelson et al. (2010) examine the impact of court activity in labor cases on Russian labor market outcomes.

almost any other area of policy or institutional reform" (Anderson et al. 2005: 57). At the same time, the relative dearth of empirical analyses of the functioning of courts in the post-socialist region stands in stark contrast with the voluminous body of empirical research on firm behavior in transition (see, e.g., Djankov and Murrell 2002, Brown et al. 2006, Murrell 2005).

As a step towards filling this void in the literature, we draw on court data from post-socialist Bulgaria to examine two fundamental and related aspects of court activity: the determinants of the volume of case dispositions and the presence of a quantity-quality tradeoff in judicial decision-making. Applied to the Bulgarian context, we address questions such as: How do courts cope with an increase in caseload? Would increasing the size of the judiciary increase its effectiveness as measured by the ability of courts to dispose cases? Do courts that dispose more cases on average make worse adjudicatory decisions than courts that dispose fewer cases, all else equal?

Bulgaria provides an interesting and under-researched case for the study of the judiciary.[2] After the fall of communism, the country experienced a difficult economic transition. Despite numerous reforms, including in the judicial system, economic growth has been sluggish. Bulgaria joined the European Union (EU) in 2007 but remains the union's least developed member. Together with concerns about organized crime and corruption, the lack of an effective judiciary has been a major obstacle to economic progress. Court backlogs and delays were a very significant issue during the first decade and a half of transition (Anderson et al. 2005). Recent limited data suggest an improvement in the ability of Bulgarian courts to dispose cases in a timely fashion (see, e.g., CEPEJ 2014). However, delays are still pervasive

[2] See Schoenfelder (2005a, 2005b) and Koford and Miller (2006) for analysis of elements of the Bulgarian legal system during early transition.

especially in criminal justice and insolvency cases (see, e.g., European Commission 2014a: 5; European Commission 2014b). Identifying measures that would further enhance the effectiveness of the Bulgarian judicial system thus remains of vital policy importance (European Commission 2014a). To this end, it is important to understand the drivers of the volume of case disposition in Bulgarian courts.

In examining the determinants of court output in Bulgaria, we exploit the panel structure of our dataset. To address endogeneity concerns, we use the fixed effects framework and panel data-specific instrumental variable techniques. These methods, which rely on instrumental variables that are internal to the researcher's data, are particularly appealing in contexts such as ours where multiple sources of endogeneity bias exist but instrumental variables external to the researcher's data are not readily available.

Our analysis shows that case disposition in Bulgarian courts is heavily demand driven. All else equal, a ten percent increase in caseload leads on average to a ten percent increase in the number of disposed cases in both large and small district courts. In contrast, the number of judges as a key court resource is statistically significantly associated with court output only in small district courts, in which output responds positively but inelastically to the number of serving judges. These findings suggest, first, that judges tend to adjust their productivity depending on the number of judges at a court (see, e.g., Beenstock and Haitovsky 2004) and, second, that an increase in the size of the judiciary would be limitedly effective at increasing court output, and thus further reducing case disposition times, only in a subset of Bulgarian courts.

In the second step of our analysis, we assess whether case disposition in Bulgarian courts is subject to a quantity-quality tradeoff and, therefore, if legal reform emphasizing an increase in the volume of case disposition might come at a cost of diminished quality of judicial decision-making. The essence of our empirical approach is the comparison of court-year observations for which the number of disposed cases is *greater* than the regression-predicted value with court-year observations for which the number of disposed cases is *smaller* than the regression-predicted value. Utilizing the number of appealed cases as a measure of the quality of judicial decision-making, we find no statistically significant difference between the two groups. This result holds both for large and small district courts. A policy implication of our finding is that in Bulgaria any benefit from measures aimed at increasing court productivity can likely be captured without a reduction in adjudicatory quality.

Our paper, therefore, makes three contributions. First, we add to the growing empirical law and economics literature on the activity of courts. Within this literature, relatively few studies have been able to draw on original court-based data from judicial systems outside of the U.S. or the common law world (see, e.g., Beenstock and Haitovsky 2004, Mitsopoulos and Pelagidis 2007, Rosales-Lopez 2008, Schneider 2005; Di Vita 2012a, 2012b; Elbialy and Garcia-Rubio 2011, Finocchario Castro and Guccio 2014, Santos and Amado 2014, Falavigna et al. 2015). Our analysis of Bulgarian courts contributes to the literature by shedding light on the functioning of the judicial system in the understudied post-socialist region.

Second, we advance the existing literature on the determinants of court activity by addressing endogeneity problems in novel ways and to a fuller extent than existing contributions. While long acknowledged (see, e.g., Priest 1989, Buscaglia and Ulen 1997),

reverse causality has rarely been tackled in the literature on the determinants of court activity. Murrell (2001), Micevska and Hazra (2004), and Dimitrova-Grajzl et al. (2012a) are to our knowledge the only contributions that explicitly address two-way causality between the demand for court services and measures of court activity. Much like Dimitrova-Grajzl et al. (2012a), we additionally tackle the reverse causality between court output and judicial staffing.[3] However, in comparison with the analysis of Dimitrova-Grajzl et al. (2012a), we explore a wider set of instrumental variable approaches and specifications. In particular, we exploit the instrumental variable approach based on the general method of moments (Arellano and Bond 1991) which has not been utilized in any of the previous studies on the determinants of court activity.

Third, we contribute to the scant existing literature that examines whether judicial systems face a tradeoff between the quantity and quality of case resolution. Previous studies on the topic (Posner 1996, Rosales-Lopez 2008, Dimitrova-Grajzl et al. 2012b, Coviello et al. 2014) find mixed results. We contribute to this debate by presenting new evidence from a judicial system in which increasing court output is viewed as an important policy goal and thus concerns about the potential adverse repercussions for quality of adjudication are especially relevant.

The rest of the paper is organized as follows. Section 2 provides a brief overview of the Bulgarian judicial system. In Section 3, we develop the conceptual framework guiding our

[3] Beenstock and Haitovsky (2004) indicate the possibility for reverse causality between caseload and judges on one hand, and court output on the other hand. They argue, however, that their use of cointegration analysis applied to time-series dominated panel data yields "super-consistent" parameter estimates, which "have the effect of asymptotically removing simultaneous equation bias from the parameter estimates induced by the possible endogeneity of lodges cases and the feedback effect…of caseload pressure on the appointment of judgeships" (Beenstock and Haitovsky 2004: 360-361).

empirical analysis of the determinants of court case disposition. Section 4 introduces the data and variables. Section 5 presents the empirical strategy and the results on the determinants of court case disposition. In Section 6 we assess whether there is a quantity-quality tradeoff in Bulgarian courts. Section 7 concludes.

2. The Bulgarian Judicial System: A Selective Overview

2.1. The Court System, Jurisdiction, and Staffing

The foundation of the current judicial system in Bulgaria was established with the enactment of the Constitution of the Republic of Bulgaria in 1991, following the demise of the socialist regime. The court system consists of courts of general and specialized jurisdiction. The administration of justice is based on three instances. Aside from 113 district courts (*raionen sad*), which are the subject of our analysis, there are also 28 provincial courts (*okrazen sad*), 28 administrative courts, five appellate courts, five military courts, one specialized criminal court of appeal, the Supreme Court of Cassation, and the Supreme Administrative Court. A specialized criminal court that adjudicates cases on organized crime, terrorism and corruption was established in 2011.

The 113 district courts are first instance courts of general jurisdiction (civil, commercial, and criminal). District courts have jurisdiction over all civil and commercial disputes with value under BGN 25,000 (about EUR 12,500), as well as disputes about immovable property (such as land) with value under BGN 50,000 (about EUR 25,000). Judges in district courts do not specialize; as generalists, they adjudicate all relevant cases. Appeals to the decisions of district courts are adjudicated in provincial courts, which at the same time serve as courts of first

instance for cases involving higher stakes and greater legal complexity. The Supreme Court of Cassation serves as the last instance court for cases adjudicated in district courts.

