The Role of Adjudication Procedures in Judicial Timeliness: Quasi-Experimental Evidence

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Abstract

The Italian judicial system is notoriously slow, with an estimated backlog of five million cases. We use a sample of 652,174 court cases in Turin to study the role that various adjudication procedures play in judicial timeliness. We exploit plausibly exogenous variation in the procedures governing how judges rule on small claims and implement a quasi-experimental approach. We estimate the causal effect that different adjudication procedures entailing varying degrees of procedural complexity have on time to disposition. The unique institutional features of the country's small claims court allows us to do so. For any claim valued below €1,100, judges do not need to provide formal legal justification for their decisions. They can rule based on "equità", i.e., fairness, intuition or commonsense grounds. For cases valued above this threshold, judges do not have such flexibility. Our regression discontinuity estimates, which exploit the variation in these adjudication procedures just above and just below this threshold, reveal that when judges are able to rule without providing legal justification, decisions are made one month faster. We document the robustness of our results in light of observable strategic behavior on the part of litigants. We discuss how our results align with recent policy reforms in the realm of small claims including methods to ease congestion in Italian courts and efforts to improve judicial performance more broadly.

JEL Classifications: K41, C21

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1 Introduction

Recent literature has highlighted the importance of the judiciary in order to promote economic development. Understanding the determinants of a well-functioning judiciary has straightforward policy implications, yet the precise components of performance and accuracy in courts often remain unclear. Staats et al. (2005) identified five different dimensions of judiciaries' performance that have been later tested as significant determinants of countries' economic performance: *i)* independence (Voigt et al., 2015), *ii)* efficiency (Ippoliti et al., 2015), *iii)* accessibility (Desau et al., 2019), *iv)* accountability (Voigt, 2008) and, mostly, *v)* effectiveness (Jappelli et al., 2005; Visaria, 2009; Chemin, 2009, 2012; Mora-Sanguinetti et al., 2017; Melcarne and Ramello, 2020). Attention has particularly focussed on the role of judicial effectiveness[1] on economic transactions. Following the well-known legal maxim, *justice delayed is justice denied*, scholars have concentrated their studies mostly on the role of time to disposition: that is, the time needed by a court to adjudicate a case. Research in this area has tried to identify variance in this specific dimension of judicial performance by exploiting cross-country differences (Safavian and Sharma, 2007), lab experiments (Feess and Sarel, 2018), exogenous shocks deriving from changes in legislation (Chemin, 2009, 2012; Visaria, 2009), external factors affecting judges' attention (Clark et al., 2018) or case management deadlines (de Figueredo et al., 2019; Frakes and Wasserman, 2017; Petkun, 2019). We use a quasi-experimental econometric method with a large sample of judicial decisions to investigate how adjudication procedures characterized by different levels of procedural formalism (namely the requirement for judges to give formal legal reasons motivating their decisions) impact the magnitude of the time needed to solve cases in courts. To the best of our knowledge, this exercise is the first to study quasi-random assignment of cases to different adjudication procedures entailing varying degrees of procedural complexity by employing regression discontinuity methods.

Early literature (Buscaglia and Ulen, 1997; Botero et al., 2003; Djankov et al., 2003) claimed judicial timeliness to be mainly determined by procedural complexity, a legal feature originally thought to be derived from a country's legal tradition (Glaeser and Shleifer, 2002). More complex regulation constrains judges' and lawyers' work, slowing down the functioning of the judiciary. The policy implications emerging from these studies suggested that a streamlined judicial procedure would allow courts to deliberate faster, thus ultimately better supporting economic activity. Such suggestions have then heavily influenced the policy proposals of several international organizations including the World Bank's *Doing Business*

[1] See Marciano et al. (2019) for a thorough definition of judicial effectiveness, in what it differs with judicial efficiency and the proper ways to measure both.

Program which in its first report adopted Djankov et al.'s precise approach and metrics. The attractiveness of such suggestions was that better judicial performance was possible by merely simplifying procedures with virtually zero extra public spending: no need to build new courts, hire more judges, invest in information technologies, etc. Later works have then enriched this literature suggesting that legal origins do not explain present countries' variance in procedural complexity (Klerman and Mahoney, 2007; Spamann, 2010). However, also when using new metrics less affected by problems of measurement error as the original indexes by Djankov et al. (2003) were, Spamann (2010) finds empirical evidence of a significant relationship between procedural complexity and courts' timeliness.

Building on these works, we test how the degree of procedural complexity deriving from different adjudication procedures constrains judicial behavior and affects deliberations' celerity, focusing on Italy as a case study for two reasons. First, the Italian judiciary is internationally renowned for its inefficacy and slowness, [2] an institutional feature that has proven to slow down the economy (Melcarne and Ramello, 2020). Second, Italian courts are characterized by a unique institutional feature and one that lends itself to a quasi-experimental research design. We are able to test the impact of different levels of procedural complexity deriving from adjudication procedures on judicial timeliness with case-level data collected from more than 650,000 trials by exploiting plausibly exogenous variation in the amount of flexibility that small claims court judges have when ruling on cases.