The jurisdiction of individual district courts is determined on a territorial basis. The law stipulates that, in general, cases must be adjudicated in the court with geographic jurisdiction over the area of the defendant's permanent address.[4] However, the law also provides a number of exceptions to the rule of territorial jurisdiction. For example, lawsuits initiated by consumers, contractual damage claims, and tort claims may all also be filed in the court with geographic jurisdiction over the area of the plaintiff's permanent (and in some cases even current) address. Similarly, labor claims may be filed in the court with geographic jurisdiction over the area where the plaintiff works and real estate cases spreading over multiple jurisdictions may be filed in any of the courts with geographic jurisdiction over the relevant areas. Thus, choice of adjudication forum is a possibility in Bulgaria.

Overseeing the judiciary and resource and personnel management fall under the authority of the Supreme Judicial Council (SJC). In coordination with the Ministry of Justice, SJC determines the allocation of resources throughout the judicial system. This includes decisions about judicial staffing. Depending on the observed workload and output of courts, which SJC monitors on an annual basis, SJC may open new judicial positions, eliminate vacancies, and transfer judges across courts.

SJC also manages judicial appointments and promotions. Judges are appointed via a competitive procedure where candidates apply for openings. The performance of judges is evaluated periodically (typically every four years) by a SJC sub-committee. Assessment criteria

[4] Code of Civil Procedure (*Grajdanski Procesualen Kodeks*), State Gazette No. 59/20.07.2007 and amendments.

include the effectiveness at delivering timely and correct decisions. Tenure is granted after the completion of the first five-year term and a favorable evaluation. Judicial salary depends on a judge's professional experience and rank within the judicial system. In the busiest courts in the capital, Sofia, judges are assigned one rank higher than their professional peers in courts outside Sofia. The basic monthly remuneration for the lowest judicial position is set at double the amount of the average monthly salary for public-sector employees. Judges can earn limited salary bonuses for overtime work.[5]

2.2. Judicial Reforms and Challenges

During the first decade and a half after the collapse of socialism, Bulgaria struggled with implementing an effective judicial system. The promulgation of new laws outpaced the improvements in supporting institutions, such as courts. Accordingly, firms viewed administration of justice as exceptionally slow, courts as corrupt, and the judicial system as a serious obstacle to doing business (Anderson et al. 2005). Concerns about the lack of efficient judiciary trumped those about the lack of judicial independence (ibid.). In the process of Bulgarian accession to the EU, the European Commission therefore urged for reform to enhance the efficiency and independence of the judiciary.

A large number of judicial reform measures have been implemented since the early 2000s.[6] A reform in 2003 introduced measures to ensure judicial freedom from undue influence. The constitutional reform of 2005 largely revolved around EU integration and the corresponding transfer of authority from the national level to the EU. The 2006 and 2007 reforms aimed to strengthen the structure of the judiciary and its independence vis-à-vis the

[5] Judiciary System Act (*Zakon za Sadebnata Vlast*), State Gazette No. 64/7.08.2007 and amendments.
[6] As our goal is not to directly evaluate the effectiveness of any specific reform, we only list the key milestones.

executive and the legislative branch. Among others, the 2006 and 2007 reform measures clarified the advisory (as opposed to decision-making) role of the Minister of Justice with respect to the judiciary and the Supreme Judicial Council (Venice Commission 2008, 2009).

The 2007 reform redefined the role and strengthened the authority of the SJC as the highest administrative authority in the judicial branch in Bulgaria. It also introduced a new autonomous institution to monitor the efficiency and effectiveness of the judiciary: the Inspectorate with the SJC. New laws governing civil, criminal, and administrative procedure were adopted in years 2006 and 2007. Since the adoption of this new set of procedural rules, the focus has shifted to court delays and concerns have been raised about inadequate provisions to tackle the problem (European Commission 2008).

Following EU accession, Bulgaria became eligible for assistance aimed at overcoming institutional deficiencies through the Co-operation and Verification Mechanism (CVM). The EU, via CVM, has provided expertise and monitoring of the process of judicial reform as well as suggested measures for combating high-level corruption and organized crime. Several years after EU accession, however, the Bulgarian judicial system still faces many challenges both with respect to the independence and efficiency of the judiciary. Accordingly, trust in judicial institutions remains very low and concerns about subversion of justice, corruption, and political influence persist.[7]

There are no comprehensive statistics on disposition times for civil and commercial cases in Bulgarian courts of first instance (see, e.g., CEPEJ 2014: Figure 9.9). The available statistics portray a mixed picture. On the one hand, the reported average time to disposition

[7] Popova (2012) and Schoenfelder (2005a) provide a critical discussion of corruption and judicial independence in Bulgaria.

for non-criminal cases in Bulgaria (70 days) for year 2012 is shorter than the median value (149 days) for the CEPEJ (2014) sample. On the other hand, the reported time to disposition for insolvency cases (3.3 years) in Bulgaria is among the longest in the EU (European Commission 2014b: Figure 4) and many criminal cases are still characterized by "widespread delays" (European Commission 2014a: 5). Furthermore, "[t]he issues of workload imbalances and distribution of resources" remain a concern (ibid.: 10).

To tackle the problem of court delays, Bulgaria in 2012 introduced a mechanism through which citizens can seek compensation from the national government for slow and inefficient delivery of justice. The introduction of this mechanism was partly driven by an increase in the number of claims, filed at the European Court of Human Rights, addressing an individual's right to a hearing within a reasonable period of time. Upon filing a complaint and a favorable decision from the Inspectorate with the SJC, a successful plaintiff is entitled to a maximum compensation in the amount of EUR 5,000. In sum, implementation of measures improving the efficiency of the judicial system remains a high policy priority in Bulgaria.

3. The Determinants of Court Output: Conceptual Framework and Empirical Model

We are interested in examining the effect of two key determinants of the volume of case dispositions: the number of serving judges as the key input in the production of court output and caseload as a proxy for demand for court services.[8] To this end, we follow Dimitrova-Grajzl et al. (2012a) and posit the following general empirical model:

$$Resolved_{ct} = f(Judges_{ct}, Caseload_{ct}, x_{ct}) + \varepsilon_{ct}, \quad (1)$$

[8] See Dakolias (1999) for an extensive discussion of determinants of judicial performance.

where $Resolved_{ct}$ is the number of resolved cases for court *c* in year *t*, $Judges_{ct}$ is the number of judges at court *c* as measured at the end of year *t*, and $Caseload_{ct}$ is the sum of unresolved cases at court *c* at the beginning of year *t* and newly filed cases at court *c* during year *t*. x_{ct} is a vector of other controls that vary by court or/and over time and which may affect court output. ε_{ct} is the error term.

A common policy presumption regarding the impact of judicial staffing on court output is that, all else equal, increasing the number of serving judges increases court output.[9] This rationale, grounded in a simple production function model of courts, has underpinned a variety of court reform efforts worldwide (see, e.g., Buscaglia and Dakolias 1999; Botero et al. 2003, Hammergren 2007, Decker et al. 2011). Empirical literature, however, casts doubt on whether court effectiveness can indeed be increased through an increase in court resources (Botero et al. 2003). Beenstock and Haitovsky (2004) and Dimitrova-Grajzl et al. (2012a), for example, do not find a statistically significant effect of judicial staffing on court output in Israel and Slovenia, respectively. A plausible explanation for the lack of a positive effect of judicial staffing is that judicial productivity is endogenous and, hence, that existing judges prefer to reduce their productivity in light of new appointments (Beenstock and Haitovsky 2004: 366). Given the substantial costs involved in increasing the size of the judiciary, further empirical evidence, especially from a thus-far unexplored judicial system such as the Bulgarian, is warranted.

Theoretically, demand for court services as proxied by caseload also has an ambiguous effect on court activity. On the one hand, greater demand for court services might have a

[9] The exposition of the conceptual framework in this section in part draws on the discussion in Section 3 of Dimitrova-Grajzl et al. (2012a), which focuses on Slovenian courts.

positive effect on court output by incentivizing judges to avoid backlogs (see, e.g., Luskin and Luskin 1986, Rosales-Lopez 2008). On the other hand, an increase in caseload might reduce a court's ability to resolve cases promptly due to a congestion effect (e.g., Murrell 2001; Buscaglia and Ulen 1997: 286). If the incentive effect is sufficiently weakened by the congestion effect, we expect the resolved cases to be either inelastic with respect to caseload or possibly even decrease with caseload. In contrast, if the incentive effect is strong and dominates the congestion effect, we would expect the number of resolved cases to be elastic with respect to increases in caseload.

Endogeneity further complicates matters. One important source of endogeneity is court-level unobserved heterogeneity, subsumed in the error term ε_{ct} in (1), which may be correlated with the number of judges, caseload, or both. The caseload of a court, for example, may depend on unobserved factors such as the degree of litigiousness or extent of criminal activity in the area of the court's geographic jurisdiction. When the legal system allows disputing parties to choose in which court to file a claim, as is the case in Bulgaria (see Section 2.1), a court's caseload likely also depends on the unobserved court reputation for speed and quality of case resolution. Judicial staffing may likewise depend on unobserved court characteristics. In Bulgaria, judicial vacancies are filled through a competitive process where qualified candidates apply for publicly advertised court openings (see Section 2.1). Consequently, judges might choose to apply for a position in a specific court chosen for its reputation and career development possibilities.

Another source of endogeneity is reverse causality between court output on the one hand, and judicial staffing and caseload on the other hand. When court output is low, it is likely

that new judges will be appointed in order to reduce backlogs and delays (de Figueiredo and Tiller 1996: 440). Judicial staffing at courts is thus often a direct response to the observed case disposition record (Beenstock and Haitovsky 2004, de Figueiredo et al. 2000, Dimitrova-Grajzl et al. 2012a). In Bulgaria, the authority over judicial staffing resides with the Supreme Judicial Council (see Section 2.1). Depending on the court's ability to manage its workload, the Council may open new or close existing judicial positions as well as transfer judges across courts. Furthermore, a court that can ceteris paribus resolve more cases might attract more filings than a court that can resolve fewer cases (Buscaglia and Ulen 1997: 282). As long as there exists some possibility for the parties to choose the adjudication venue, which is the case in Bulgaria (see Section 2.1), causality will run not only from demand for court services to court output, but also in the opposite direction. Caseload will thus be endogenous to the number of resolved cases (Priest 1989; Murrell 2001: 13).

When endogeneity is an issue, failure to address it leads to biased and inconsistent estimates, which may in turn lead to erroneous policy conclusions. We use several empirical approaches to tackle endogeneity concerns in Section 5. We proceed with discussion of our data.

4. Data and Variables

4.1. Data

We use the official statistical records collected and published by the Supreme Judicial Council of Bulgaria. Our dataset covers Bulgarian district courts (*raionen sad*) during the 2005-2013 period. Following the classification used in official court statistics, we split the sample of 113 district courts into a sample of 27 large district courts and a sample of 86 small district courts.

While all district courts are first-instance courts and have jurisdiction over the same substantive issues (see Section 2), the two groups differ substantially in size (see below) and have jurisdiction over different geographic areas. Large courts serve metropolitan areas. Smaller courts serve predominantly rural areas.

For each court we observe (i) the number of cases resolved during a year, (ii) the number of serving judges as recorded at the end of the year, and (iii) caseload (the sum of cases pending at the beginning of the year and new cases filed during a year). We have 243 court-year observations for the large district courts sample and 772 court-year observations for the small courts sample.[10]

Table 1 presents summary statistics separately for the two sets of courts. The average caseload and the number of resolved cases are more than seven times greater in large district courts than in small district courts. The average number of judges is more than four times greater in large district courts. The average caseload per judge (not reported in Table 1) is about 47 percent greater in large district courts than in small district courts. Large district courts on average also face nearly ten times more appeals in total and about 67 percent more appeals per judge (not reported in Table 1) than small district courts. Finally, the group of large district courts is more heterogeneous than the group of small courts: the standard deviation-to-mean ratio for each variable for the sample of large courts exceeds that for the sample of small courts.

4.2. Outcome Variable

[10] The panel of large district courts is balanced. The panel of small district courts is unbalanced because data are not available for the years 2005 and 2006 for one of the courts.

Judicial performance can be measured along several dimensions (see, e.g., Dakolias 1999, Staats et al. 2005, Ramello and Voigt 2012), including independence, efficiency, and accessibility. In Section 5, we focus only on judicial effectiveness as measured by the ability of the judges and the court system to resolve cases (see, e.g., Beenstock and Haitovsky 2004, Rosales-Lopez 2008, Chemin 2009, Choi et al. 2009a, 2009b, 2010, 2011a, 2012; Ramseyer 2012, Dimitrova-Grajzl et al. 2012a, 2012b).

The number of resolved cases is a quantitative measure of court activity and, hence, does not directly reflect on the quality of court decisions. (We turn to an examination of the relationship between quantity and quality of court decisions in Section 6.) Furthermore, measuring court output through resolved cases does not distinguish between trial-based and non-trial based modes of case disposition (such as settlements, withdrawals, and abandonments); trial-based modes of disposition are typically much more resource-intensive than non-trial based modes (see, e.g., Dimitrova-Grajzl et al. 2014). Resolved cases, nevertheless, are an appropriate measure of court activity in legal systems such as the Bulgarian one, where the ability of courts to dispose cases promptly remains a policy priority.[11] Given these clarifications, we proceed with describing our explanatory variables.

4.3. Explanatory Variables

[11] In the absence of case-level data, some contributions (e.g., Buscaglia and Ulen 1997, Murrell 2001, Micevska and Hazra 2004, Mitsoploulos and Pelagidis 2007) measure courts' ability to resolve cases with indirect proxies such as the case turnover ratio, congestion rate, and average disposition time (see CEPEJ 2014). However, these measures, which seek to approximate the micro-level dynamics of case resolution with court-level aggregate data, are problematic for purposes of regression analysis as the components of these outcome measures (e.g. unresolved cases) at the same time also appear as explanatory variables (e.g. caseload is the sum of unresolved and newly filed cases).

We seek to evaluate the effect of two key determinants of court output. The first is judicial staffing as measured by the number of serving judges. The second is the demand for court services as measured by a court's caseload.

Figures 1 and 2 show the evolution of the cross-sectional mean for number of judges, caseload, and resolved cases over time for large and for small district courts, respectively. In both large and small district courts, the average caseload was increasing relatively steadily until 2011. It decreased somewhat after year 2011, likely as a consequence of the economic downturn; the financial and economic crisis reached Bulgaria and other Eastern European countries with a delay.[12] The mean number of resolved cases is smaller than the mean caseload in every year for both large and small district courts. This suggests that courts have not been fully successful at eliminating backlogs. Figure 3 confirms this: the clearance rate (the ratio of resolved to newly filed cases, multiplied by 100) never exceeds 103 and is in fact smaller than 100 for multiple years for both large and small district courts.

The average number of judges in large district courts has been steadily increasing. This reflects the initiatives, led by the Supreme Judicial Council, aimed at increasing court output and reducing backlogs (see Section 2). In contrast, the mean number of judges in small district courts peaked in 2009. Albeit the relative drop has been small (less than 0.3 judges), the mean number of judges in small district courts has been decreasing since then, possibly as a result of the policy to transfer some judges from small district courts to large district courts. The need to increase output has in general been perceived as greater in the case of large district courts than in the case of small district courts.

[12] The economic and financial crisis likely led to an increase in the volume of insolvency procedures. Insolvency procedures, however, are adjudicated in provincial courts in Bulgaria.

Figures 1 and 2 entail two interesting comparative patterns. First, the mean number of resolved cases closely tracks the mean caseload for both large and small district courts. Second, the mean number of resolved cases and the mean number of judges appear to co-evolve much less closely. The patterns in Figures 1 and 2, of course, do not allow one to draw immediate conclusions about the causal relationships (or lack thereof) between resolved cases on the one hand, and caseload and the number of serving judges on the other hand. To investigate causality, we turn to regression analysis.

5. The Determinants of Court Output: Empirical Strategy and Results

We use several empirical approaches to estimate variants of model (1). We log all of the variables in order to smooth the effect of outliers and to facilitate the interpretation of coefficient estimates as elasticities. We base inference on heteroskedasticity-robust standard errors clustered at the court level. Clustering errors at the court level allows us to correct for plausible correlation of error terms over time for all courts, but we rule out correlation between error terms across courts.