One of the key dimensions of procedural complexity is the requirement of legal justification: it matters whether judges must expressly state the legal justification for their decision, citing articles of the law or case law, or whether their decision may rest simply on a more generic notion of justice or fairness.[3] Our study of the effect of procedural complexity is possible through the unique institutional features of the Italian small claim courts system. Italian law demands judges to decide cases with value below or equal to €1,100 solely on the basis of "*equità*," i.e., equity, fairness or a commonsense view of justice. We describe what the *equità* rule means for judges and litigants in detail below, but, simply put, the *equità*

[2] According to the Council of Europe's 2014 "CEPEJ Report," the Italian judiciary's civil backlog was estimated at roughly 5 million cases, creating an average disposition time for a civil lawsuit of nearly 2 years to obtain a first instance ruling. From a comparative perspective, in 2016 the World Bank's *Doing Business Project* ranked Italy 176^{th} out of 194 countries with respect to civil jurisdictions' performance in terms of time to disposition.

[3] Djankov et al. (2003) originally included the possibility of a judgement to be motivated on law or "equity" as one of the determinants of their legal formalism index; the authors also claimed that such procedural characteristic was proper of common law systems. Klerman and Mahoney (2007) however point out that, although similar remedies were first developed in some particular medieval English courts, this concept was alien to the common law courts and, on the contrary, was adopted in those courts more resembling their civil law French counterparts (the Court of Chancery, among others).

rule limits the legal justifications judges are required to provide for their decisions. For cases valued above €1,100, judges have substantially less flexibility and less discretion. These discrete differences in adjudication procedures around €1,100 lend themselves to a quasi-experimental econometric approach studying cases just to either side of the threshold. We implement a regression discontinuity design to causally estimate the impact of the procedural complexity deriving from this institutional setting on time to disposition.

Beyond contributing to the ongoing academic debate on judicial decision-making our work also attempts to supply solid empirical evidence with regards to a recent reform affecting litigants in small claim courts. With the legislative decree No. 116 of 2017, the Italian Parliament decided to more than double the *equità* threshold, raising it to €2,500 to become effective in 2021. This suggests that Italian policy makers predict less formalized adjudication procedures will benefit the average users of small claim courts, such as everyday consumers and small-business owners, perhaps while simulaneously relieving some pressure on courts' backlog.

[Figure 1 about here.]

As can be seen from Figure 1, we find that, just from being marginally above the €1,100 threshold and therefore not having the *equità* rule applied causes cases' disposition time to significantly increase. In our baseline model, the effect appears economically large and highly statistically significant: 138 extra days, with an overall average disposition time in our sample of 175 days. However, our robustness checks mitigate the magnitude of our estimated coefficients and their generalizability to some extent. First, when narrowing our analysis solely to a very narrow window of similar cases around the threshold, the magnitude of our coefficient of interest, despite remaining statistically significant, drops substantially to 32 days, which however would still account for a 18% reduction with respect to the average disposition time. Because the result of 138 days is estimated from a sample containing both high-value cases which are also likely to be more complex as well as lower-value (and therefore simpler) cases, we believe that cases' complexity might play a role in the actual implementation of the *equità* rule. Second, we find that the effect of adjudication procedure on judicial timeliness is not homogenous across different sub-samples of cases. Overall, our case-level dataset together with the regression discontinuity setup supplies novel insights on the causal effect of procedural complexity on time to disposition.

Below we describe the relevant details of the Italian court system including the source of quasi-experimental variation in adjudication rules (Section 2). Then we discuss the data used (Section 3) and explain our empirical approach, a traditional regression discontinuity design. In Section 4 we then present our results and robustness checks including a "donut" regression discontinuity design (Section 4.4.4)

and finally discuss the implication of our findings for policy reforms and future research in Section 5.

2 Institutional Setting

2.1 Italian Small Claims Courts

The Italian legal system follows the civil law tradition. As such, Italian law is mainly codified into statutory law as opposed to judge-made law. The judicial organization in charge of enforcing civil and criminal law is structured in three tiers.[4] At the first level, trial courts (*Tribunali Ordinari*) have jurisdiction over claims filed by plaintiffs. Appeals are directed towards courts of appeals (*Corti d'Appello*). At the apex of this pyramid, a court of last resort (*Suprema Corte di Cassazione*) is in charge of examining final appeals.

In 1995 a separate type of proceeding was added for small claims (according to the amended Article 7 of the Civil Procedural Code, those with value below €5,000) in order to help ease courts' backlog.[5] *Giudici di Pace* ("Justices of Peace") have since had exclusive jurisdiction over these cases. Despite the important number of cases processed by these kinds of jurisdictions, small claims courts have been until now relatively overlooked by the literature, with a notable exception in Niblett and Yoon (2017). Generally speaking, both the organization of these courts and the specific legal procedures their judges must apply are designed to reduce procedural frictions and thus accelerate the resolution of cases. Between 1995 and 2013, this jurisdiction was geographically organized into 846 small claims courts covering the entire Italian territory and employing 4,690 judges. With Legislative Decree No. 156 of 2012, starting in 2013, most small claims courts were merged with others in an attempt to increase economies of scale and save public resources. The reorganization resulted in just 179 courts having jurisdiction over small claims.

Justices of Peace do not have to undergo a national examination as judges in the other branches of the Italian judiciary do. A Justice of Peace needs to be between 30 and 70 years of age, to have earned a law degree and either have practiced as an attorney, or served as a (non-tenured, non-career) lay judge, university law professor or manager in the public administration for at least two years. Judges are appointed to a four-year term that is supposed to be renewed just once for an additional four years. Due to the scarcity of qualified applicants, however, Justices of Peace appointments have often been renewed indefinitely on a yearly

[4] Administrative and tax courts are organized in just two levels. We do not study those here.

[5] Law No. 374 of 1991 introduced small claims courts, but did not became operational until May 1^{st} 1995. Previously, small claims were adjudicated in ordinary tribunals.

basis. Differently from common law countries (Anderson and Helland, 2012) or even other courts in Italy, there are no promotions or longer-term career incentives for Justices of Peace.

Articles 311 through 322 of the Civil Procedural Code govern the more fundamental procedural steps in small claims courts. First, the plaintiff has to file a petition with the court. The case will then be randomly assigned to a judge who will unilaterally fix a date for the first judicial hearing. Only then will the plaintiff have the burden of notifying the defendant. In the petition, the plaintiff has to include all the elements necessary to establish his or her claim and produce all evidence in support of it; the defendant will produce his/her evidence on the day of the first hearing. At the first hearing, if the judge believes there is already sufficient evidence to either grant or deny the plaintiff's claim, the case will be decided on the spot and a formal verdict will be written by the judge and communicated formally to the litigants. However, it is often the case that either for organizational/planning purposes or to allow more evidence to be presented in court, the decision is postponed to a subsequent hearing. Review is mandatory in the sense that judges do not have discretion in choosing which cases to hear: in other words, all cases assigned to them must receive a formal decision. No rule governs how many claims can be filed by litigants, and there is no method to discard or discourage frivolous claims. It is also worth mentioning that, like most civil law countries, the Italian legal system prescribes that litigation expenses are allocated according to the "English Rule" (*loser-pays-all*). All legal expenses (both attorneys' fees and filing fees) are borne by the losing party. In Section 5, we explore how these institutional features may affect litigant strategies.

2.2 "Discontinuities" in Adjudication Rules

In order to test the impact of legal formalism on disposition time, we exploit a unique institutional design created by Italian law. According to the second paragraph of Article 113 of the Italian Civil Procedure Code, Justices of Peace must resolve all cases whose value does not exceed €1,100 solely on what Italian law describes as *equità*, directly translated to English with the meaning "fairness." Below such threshold, judges are not bound to apply substantive law when deciding a case and need not provide a legal justification for their decision.[6] On the contrary they are free to dispose litigation on the basis of a more generic sentiment of justice or fairness. In practical terms, this means that judges are not bound by formal rules in deciding whether and to what extent damages will be awarded or

[6]As already mentioned above, in order to favor less formalized procedure, the Legislative Decree No. 116 of 2017 has revised Article 113 of the Civil Procedure Code, increasing the *equità* threshold to €2,500 effective October 31, 2021.

the exact nature of a non-monetary obligation that needs to be fulfilled. For example, both the Italian statutory and case law normally regulate the quantification of damages to be awarded in a torts case. "Economic" damages (loss of earnings or property damages) need to be determined in strict relation to the corresponding market prices of the services and goods concerned by the case (for example, monthly salaries or the retail price of the damaged good). "Non-economic" damages (pain and suffering or emotional distress) are similarly determined on the basis of specific criteria quantifying a monetary equivalent for any physical or psychological impediment. In the case of a claim above €1,100, the judge must apply these rules in order to calculate damages and account for such application in the decision's motivation. If they do not, the consequent decision could be reversed on appeal. On the contrary, if the case's value does not exceed €1,100, the judge will be free to evaluate damages based on the concepts of fairness and justice.[7] As a consequence, decisions based on *equità* cannot be appealed based on the merits, but solely in the case of procedural irregularities.

From a theoretical perspective one can thus hypothesize that the less procedural formalism judges face when using the *equità* rule should translate into a smoother decision making process and thus require less time to adjudicate a case, which is precisely what our regression discontinuity design aims to estimate.

The institutional discontinuity across the €1,100 threshold allows us to isolate the causal impact of legal formalism so long as no other differences occur at such value that could affect our dependent variable, disposition time. However, one other important change does occur at this threshold. In cases below €1,100, litigants can avoid the requirement of legal representation and thus file a case *pro se*. Because we seek to identify effects of legal justification requirements on judicial decisions alone, rather than the effects of attorney behavior, this change across the threshold could theoretically pose a problem for our identification strategy. And in fact, Italian legislation[8] bans attorneys from being remunerated with contingency fees and thus potentially incentivizing the extension of a case's length, the precise outcome we are measuring. However, our dataset allows us to discern whether litigants opted for legal services and, if so, how many attorneys they employed. As described below in Section 4.4.1, we document no economically relevant differences in the use of attorneys around the €1,100 threshold.