5.1. Benchmark: Pooled OLS

As a benchmark, we examine the association between court output and judicial staffing and the demand for court services using the following empirical models:

$$Resolved_{ct} = \beta_0 + \beta_1 \cdot Judges_{ct} + \beta_2 \cdot Caseload_{ct} + \varepsilon_{ct} \quad (2a)$$

$$Resolved_{ct} = \beta_0 + \beta_1 \cdot Judges_{ct} + \beta_2 \cdot Caseload_{ct} + \lambda_t + \varepsilon_{ct}, \quad (2b)$$

where β_0 is a regression constant. The variables $Resolved_{ct}$, $Judges_{ct}$, and $Caseload_{ct}$ are logged resolved cases, logged number of judges and logged caseload for court c in year t. λ_t in model (2b) is a year fixed effect included to control for any factors that affect all courts but vary across

time, such as policy changes affecting the judicial system and the effects of the business cycle. All of these plausibly affect court output, judicial staffing and the demand for court services.

We estimate models (2a) and (2b) using pooled OLS. The results are reported in columns (1), (2), (5), and (6) of Table 2. Both in the case of large courts and in the case of small courts, we find a statistically significant positive association between court output and caseload. The implied elasticity of court output with respect to caseload value is slightly larger than one for the specification without year effects (columns (1) and (5)) and slightly smaller than one for the specifications with year effects (columns (2) and (6)).

In both court samples, the number of judges is, surprisingly, negatively and statistically significantly associated with court output in the specification without year effects (columns (1) and (5)). Once we control for year effects, however, the coefficient on the number of judges becomes statistically insignificant in both court samples (column (2) and (4)).

In order to interpret the pooled OLS estimates of β_1 and β_2 from expressions (2a) and (2b) in Table 2 as causal effects of judicial staffing and the demand for court services, respectively, the error term ε_{ct} must be uncorrelated with the explanatory variables of our interest. Given the likely presence of reverse causality and perhaps even omitted variables (despite high values of R-squared across all four specifications), this is an untenable assumption. As a result, the pooled OLS estimates of β_1 and β_2 discussed above are likely biased. To address endogeneity concerns, we turn to estimation of specifications with court fixed effects.

5.2. Specifications with Court Fixed Effects

We explore the following two-way fixed effects specifications:

$$Resolved_{ct} = \beta_1 \cdot Judges_{ct} + \beta_2 \cdot Caseload_{ct} + \lambda_t + \mu_c + \varepsilon_{ct} \quad (3a)$$

$$Resolved_{ct} = \beta_1 \cdot Judges_{ct} + \beta_2 \cdot Caseload_{ct} + \lambda_t + \mu_c + \mu_c t + \varepsilon_{ct}. \quad (3b)$$

where μ_c is the court fixed effect and the remaining variables are as defined in (2b). μ_c captures all court-level, time-invariant factors that may affect court output as well as judicial staffing and the demand for court services. Examples include geographic differences in litigation culture and any entrenched informal court norms (see, e.g., Church 1985). Specification (3b) in addition includes a court-specific linear time trend, $\mu_c t$, which controls for any unobserved court-specific trends in court output. It may be, for instance, that a subset of courts has exhibited a favorable trend in the resolution of cases and, at the same time, these courts happened to experience the greatest change in either the caseload or judicial staffing. In that case, we would observe an association between court output and judicial staffing or the demand for court services, yet the association would not be causal. The inclusion of the court-specific time trend mitigates such concerns.

We estimate models (3a) and (3b) using the least squares dummy variable (LSDV) approach: we run OLS regressions after inclusion of a full set of court and year dummies (and, in the case of specification (3b), court-specific linear time trend).[13] The results are reported in columns (3), (4), (7), and (8) of Table 2. The number of resolved cases statistically significantly increases with caseload in both large and small district court samples. Under a causal interpretation, a ten percent increase in caseload, all else equal, leads to an approximately ten percent increase in the number of resolved cases. The estimated elasticity of court output is in

[13] This approach is equivalent to 'within' fixed effects estimation whereby one first time-demeans the data (to eliminate the court fixed effect) and then estimates the resulting time-demeaned model with OLS.

fact statistically insignificantly different from one in both columns (4) and (8) (the respective *p*-values for the F-test of the null hypothesis that β_2=1 are 0.418 and 0.794).

The estimated coefficient on the number of judges is positive, but small in magnitude and statistically insignificantly different from zero in all four columns (3), (4), (7), and (8). Thus, we find no evidence that increasing the number of serving judges increases court output.

Unlike the pooled OLS estimates, the fixed effect estimates in Section 3.2 are consistent even if judicial staffing and caseload are correlated with some court-level, time-invariant factor or trend (see (3b)), which also affects total court output. However, fixed effects estimation of specifications (3a) and (3b) does not allow for the possibility of correlation between judicial staffing and caseload, and the time-varying component of the error term, ε_{ct}. Such correlation could arise because of reverse causality or some aspect of time-varying, court-level unobserved heterogeneity. Furthermore, models (3a) and (3b) do not allow for plausible persistence in the volume of case resolution at a court over time. To further address endogeneity concerns, we thus turn to instrumental variable approaches applied in the context of dynamic panel estimation. Because these techniques rely on instrumental variables that are internal to the researcher's data, they are particularly appealing in contexts such as ours, where suitable external instrumental variables are not available.

5.3. Instrumental Variable Approaches

We posit the following dynamic model:

$$Resolved_{ct} = \rho \cdot Resolved_{c,t-1} + \beta_1 \cdot Judges_{ct} + \beta_2 \cdot Caseload_{ct} + \lambda_t + \mu_c + \varepsilon_{ct}, \qquad (4)$$

where the right-hand side features a lagged dependent variable that controls for possible persistence in court case resolution. The fixed effects estimator is biased and inconsistent in

the presence of a lagged dependent variable (Nickell 1981). However, the fixed effects estimator may be biased even when the true model is (3a), that is, when ρ=0 in (4). Bias will arise in the presence of reverse causality or existence of an unobserved court-level time-varying (as opposed to time-invariant) variable that affects either judicial staffing or caseload (or both) and, at the same time, court output.

To address the resulting endogeneity problem, we proceed as follows. We, first, first-difference expression (4), thereby eliminating court fixed effects:

$$\Delta Resolved_{ct} = \rho \cdot \Delta Resolved_{c,t-1} + \beta_1 \cdot \Delta Judges_{ct} + \beta_2 \cdot \Delta Caseload_{ct} + \Delta\lambda_t + \Delta\varepsilon_{ct} \qquad (5)$$

In expression (5), $\Delta Resolved_{c,t-1}$ is correlated with $\Delta\varepsilon_{ct}$ by construction: by expression (4), $\varepsilon_{c,t-1}$ is correlated with $Resolved_{c,t-1}$. Furthermore, even if ρ=0 (and, hence, the true model is (3a)), reverse causality or time-varying omitted variables imply that both $\Delta Judges_{ct}$ and $\Delta Caseload_{ct}$ are correlated with $\Delta\varepsilon_{ct}$. However, as long as the number of judges and caseload at court *c* in year *t* are weakly exogenous, that is, conditionally uncorrelated with future realizations of the error term, then the second and deeper lags of $Judges_{ct}$ and $Caseload_{ct}$ are uncorrelated with $\Delta\varepsilon_{ct}$. Similarly, as long as ε_{ct} is not serially correlated, the second and deeper lags of $Resolved_{ct}$ are uncorrelated with $\Delta\varepsilon_{ct}$. Under these assumptions, the second and deeper lags of $Resolved_{ct}$, $Judges_{ct}$, and $Caseload_{ct}$ are respectively available as instruments for $\Delta Resolved_{c,t-1}$, $\Delta Judges_{ct}$, and $\Delta Caseload_{ct}$. Model (5) can then be estimated using either the two stage least squares (IV-2SLS) approach suggested by Anderson and Hsiao (1982) and Wooldridge (2002, Ch. 11) or the general method of moments (GMM-IV) approach developed by Arellano and Bond (1991). We present results for both approaches, with and without the ρ=0 restriction.