[7] It is important to highlight that the claim's value is defined by the plaintiff when filing the case. Accordingly, this leaves space for strategic behavior on the plaintiff's side, a possibility which we discuss in Section 4 below.

[8] Article 13 paragraph 3 of Law No. 247 of 2012 states that legal fees can be freely negotiated among attorneys and clients according to hourly fees, lump-sum payments, per procedure fees or with a share of the value of claim. However as stated by paragraph 4 of the very same article and Article 1261 of the Italian Civil Code, under no circumstances is it possible to negotiate a fee structure that will be paid only in case of a favorable result for the client.

From an institutional perspective, it is worth mentioning another discontinuity that could have a potential impact on our analysis. Claims submitted with value above €1,033 are subject to a €200 filing fee.[9] Furthermore, being above this €1,033 threshold requires an extra procedural step: before the case can be declared resolved, it is necessary for the judicial administration to file a request to the fiscal authority in order to apply the fee. This step can take some time and could therefore influence our dependent variable, disposition time. We explore methods in Section 4.4.3 to disentangle the effects of the filing-fee threshold from the *equità* threshold.

3 Data Description

[Table 1 about here.]

Table 1 provides summary statistics of the two main variables of interest in our dataset. The data contain the universe of small claims submitted in the Turin district from January 1995 to August 2017 gathered from the Italian Ministry of Justice. Once cleaning the dataset of incomplete entries and typos our sample consists of 652,144 cases. The most common type of case is debt recovery,[10] followed by torts and contracts. The dataset contains other types of cases, such as property disputes, but we focus on the three most common types. Our main outcome of interest, motivated by the well-known inefficacies in the Italian court system described above, is the number of days it takes a judge to close a case, an outcome which we term *time*. We are able to precisely measure disposition time as our dataset contains, for each case, the date the claim was filed by the plaintiff and the date of the final decision on the case. Table 1 shows that cases are slow to resolve overall: the average adjudication time for all claims in our dataset is 175 days, with the average torts case taking nearly 10 months (302 days). In Table 2, we also show the raw data on how disposition time differs across the €1,100 threshold, for various ranges of the data around €1,100. We describe these subsamples, called "bandwidths" in more detail in Section 4.3. The first row in Table 2 shows that in the raw averages, there is a dramatic difference in disposition time when crossing the €1,100 threshold, strongly suggestive of the hypothesis that *equità* procedures lead to quicker resolution of cases. From Table 2, we see that for all

[9]The reason this threshold value is not a round number derives from the fact that it has been directly converted from the previous threshold in Italy's former currency (*lira*), £2,000,000; the official conversion rate being: €1=£1,936.27.

[10]The Italian Civil Procedure Code provides for a specific legal instrument, *procedimento d'ingiunzione*, characterized by a rather simplified procedure, that allows creditors to obtain an immediately executive order of payment from a judge (*decreto ingiuntivo*).

types of cases, the average disposition time for cases below the €1,100 threshold is 103 days and for cases valued above the €1,100 threshold it is 326.5 days.

[Table 2 about here.]

Table 1 also shows the mean and median of the case values. Recall that small claims court in Italy only handles cases with a value no greater than €5,000. The overall median case value is €703, Torts claims are the highest value with mean value of €2,052 and median of €1,673.

4 Empirical Analysis

4.1 Methodology

The adjudication procedure a judge will use to rule on a case is determined solely by the size of the claim. Claims with a value below €1,100 will be decided based on *equità*. For claims valued at €1,100 and above, judges will use standard civil law procedures, justifying their decisions with substantive law. Therefore, we can study the causal effect of allowing judges to use *equità* by comparing outcomes for claims just below €1,100 and those just above €1,100. The so-called regression discontinuity design is a quasi-experimental approach (Imbens and Lemieux, 2008) well suited for this type of situation. Under a number of assumptions, which we describe and test below, one can treat cases as randomly assigned to their adjudication procedure. Therefore, estimates of the difference in disposition time for cases just above and just below the threshold can be interpreted as the treatment effect of the differential adjudication process.

The main assumptions of a regression discontinuity (RD) design are: 1) that observations on either side of the threshold are similar on all other relevant characteristics and 2) that the "running variable" in our setting, that is the value of the case ("*valore*" in Italian), is not manipulated by litigants. In our setting, violation of the latter could take the form of litigants submitting, say, a claim worth €1,000 instead of €1,200, thinking that the *equità* procedures used to evaluate claims below €1,100 would result in a faster and thus more favorable ruling for them. Below we describe our tests for these two assumptions, ie., that covariates are balanced around the €1,100 threshold; and that there is no bunching of *valore* near the threshold. We begin by describing the RD procedure and presenting our baseline estimations.