Specifically, when we use the 2SLS approach and impose ρ=0, in which case the true model is assumed to be (3a), we instrument for $\Delta Judges_{ct}$ and $\Delta Caseload_{ct}$ with $Judges_{c,t-2}$ and $Caseload_{c,t-2}$, respectively. When we impose no a priori restriction on ρ, in which case the true model is assumed to be (4), we instrument for $\Delta Resolved_{c,t-1}$, $\Delta Judges_{ct}$, and $\Delta Caseload_{ct}$ with $Resolved_{c,t-2}$, $Judges_{c,t-2}$, and $Caseload_{c,t-2}$, respectively.

Given the use of lags as instruments, application of 2SLS reduces the sample size. The choice of lags deeper than the second lag as instruments in particular comes with a notable loss of efficiency. To increase efficiency, we also use the difference GMM estimator that in comparison with the 2SLS approach better mitigates the tradeoff between lag length and sample size (see Roodman 2009a). Under the assumptions of no serial correlation in the error term ε_{ct}, the two-step difference GMM estimator that we utilize therefore relies on the following moment conditions when we impose no a priori restriction on ρ: $E[\Gamma_{c,t-s}(\varepsilon_{ct}-\varepsilon_{c,t-1})]=0$, where $\Gamma_{c,t-s}\equiv(Resolved_{c,t-s}, Judges_{c,t-s}, Caseload_{c,t-s})$ for $s\geq 2$, $t=3,\ldots,T$. When we impose ρ=0, $\Gamma_{c,t-s}\equiv(Judges_{c,t-s}, Caseload_{c,t-s})$.[14] The implied number of available internal instruments is large. However, "[s]imply by being numerous, instruments can overfit instrumented variables, failing to expunge their endogenous components and biasing coefficient estimates towards those from non-instrumenting estimators" (Roodman 2009b: 139). To avoid this instrument proliferation problem, we follow Roodman (2009a: 108, 129) and always 'collapse' the resulting GMM-style instrument set.

[14] We also estimated the model using the 'system GMM' estimator of Arellano and Bover (1995) and Blundell and Bond (1997), which relies on an additional set of moment conditions and can improve the small-sample and asymptotic performance of the 'difference GMM' estimator. The diagnostic tests for the results obtained using the system GMM estimator, however, cast doubt on the validity of the system GMM instruments. We thus present our conclusions based on the results obtained using the difference GMM estimator.

The consistency of the difference GMM estimator depends on whether lagged values of the explanatory variables are valid instruments. To address this issue, we consider two standard diagnostic tests: the Hansen test of over-identifying restrictions and the Arellano-Bond test for serial correlation of the error term ε_{ct}.

The results using both 2SLS and GMM instrumental variable approaches are reported in Table 3. Using the 2SLS approach, we find a positive and statistically significant effect of caseload on court output in both large and small district courts. The effect of caseload on court output is positive and significant for both the model without a lagged dependent variable (columns (1) and (5)) and the model with a lagged dependent variable (columns (2) and (6)).

In contrast, the estimated effect of the number of judges is not statistically significantly different from zero in any of the four IV-2SLS columns in Table 3. We also find evidence of persistence in court output: the estimate of ρ is positive and statistically very significant (columns (2) and (6)). Based on the estimates in columns (2) and (6), the implied steady-state elasticity of court output (when $Resolved_{c,t-1}=Resolved_{ct}=Resolved_{c,\infty}$) with respect to caseload equals 0.82 for large district courts and 0.58 for small district courts.

The full set of first-stage regressions is reported in Table 1A in the Appendix. The estimated models are exactly identified. Hence, tests of over-identifying restrictions are not possible. The F statistic for the test of excluded instruments ranges from 2.81 to 18.32. Based on the rule of thumb suggested by Staiger and Stock (1997), therefore, some of our instruments may be only weakly correlated with the endogenous regressors.[15] Since weak

[15] The critical values of the Stock and Yogo (2005) test statistic, which are sometimes used in conjunction with the the Kleibergen-Paap rk Wald F-statistic to assess instrument strength, are not available for scenarios with three endogenous regressors and three excluded instruments.

instruments bias 2SLS estimates in the direction of OLS estimates (see, e.g., Bound et al. 1995), our 2SLS estimates should be interpreted with some caution.

Using the GMM approach, which results in efficiency gains relative to the 2SLS approach, we again find a positive and statistically significant effect of caseload on court output in both large and small district courts. The effect of caseload on court output is positive and significant for both the model without lagged dependent variable (columns (3) and (7) in Table 3) and the model with lagged dependent variable (columns (4) and (8)). We also find evidence of persistence in court output (columns (4) and (8)). The effect of judicial staffing is statistically insignificantly different from zero for large courts (columns (3) and (4)), but, this time, positive and statistically significant in the case of small courts (columns (7) and (8)).

Based on the estimates in columns (4) and (8) of Table 3, the implied steady-state elasticity of court output with respect to caseload equals 1.02 for large courts and 1.08 for small courts. In the case of small courts, where the effect of judicial staffing is statistically significant, the implied steady-state elasticity of court output with respect to the number of judges equals 0.20. That is, while a ten percent increase in caseload ceteris paribus leads to a ten percent increase in court output, the same (ten percent) increase in court output would require a 50 percent increase in the number of judges, all else equal.

Finally, the Hansen tests of over-identifying restrictions in columns (3), (4), (7), and (8) and the tests of serial correlation are suggestive of the appropriateness of our instrumentation strategy within the GMM framework.

5.4. Summary and Discussion

Our empirical results suggest that the demand for court services is a statistically robust and economically important determinant of case disposition in both large and small district courts in Bulgaria. Based on our preferred specifications (columns (4) and (8) in Tables 2 and 3), we find that court output is almost exactly unit elastic with respect to caseload in both large and small district courts. This suggests that any caseload-induced congestion effects are weak in comparison with the incentive effect (see Section 3).

The evidence on the effect of judicial staffing varies by type of district courts. In large district courts, we find no statistically significant effect of the number of judges on court output based on any of our preferred specifications (column (4) in Tables 2 and 3). In small district courts, in contrast, the number of judges has a positive effect on court output when we address endogeneity concerns using our preferred instrumental variable approach. According to our estimates, court output in small district courts is inelastic with respect to the number of judges.

Our finding that demand for court services is a key driver of court output in Bulgaria resonates with the findings for Israel (Beenstock and Haitovsky 2004) and Slovenia (Dimitrova-Grajzl et al. 2012a). However, in contrast to the findings from Israel and Slovenia, as well as other evidence on the lack of effectiveness of resource increases on the performance of judiciary (Botero et al. 2003), we find that policy measures aimed at increasing resources available to courts can be impactful in Bulgaria. In the case of small district courts, we find that judicial staffing does influence case disposition when we address endogeneity concerns, although the effect is quantitatively relatively small. Based on our estimates, a ten percent

long-run increase in total case dispositions would ceteris paribus require a fifty percent increase in the number of serving judges.

What might explain the positive (albeit relatively small) effect of judicial staffing in small district courts and the lack of an effect of judicial staffing on court output in large district courts in Bulgaria? In the absence of judge or case level data, which would allow us to paint a more nuanced picture of the functioning of courts, we offer a conjectural explanation and leave a more detailed analysis of this issue for future research. Our argument builds on legal realists' premise that judges, like everybody else, trade off work for leisure (Posner 1993, Beenstock and Haitovsky 2004, Stephenson 2009). In order to increase leisure, incumbent judges may therefore rationally choose to decrease their work effort, and hence productivity, in response to new judicial appointments (Beenstock and Haitovsky 2004: 366). The extent to which incumbent judges adjust their work effort, however, in part depends on their marginal utility from leisure.

We suggest that the variation in the effect of judicial staffing on court output in small versus large district courts in Bulgaria may reflect systematic differences in the marginal utility of leisure between large district court judges and small district court judges, and that these differences in turn possibly reflect disparities in workload per judge across courts. In Bulgaria, the average caseload per judge in large district courts (499) is for our sample higher than the average caseload per judge in small district courts (338). If overburdened, the incumbent judges in large district courts have a relatively high marginal utility of leisure and may prefer to direct any new judicial appointees to take over a portion of the incumbent judges' caseload rather than resolve any new cases. A net result is a decrease in case dispositions by the

incumbent judges and, consistent with the pattern observed in the data, no additional disposed cases at the level of a court.