We use a standard RD framework that takes the form of Equation 1 below. Our coefficient of interest is α_1, the coefficient on an indicator variable for a case having a value, or *valore*, above €1,100. In the RD framework, this coefficient can

be interpreted as the local treatment effect.

$$\begin{aligned} time_c =& \alpha_0 + \alpha_1 \mathbb{1}\{valore_c > 1100\} + \beta_1 (valore_c - 1100) \\ &+ \beta_2 \mathbb{1}\{valore_c > 1100\} * (valore_c - 1100) + \beta_3 valore_c^2 + \gamma X_c' + \epsilon_c \end{aligned} \tag{1}$$

As is standard practice, our regressions include the distance between *valore* and the threshold and an interaction of that distance with the indicator for a case having *valore* above €1,100. We control for *valore* with a quadratic polynomial.[11] X_c is a vector of case characteristics which include year fixed effects, the number of litigants and the number of attorneys, where c indexes each case.

4.2 Results

Our baseline results are shown in column 2 of Table 3 and Figure 1. One nice feature of the RD design is the ocular test: effects should be discernible with the naked eye. One can clearly see our results in Figure 1, which plots the number of days it takes for a judge to rule on a case (*time*) on the y-axis against our running variable *valore* on the x-axis. The vertical line at €1,100 denotes the threshold below which *equità* procedures are used. Polynomials of best fit to either side of the threshold are shown. The raw data plotted in Figure 1 shows a strong effect: cases where standard civil law procedures are used take substantially longer. Results are confirmed by the regression results in Table 3. Referring back to Equation 1 above, our coefficient of interest is the indicator variable for being above the threshold, α_1. Column 2 of Table 3 reveals an effect of an additional 138-days of adjudication time for cases above €1,100 relative to those below such threshold. The effect is statistically significant at the 1% level. Worthy of mention is the coefficient for the number of attorneys. As emphasized above, given their specific pay structure, lawyers are incentivized to prolong cases: every additional attorney participating in the lawsuit increases cases' disposition time of 85 days.

For robustness purposes, we also run regressions separately by case type. The results for contract, torts and debt recovery cases can be found, respectively in column 1 of the three panels of Table 4 and are represented graphically in Figure 2. We find that the *equità* rule implies an additional disposition time of 140 days for debt-recovery cases, 189 days for contracts cases and 73.5 days for torts cases. Again, all coefficients are significant at the 1% level.

[Table 3 about here.]

[11] Motivated by the recent research documenting the potentially problematic use of higher order polynomials to control for the running variable, (Gelman and Imbens, 2017; Card et al., 2014), we run this simple specification with a quadratic in case value.

We next explore the results for small bandwidths of data close to the threshold. The RD approach is only valid as a quasi-experimental approach to the extent that data on either side of the threshold are similar on all characteristics except for which side of the threshold they fall. Including cases of both very high and very low value as we do in the full sample regressions may not be optimal. For example, these cases may differ in complexity and more complex cases could take judges longer to resolve. We test explicitly for this complexity effect in Table 2, discussed below in Section 4.4.1.

[Figure 2 about here.]

4.3 Optimal Bandwidths

Relying on the growing literature documenting best practices in RD designs (Calonico et al., 2018; Imbens and Kalyanaraman, 2012), we next rely on optimal bandwidth estimators to guide us in selecting sample sizes around the threshold. Such estimators calculate a range, or "optimal bandwidth" around the threshold for which to limit the specific sample. These optimal bandwidth choices are derived from an estimation technique that trades off the need for sufficient econometric power while simultaneously restricting the sample to observations not so far from the threshold to be materially different except for which side of that threshold they fall.

Columns 4 and 6 of Table 3 show the main regression results restricting the sample to two optimal bandwidth estimators[12]. These recommended bandwidths range from about €31 to €37 on either side of the threshold. Even when applying the most restrictive bandwidth, our estimates remain statistically significant at the 5% level. However, the magnitude of our estimated coefficient substantially drops, suggesting that, once restricting our sample, the effect of the *equità* rule decreases to just a little more than one month of additional time. Such results deserve a few words of further explanation.

As emerges from Figure 1, regardless of the jump at the threshold, time to disposition seems to be consistently increasing in case value. If one interprets case value as a proxy for a case's complexity, this would suggest that apart from procedural formalism (as expressed by the *equità* rule), cases' complexity also appears to be a relevant determinant of judicial timeliness. This result is confirmed if we

[12]In calculating the optimal bandwidths, we rely on the recent work of Calonico et al. (2018), who prove that the most popular and simple "mean square error" (MSE) optimal bandwidth is biased and asymptotically invalid. The authors derive a new "coverage error-rate" (CER) optimal bandwidth algorithm which "trade[s] off coverage error against interval length...conceptually analogous to trading size and power." For more on both MSE and CER optimal bandwidth calculations see Calonico et al. (2018) and Imbens and Kalyanaraman (2012).

look at the other estimated coefficients displayed in Table 3. When we estimate in column (2) our full model on cases with value between €0 and €5,000 we find that case complexity (as represented by the "$valore - 1100$" coefficient which simply measures the distance from the threshold) is a highly statistically significant determinant of time to disposition: *ceteris paribus*, every additional €100 of case value increase adjudication time by 5.2 days.