The marginal utility from leisure for judges in small district courts is likely comparatively lower because judicial workload in small district courts is on average smaller. If so, then in light of new judicial appointments, the incumbent judges in small district courts rationally reduce their work effort to a lesser extent than their peers in large district courts. New judicial appointees in small district courts may thus be able to actively contribute to court output by disposing new cases assigned to them. A net result is an increase in total court output in small district courts, as observed in the data.

6. Assessing the Presence of the Quantity-Quality Tradeoff in Case Resolution

In legal systems where court delays are a concern, implementation of measures aimed at increasing the rate of case resolution understandably emerges as a policy priority. However, can legal reform strive to increase the speed of case resolution without compromising the quality of judicial decisions? An increase in the speed of judicial case resolution, and thus ceteris paribus total court output, may come at the expense of the quality of judicial verdicts. Given the complexity of a case and the stakes involved, when judges spend less time deliberating each case, they are more likely to make mistakes (Posner 1996: 223). Thus, a cost-benefit analysis of legal reform aimed at increasing judicial and court productivity necessitates an understanding of whether there exists a tradeoff between quantity and quality in judicial case resolution.

6.1. Empirical Approach

To assess whether there is a quantity-quality tradeoff in judicial decision-making in Bulgaria, we follow the approach of Rosales-Lopez (2008) and Dimitrova-Grajzl et al. (2012b). Applied to our court-level data, the essence of our statistical test is to examine if court-year observations for which the number of case dispositions is large relative to the regression-estimated benchmark differ systematically in terms of the number of appealed cases (see, e.g., Mitsopoulos and Pelagidis 2007, 2010; Coviello et al. 2014) from the court-year observations for which the number of case dispositions is small relative to the regression-estimated benchmark. The number of appealed cases proxies for the quality of judicial decision-making since lower court judges prefer to avoid reversals by appellate courts (see, e.g., Stephenson 2009); hence, they prefer to avoid having their decisions appealed. The summary statistics for the variable Appealed Cases are presented in Table 1.

The number of appealed cases is admittedly an imperfect proxy for the quality of judicial decisions. The decision to appeal a tried case is endogenous to plaintiffs' and defendants' estimated probabilities of successful overturning of verdicts and the costs involved in the process (see, e.g., Priest and Klein 1984).[16] Any difference in the number of appealed cases between two court-year observations may, therefore, reflect merely the unobserved differences in parties' subjective assessment of the likelihood of favorable verdict and not the quality of judges' decisions. In the case of post-socialist Bulgaria, the scope for divergent expectations, and thus the parties' tendency to appeal judicial decision, may be particularly

[16] An alternative proxy for the quality of judicial decision-making is the number of overturned cases (see, e.g., Posner 2000). Comprehensive data on overturned cases is unfortunately not available to us. Moreover, overturned cases are also an imperfect proxy for the quality of judicial decision-making: holding everything else constant, a smaller number of overturned cases may reflect a judge's ability to anticipate the preferences of a higher court (see, e.g., Choi et al. 2011a) rather than better quality of the judge's decisions.

severe due to relatively high levels of institutional uncertainty characteristic of post-socialist environments.[17] Our conclusions should therefore be interpreted with caution.

Subject to the above caveats, if we establish that those court-year observations featuring a relatively large number of cases also exhibit a relatively large number of appealed cases, we may conclude that that an increase in the volume (or speed) of judicial case resolution comes at a cost of lower quality of judicial decision. If, however, the opposite is true, then the implementation of policies aimed at increasing judicial productivity will not come at the cost of quality and, hence, may be socially desirable.

To conduct the test, we draw on the regressions reported in columns (4) and (8) of Table 2. These regressions, which include a full set of court fixed effects and the court-specific time trend, allow us to explain as much of the variation in court output as possible given available data. The associated R-squared is very high for both the sample of large district courts (0.9982) and the sample of small district courts (0.9950). For each type of court, we then classify court-year observations into two groups. The first group, which we refer to as the Above-Average Output group, consists of court-year observations with positive residuals from the respective court output regression. The second group, referred to as the Below-Average Output group, are court-year observations with negative residuals from the respective court output regression.

We calculate the mean number of the log of (Appealed Cases+1) for each of the two groups. Adding a one to Appealed Cases before logging the variables allows us to retain court-

[17] In Bulgaria, the volume of appeals may also reflect the public's distrust in the judicial system. However, as long as any effect of distrust in the judicial system on the volume of appeals is approximately proportional across courts, it should not affect the validity of our analysis which is based on the relative comparison of the volume appeals between groups of court-year observations.

year observations with no appealed cases during the time period of our study and thus avoid sample selection bias. Logging the variables addresses the non-normal distribution of the data (see, e.g., Choi et al. 2011b: Sec. IV and Table 1, Panel B). We then compare the mean of log of (Appealed Cases+1) for the Above-Average Output group with the mean of log of (Appealed Cases+1) for the Below-Average Output group. Specifically, we test the null hypothesis that the two means are the same.[18]

6.2. Results and Discussion

Our results are summarized in Table 4. In the case of large district courts, the set of court-year observations with above average output in fact features fewer appeals, on average, than the set of court-year observations with below average output. The difference in means (about 478 cases) is not statistically significant. In the case of small district courts, in contrast, the set of court-year observations with above average output does feature more appeals, on average, than the set of court-year observations with below average output. The difference in means (about eight cases), however, is again not statistically significant. In sum, we do not find persuasive evidence of the existence of a quality-quantity tradeoff in Bulgarian district courts.

Our results resonate with some of the earlier conclusions in the literature. Rosales-Lopez (2008) demonstrates that, in a cross-section of courts of first instance in the Spanish region of Andalusia, the courts that resolve an above-average number of cases face a lower reversal rate than the courts that resolve a below-average number of cases. Coviello et al.

[18] As a robustness check, we also ran regressions with the log of (Appealed Cases+1) as the dependent variable, and logged Resolved Cases, Number of Judges, Caseload as well as a full set of year and court fixed effects and court-specific time trend simultaneously included as explanatory variables in the estimated equation. Our interest was in the sign and statistical significance of the coefficient on logged Resolved Cases. A positive and statistically significant coefficient indicates the presence of a quantity-quality tradeoff in judicial case resolution. Our conclusions (detailed results are available upon request from the authors) based on this approach were identical to the conclusions reported in the paper.

(2014) examine a small sample of judges specializing in labor issues at one Italian court and show that shorter duration of trials is associated with a lower probability of appeal. Posner (1996: 172) in a comprehensive study of the U.S. federal appellate courts notes that there is no clear evidence of a reduction in adjudicatory quality, even though the courts' caseload, and hence output, have increased over time. Dimitrova-Grajzl et al. (2012b), in contrast, draw on judge-level data from Slovenia and find that the presence (or absence) of the quantity-quality tradeoff in judicial decision-making varies across the different types of courts of first instance.

Mindful of the potential drawbacks of measuring the quality of judicial decision-making with appealed cases, a policy implication of our findings in the Bulgarian context is that legal reform may safely emphasize measures aimed at increasing judicial productivity. This conclusion applies in particular in the case of large district courts, for which court-year observations with an above-average volume of resolved cases in fact feature fewer appeals on average than court-year observations with a below-average volume of resolved cases.

7. Conclusion

Judicial efficiency in post-socialist countries has "lagged behind" and judicial reform has been identified as "a critical challenge" (Anderson and Gray 2007: 347). Successful reform of a country's judicial system requires an empirically-grounded understanding of the performance of courts and the behavior of judges. Yet thus far, little systematic empirical research has been conducted on the functioning of courts in the formerly socialist countries. Our analysis of Bulgarian courts represents a step toward filling this void in the literature.

Our results have direct policy implications. With case disposition in Bulgarian courts driven primarily by demand for court services, the effect of increasing the size of the judiciary

as means to further increasing court output, and thus the speed of justice, would be effective only in a subset of Bulgarian courts and, moreover, to a limited extent. However, our analysis also suggests that policymakers interested in increasing court output could implement measures aimed at increasing the productivity of existing judges without worrying too much about the possible adverse effects for the quality of judicial decisions. Such measures include clear articulation of court and judicial performance targets followed by improved monitoring of court and judicial output (see, e.g., CEPEJ 2014: Sec. 5.3), which reduces negative incentives. In addition, further judicial training and productivity-related salary bonuses, which foster positive incentives, might also enhance the quality of judicial decision-making.

Analysis of judge and case-level data would shed further light on the functioning of Bulgarian judiciary. It would also be interesting to see to what extent our findings apply in the context of other post-socialist judicial systems and beyond. More generally, the study of court data from developing and emerging market economies constitutes a fruitful avenue for future research.

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Table 1: Summary Statistics

	Panel A: Large District Courts				
	No. Obs.	Mean	Std. Dev.	Min.	Max.
Resolved Cases	243	10,225.04	16,143.02	1,364	111,718
Number of Judges	243	20.89	24.88	7	165
Caseload	243	12,382.68	21,289.45	1,644	145,969
Appealed Cases	243	1,327.88	2,641.78	0	19,279
	Panel B: Small District Courts				
	No. Obs.	Mean	Std. Dev.	Min.	Max.
Resolved Cases	772	1,341.51	905.66	111	6,147
Number of Judges	772	4.43	2.19	1	12
Caseload	772	1,559.97	1,060.91	121	7,369
Appealed Cases	772	143.60	115.41	0	878

Notes: The table presents summary statistics for the samples of large district courts (Panel A) and small district courts (Panel B) utilized to obtain results in Tables 2 and 4.

Table 2: Baseline Regression Results: Pooled OLS and FE

	Panel A: Large District Courts			
	Pooled OLS		FE	
Explanatory Variables	(1)	(2)	(3)	(4)
Log Number of Judges	-0.0865**	-0.0215	0.0479	0.0532
	(0.0340)	(0.0340)	(0.0754)	(0.0778)
Log Caseload	1.0372***	0.9823***	1.0264***	1.0310***
	(0.0242)	(0.0268)	(0.0370)	(0.0377)
Year FE	No	Yes	Yes	Yes
Court FE	No	No	Yes	Yes
Court Time Trend	No	No	No	Yes
R-squared	0.9947	0.9964	0.9982	0.9982
No. Obs.	243	243	243	243
	Panel B: Small District Courts			
	Pooled OLS		FE	
Explanatory Variables	(5)	(6)	(7)	(8)
Log Number of Judges	-0.0365*	0.0086	0.0295	0.0267
	(0.0184)	(0.0211)	(0.0326)	(0.0350)
Log Caseload	1.0108***	0.9723***	1.0049***	1.0061***
	(0.0125)	(0.0132)	(0.0237)	(0.0233)
Year FE	No	Yes	Yes	Yes
Court FE	No	No	Yes	Yes
Court Time Trend	No	No	No	Yes
R-squared	0.9861	0.9893	0.9950	0.9950
No. Obs.	772	772	772	772

Notes: Heteroskedasticity-robust standard errors clustered at court level in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 3: Instrumental Variable Regression Results: IV-2SLS and IV-GMM

	Panel A: Large District Courts			
	IV-2SLS		IV-GMM	
Explanatory Variables	(1)	(2)	(3)	(4)
Lagged Log Resolved Cases		0.2266***		0.2096***
		(0.0767)		(0.0531)
Log Number of Judges	-0.2021	0.1795	-0.1878	0.0165
	(0.2200)	(0.4332)	(0.1261)	(0.1898)
Log Caseload	1.1202***	0.6363**	1.1250***	0.8030***
	(0.1236)	(0.2808)	(0.1101)	(0.1215)
Year FE	Yes	Yes	Yes	Yes
Court FE	Yes	Yes	Yes	Yes
No. Obs.	189	189	216	189
Hansen over-id. test (*p*-value)			0.936	0.313
Arellano-Bond serial corr. test (*p*-value)			0.797	0.392
	Panel B: Small District Courts			
	IV-2SLS		IV-GMM	
Explanatory Variables	(5)	(6)	(7)	(8)
Lagged Log Resolved Cases		0.2351***		0.1756***
		(0.0639)		(0.0538)
Log Number of Judges	0.0234	0.0795	0.1665**	0.1683**
	(0.1644)	(0.2484)	(0.0807)	(0.0828)
Log Caseload	0.8392***	0.4427*	1.0272***	0.8924***
	(0.1473)	(0.2407)	(0.0488)	(0.0874)
Year FE	Yes	Yes	Yes	Yes
Court FE	Yes	Yes	Yes	Yes
No. Obs.	600	600	686	600
Hansen over-id. test (*p*-value)			0.453	0.190
Arellano-Bond serial corr. test (*p*-value)			0.375	0.464

Notes: Columns (1), (2), (5), and (6) presents IV-2SLS results of the first-differenced model (5), where the differenced Lagged Log of Resolved Cases (for specifications in columns (2) and (6) only), Log Number of Judges, and Log Caseload are respectively instrumented with the second lag of Resolved Cases, Log Number of Judges, and Log Caseload. Columns (3), (4), (7), and (8) present results using Arellano and Bond's (1991) two-step robust difference GMM-IV estimator. The 'collapsed' instruments set (see Roodman 2009) is based on the second and further lags of Log of Resolved Cases (for specifications in columns (4) and (8) only), Log Number of Judges, Log Caseload, and differenced year dummies. The heteroskedasticity-robust standard errors reported in parentheses are clustered at the court level and calculated using the Windmeijer (2005) and small-sample corrections. The null hypothesis for the Hansen over-identification test is that the instruments are not correlated with residuals. The null hypothesis for the Arellano-Bond serial correlation test is that the errors in the first-difference regression exhibit no second-order autocorrelation.*, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 4: The Quantity-Quality Tradeoff in Judicial Case Resolution, Test of Means of Appealed Cases

	Below-Average Output			Above-Average Output			
	No. Obs.	Mean	Std. Dev.	No. Obs.	Mean	Std. Dev	*p*-value
Large District Courts	118	1573.67	3307.01	125	1095.85	1785.95	0.607
Small District Courts	382	139.57	101.18	390	147.54	127.83	0.983

Notes: The table reports results from two-sided, two-sample *t*-tests with unequal variances of the difference in the mean number of appealed cases between Below-Average Output and Above-Average Output observations (see Section 6 for definitions). The variable Appealed Cases was added a one and logged in the calculation of *p*-values (in order to respectively avoid dropping from the sample court-year observations with zero appealed cases and to address non-normal distribution of the data), but were left untransformed for mean and standard deviation comparison in the table.

Figure 1: Time Evolution of the Cross-Sectional Mean of Resolved Cases, Caseload, and Number of Judges, Large District Courts

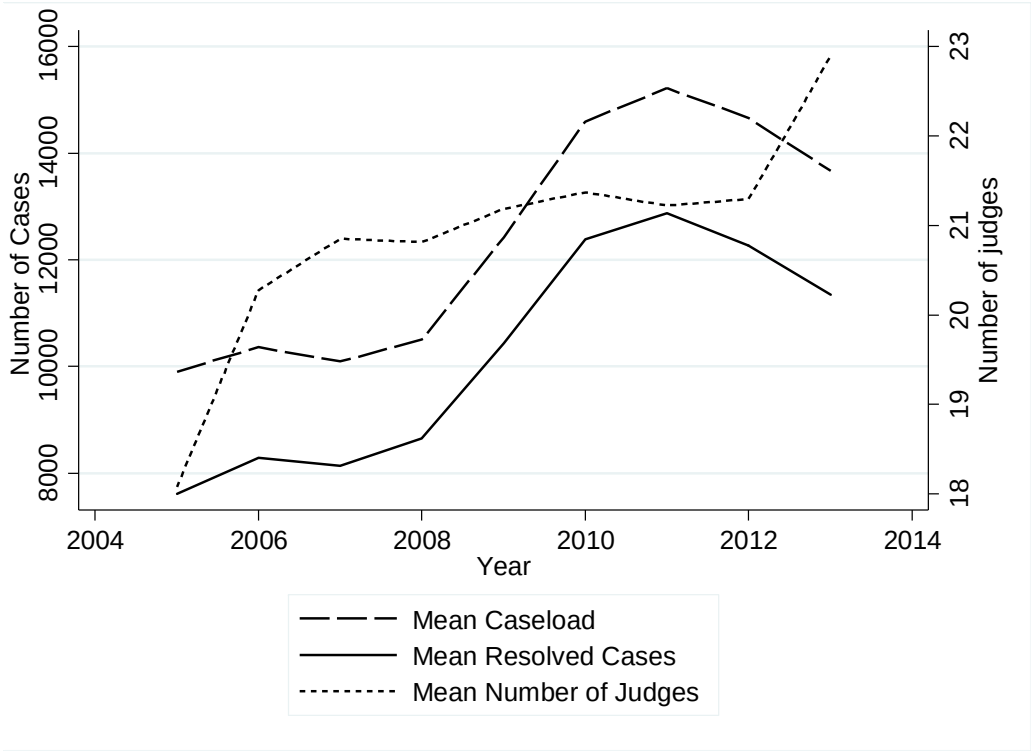

Figure 2: Time Evolution of the Cross-Sectional Mean of Resolved Cases, Caseload, and Number of Judges, Small District Courts