When we restrict our analysis to a narrower sample of cases around the threshold as we do with the optimal bandwidths, we end up comparing much more alike cases, as the assumptions of regression discontinuity design impose. The fact that once we restrict our analysis to cases with little variance in complexity, the magnitude of our coefficient of interest (*equità*) decreases so much, supports the hypothesis that there is an interplay between procedural formalism and cases' complexity. Such a claim would not undermine the validity of our findings, as legal formalism remains a statistically significant (even if weaker) determinant of time to disposition also when considering narrow samples around the threshold; on the contrary, case complexity loses all significance in columns 3 through 6. However, it could indeed limit the potential generalizability of our results in terms of policy implications: as claimed by Djankov et al. (2003) formalism matters, even to a greater extent than cases' complexity, but its impact should be considered in the broader picture.

In columns 2 and 3 of Table 4 we replicate the use of our optimal bandwidth estimators with our three different case subsamples for robustness. Results are somewhat mixed. As in the estimation on the entire sample, coefficients' magnitude decreases as we narrow the bandwidth, but to a lesser extent. However, we find that only contract cases' time to disposition is significantly affected by the *equità* rule. The downside of using the optimal bandwidths and simultaneously narrowing our subsample by case type is the reduction of statistical power in our empirical analysis: often just a few hundred cases remain (as in the torts subsample). Nonetheless, the coefficients' reduction with smaller subsamples seen here would point at the fact that the *equità* rule does not operate uniformly across different case types thus suggesting more caution when generalizing our estimates in favor of a "one-size-fits-all" policy recommendation. We next test the assumption of manipulation of the running variable across the threshold.

[Table 4 about here.]

4.4 Robustness

4.4.1 Balanced Covariates

A key assumption of the regression discontinuity strategy and one that is necessary to interpret our regression estimates as the causal effect of adjudication procedures

is that observations falling on either side of the €1,100 threshold are similar. If it were the case that observations just above and below €1,100 were different in important observable (or unobservable) ways, our estimations will be picking up jointly the effect of adjudication procedures and other causes of the increase in time to disposition, rather than the adjudication procedure alone.

In Table 2, we test whether the observable covariates are balanced across the threshold. Because personal information on litigants in our data is anonymized, we do not have a robust set of observable characteristics on the parties involved in the litigation. We do, however, have, information on four covariates to test for this balance across the threshold: the number of litigants in each case, the number of attorneys, and the day of the week and month that cases were submitted. As mentioned above, the number of attorneys in particular could be critical in interpreting our results since rules governing the use of attorneys do change across the threshold. Summary statistics for these four covariates are in Table 2. For each covariate, we show the mean of the data above and below the €1,100 threshold. Given our large sample sizes, most t-tests for differences in covariates in Table 2 do show significance at standard levels. However, the economic magnitudes of the differences are trivial: there are no detectible or meaningful differences in any of these covariates: All cases have about 1 attorney, 2 litigants (one defendant, one plaintiff) and are submitted uniformly across days of the week and month. This implies that the possibility of claimants litigating *pro se* below the threshold does not appear to affect our analysis because practically speaking, use of attorneys is not measurably different across the threshold. In fact, when further disaggregating data, it emerges than in 84.4% (83.6% for cases below the threshold and 86.1% for those above) of the entire sample of cases, only the plaintiff's attorney is present.

Results of our tests for differences in covariates around the threshold also hold for our optimal bandwidths discussed in the previous section. Again, the samples above and below the threshold are similar. Here the share of cases with just plaintiffs' attorney present increases to almost 87%. Surprisingly, when zooming in on our narrower bandwidths it appears that, if a slightly greater number of attorneys is present, it is for cases *below* the threshold, not for those above; thus pointing away from the idea that attorneys' presence inflates disposition times. Any contrast in attorneys' presence across different samples is mainly due to the fact that in the full sample we include a number of cases (with very low value) in which both parties decide to litigate *pro se* (recall from Section 2 that by legislation, cases below the threshold do not require attorney representation). When only considering alike cases with similar claim values very near the €1,100 threshold, the share of cases without any representation becomes very similar, thus also eliminating the difference in the average of the number of lawyers. It remains a possibility that attorneys play a role in the litigation process and its duration but given these

results together with our other robustness checks, we believe any attorney effects are swamped by effect of adjudication procedures.

Given the results in Table 2 that case characteristics do not seem to differ materially above and below the threshold, we have confidence that the adjudication procedures are likely the only important factor changing around the €1,100 threshold. We next move on to additional robustness checks.

4.4.2 Bunching Around the Threshold

In this section we address the issue of manipulation of the running variable, i.e. whether litigants can adjust the value of their claim in a way that would bias our regression discontinuity results. One manifestation of bunching of data around the threshold that would affect the interpretation of our results is if litigants chose to submit a claim with a lower-than-true value in order to have the simplified *equità* procedure applied to their claim[13]. Alternatively, litigants could inflate the value of their claim in order to avoid *equità* rules. From a series of interviews that we conducted with several Justices of Peace in Turin's court and legal practitioners in the same jurisdiction, it has emerged that it is indeed possible for litigants to alter the value of their claim. While such endogeneity of the running variable is worrisome for our empirical procedure, results of the following tests suggest such manipulation is not done to avoid or seek out a certain type of adjudication procedure but rather to avoid the filing fee assessed to claims above a wholly different threshold.

[Figure 3 about here.]

The first and simplest test to uncover manipulation of the running variable is to plot a histogram of the raw density of the data, as shown in Figure 3. The figure shows the raw number of observations in €50 bins of case value. It is obvious from the figure that there is bunching of the running variable, but this bunching is not at €1,100 (as one would predict if litigants were manipulating their claims to avoid certain adjudication procedures). The noticeable peak in the histogram is in fact at €1,032. As outlined above, litigants submitting claims valued at €1,033 and above must also pay a €200 fee. It appears obvious that litigants are manipulating their claims to avoid paying this fee. Figure 3 also shows that the running variable trends smoothly through our main threshold of interest at €1,100. What also works in our favor is that both of our optimal bandwidth estimators naturally restrict the

[13]It is possible that the manipulation around €1,100 causes downward bias in our estimates. If plaintiffs with more complicated claims are wanting a quick resolution, they may shave euros off the true value of their claim to get it below the 1,100 threshold. This would mean more time-consuming cases on the judges' dockets below the threshold, leading our regression estimates of the difference in disposition time to be lower than the true value.

data to a window around €1,100 that does not include €1,033. Nonetheless, as dramatic bunching is uncommon in a pure regression discontinuity design, we continue to unpack what it means for the validity of our results in the following section.

4.4.3 Bunching and Strategic Litigant Behavior

In this section we discuss the observed bunching along with other forces potentially guiding litigants' behavior. Problematic to the validity of our regression discontinuity design would be a non-random sorting or selection of cases valued just above and below €1,100 arriving on judges' dockets.

First, we note that, as shown in Figure 3, the natural slope of the distribution of cases is decreasing in claim value, *i.e.*, there are more small-value cases than large ones. Second, while there is substantial (and potentially worrisome) bunching around the €1,032 threshold (evincing litigants' knowledge of the €200 fee and successful efforts to avoid it), the case value distribution trends smoothly through €1,100. These facts point to litigants having more knowledge of and/or stronger preference to avoid the €200 euro fee than they favor or disfavor *equità* procedures.

Despite the fact that case value trends smoothly through our threshold of interest and that our optimal bandwidths are so narrow as to exclude any values directly affected by bunching, it could still suggest a problem for the research design if manipulation in the running variable differentially affects the remaining distribution of cases above and below €1,100. Of particular concern is the close proximity of the €1,033 threshold to our threshold of interest at €1,100.

Suppose litigants with cases of true value just above €1,100 were more likely to bunch at €1,032 than those with cases truly valued just below €1,100. This would lead to a selection into which cases we, as the econometricians, observe just above and below €1,100, and thus bias our regression estimates. We argue next that the cases drawn to €1,032 will be drawn nearly symmetrically from around €1,100.

Consider which litigants have the strongest incentive to manipulate the value of their claim. First, holders of claims at and above €1,233 lack *any* incentive to avoid a €200 fee by devaluing their claim by more than €200. Those with a claim below €1,233 and above €1033 are the only ones with some incentive to avoid the fee. Moreover, the incentive to manipulate is decreasing linearly from €1,032 to €1,233 as larger euro amounts of claim must be forgone to avoid a fixed €200 fee.

[Figure 4 about here.]

We illustrate these effects with theoretical functions in Figure 4, where we plot an "ideal" distribution of claim value. For simplicity and without loss of general-

ity, in the figure we focus on an optimal bandwidth of €30 (close to our actual calculations). We plot in green, based off of the histogram of real-world data, an approximation of the ideal distribution of case value, *i.e.*, one without strategic bunching. We also reasonably assume that case complexity, a potentially important factor in determining judicial timeliness, is a concave function. In fact our estimates from column 2 of Table 3 confirm that time to disposition is increasing concavely in claim value, with a very mild adjustment downwards above €1,100. This is shown in blue. What we term the "manipulation function," shown in red, plots the likelihood of manipulating one's claim downward to avoid the filing fee. It is decreasing in case value, with a support of €1,033 to €1,233. Notice that there is no plausible theoretical reason to observe anything but a smooth trend in both the manipulation function and the complexity function as we cross €1,100. Hence, we do not think there is empirical evidence nor theoretical foundation for ascribing our results to complexity or bunching, neither alone nor in concert: Complexity and bunching do change in case value, but they do not do so discreetly at €1,100. While comforting for our research design, this "ideal" figure does not take into account the known bunching at €1,033. For that we go to Figure 5.