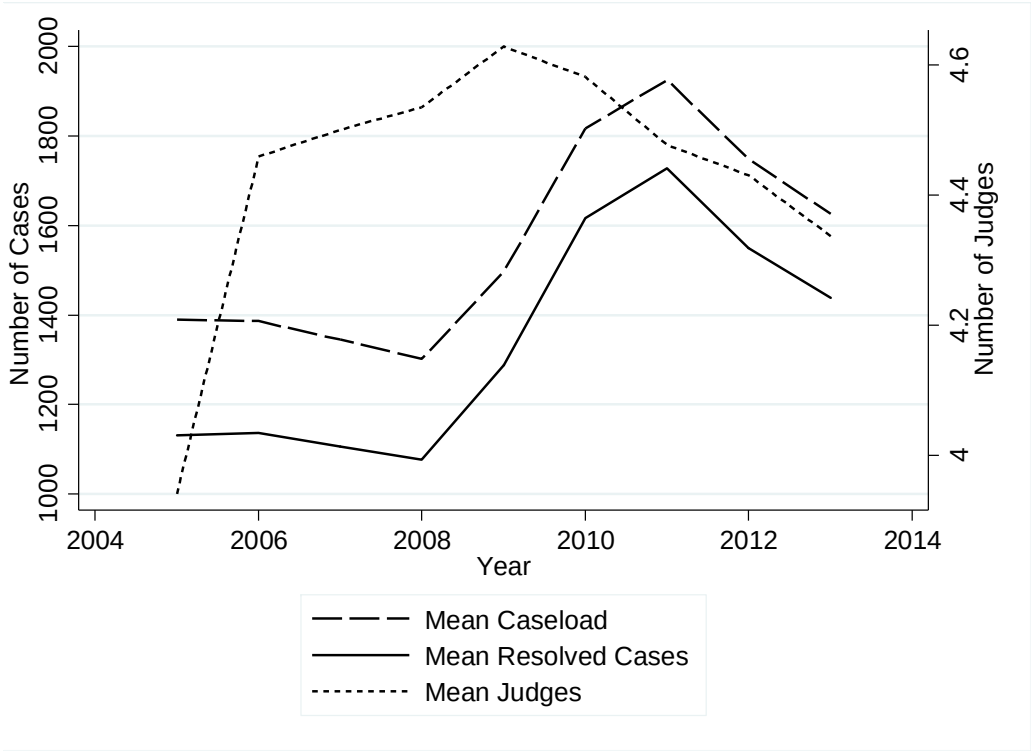

Figure 3: Time Evolution of the Clearance Rate

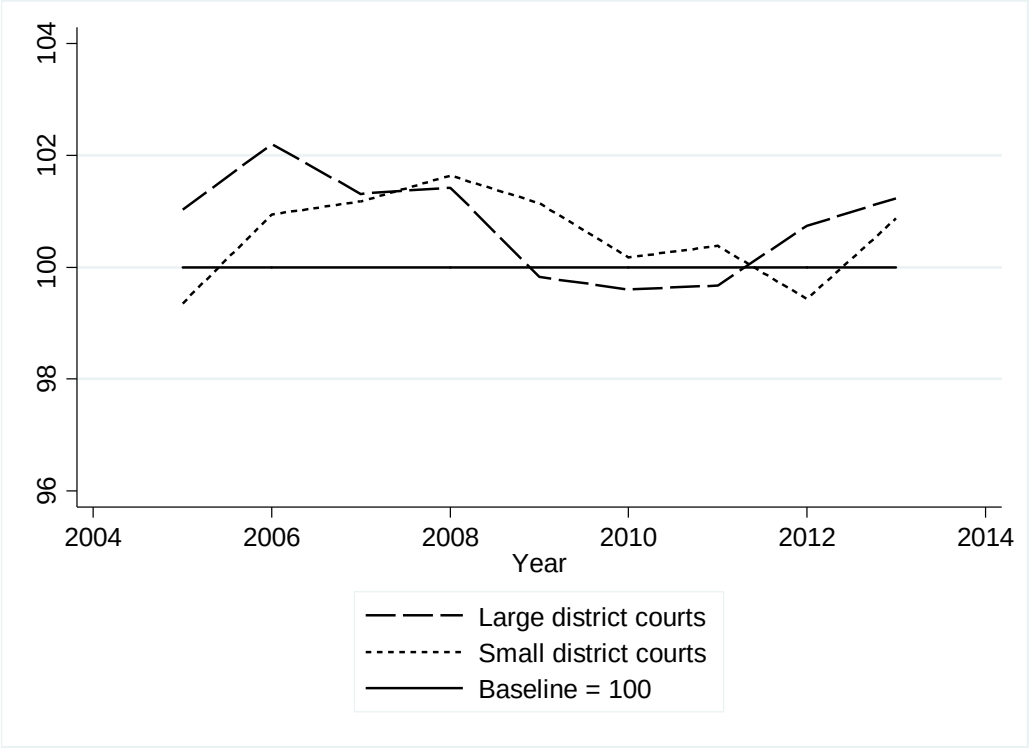

Table A1: First Stage Regressions for IV-2SLS Results in Table 3

	Panel A: Large District Courts				
	Column (1) in Table 3		Column (2) in Table 3		
Explanatory Var.'s \ Outcome Var.	$\Delta Judges_{ct}$	$\Delta Caseload_{ct}$	$\Delta Judges_{ct}$	$\Delta Caseload_{ct}$	$\Delta Resolved_{c,t-1}$
$Judges_{c,t-2}$	-0.0266	0.0734	-0.0279*	0.0731	0.1790***
	(0.0169)	(0.0659)	(0.0162)	(0.0657)	(0.0546)
$Caseload_{c,t-2}$	0.0329**	-0.0445	0.0817**	-0.0320	0.7149***
	(0.0136)	(0.0544)	(0.0388)	(0.1285)	(0.1515)
$Resolved_{c,t-2}$			-0.0495	-0.0127	-0.8773***
			(0.0416)	(0.1271)	(0.1430)
Differenced year dummies	Yes	Yes	Yes	Yes	Yes
R-squared	0.1807	0.4930	0.1849	0.4930	0.5377
F test of excluded instruments	13.52	5.62	12.01	3.85	18.32
	Panel B: Small District Courts				
	Column (5) in Table 3		Column (6) in Table 3		
Explanatory Var.'s \ Outcome Var.	$\Delta Judges_{ct}$	$\Delta Caseload_{ct}$	$\Delta Judges_{ct}$	$\Delta Caseload_{ct}$	$\Delta Resolved_{c,t-1}$
$Judges_{c,t-2}$	-0.0640***	0.0496**	-0.0648***	0.0490**	0.1087***
	(0.0207)	(0.0236)	(0.0206)	(0.0236)	(0.0287)
$Caseload_{c,t-2}$	0.0397**	-0.0454***	-0.0150	-0.0865	0.5708***
	(0.0180)	(0.0167)	(0.0560)	(0.0602)	(0.1063)
$Resolved_{c,t-2}$			0.0566	0.0424	-0.6794***
			(0.0564)	(0.0583)	(0.1170)
Differenced year dummies	Yes	Yes	Yes	Yes	Yes
R-squared	0.0678	0.4116	0.0700	0.4119	0.4797
F test of excluded instruments	7.87	4.14	5.47	2.81	17.41

Notes: Heteroskedasticity-robust standard errors clustered at court level in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.