[Figure 5 about here.]

Here we plot a distribution function more reasonably resembling the true nature of our data with substantial bunching at €1,032. Bunching at €1,032 of course leads to a shrinking of the dotted and gray areas around €1,100 relative to what we plotted in Figure 4. Again, the incentive to modify one's claim to €1,032 is decreasing in claim value, but importantly not changing discontinuously at the €1,100 threshold. The amount and composition of data in our samples above and below the threshold (shown in the dotted and gray areas) will be a function of a) our chosen bandwidth, which was calculated to be around 30, b) the overall slope of the distribution (known to be downward) and c) the slope of the so-called "manipulation function." Whether the manipulation function or alternatively the distribution function is steeper determines whether we have more data to the left- or right-hand-side of €1,100. Table 2 informs us on this point. In summarizing the optimal samples above and below the threshold we see that we have about 25 percent more observation in the range of €1,070-1,100 than €1,101-1,130, suggesting some sort of imbalance. We argue that this imbalance does not lead to a bias in our estimates because data are being drawn to €1032 nearly symmetrically from both above and below €1,100 toward €1032. Likewise, with the complexity function, complexity is increasing in case value over large ranges, but not changing discretely around €1,100. Further, we believe a reasonable assumption is that complexity is changing immaterially over such a small range like €1,070-1,130.

[Figure 6 about here.]

These hypotheses are borne out in the actual kernel density plotted from our data shown in Figure 6. Most importantly, a smooth trend through €1,100 exists. The density plot also reveals the pulling of data towards €1,032, with a change above €1,233 where the incentive to bunch disappears.

Overall these exercises and our robustness checks give us confidence that despite the observed bunching in our running variable, our regression discontinuity approach is estimating the effect of adjudication procedures on judicial decision making, not remnants of strategic litigant behavior or other factors. Finally, we remind readers of the fact that cases are literally randomly assigned to judges. The distribution of cases that arrive on judges' dockets of course matters, but any and all manipulation of claims is done on the part of litigants, not by those whose behavior we quantify here: judges.

As emerges from the baseline results shown in Figure 1 and Table 3, the quantitative effect of *equità* seems to be very big at first glance. However, a closer inspection of the very same figure suggests that part of that large ocular effect might be driven mechanically by the subset of cases needing be sent to the tax authority. Nonetheless even when considering small bandwidths that exclude data near that tax threshold of €1,033, we see that mitigated effect is still present as we showed above. Next we present new evidence to further tackle potential confounding issues such as these.

4.4.4 "Donut" Regression Discontinuity Method

Additionally, we implement a relatively new modification to the standard regression discontinuity method appropriate for settings with imbalance near the threshold of interest or when heaping of data is suspected. This method, the "donut" regression discontinuity design, is so-called because it estimates regressions for a subsample of data that excludes observations just to the left and right of threshold where heaping exists, forming what looks like a donut hole cut out of the density function. Estimations are done via a traditional regression discontinuity design on the remaining data.[14] In our setting, the heaping exists at €1032 so to implement the donut design, we restrict the sample to include data only outside of the peak in the histogram. The results of this method, described below, are largely consistent with our baseline estimates in magnitude and statistical significance, though we lose significant power when we implement the donut design separately by case type since sample sizes shrink dramatically.

[14]The method is described in detail in Barreca et al. (2011, 2016); Almond and Doyle (2011). It has been most prominently used in settings studying newborn birthweights that are known to be recorded in round numbers resulting in bunching or heaping in the distribution of weight. Eggers et al. (2015) also discuss the benefits and drawbacks of the donut rd as a solution to studying election results that exhibit sorting around the majority threshold.

We follow the method of Barreca et al. (2016) most closely and drop the "donut hole" data (*i.e.*, from 1032-1168, which is €69 of cases' value above and below the threshold of 1100.)[15]. Results are shown in Tables 5 and 6. Table 5 shows the donut regression discontinuity for all types of cases. The results vary (depending on the bandwidth size) from about 137-188, all statistically significant at the 1% level.

In Table 6, we run the donut RD specifications separately by case type: contracts, torts and debt recovery. Again we run the regressions using first the full sample minus the "donut hole" (shown in Column 1), and then restrict that sample further to the two optimal bandwidth estimators (Columns 2 and 3). As we slice the data finer, power becomes an issue, but estimates are similarly signed and general magnitude to the main specifications (shown in Column 1 of Tables 3 and 4). Once we restrict to the sample by dropping data outside the optimal bandwidth, only estimates on debt recovery are significant and not any more for contracts, somewhat of a puzzle.

Together the results of these robustness checks give us confidence in the main results. While there is known heaping at €1033, our estimates of the effect of adjudication procedures are likely not driven by those data points as regression estimates are robust to dropping those observations via the donut method.

[Table 5 about here.]

[Table 6 about here.]

5 Discussion

Our work brings a large new dataset and state-of-the-art econometrics to inform the long line of literature on judicial decision making and the debate concerning the impact of adjudication procedures on judicial timeliness. Our estimates reveal that using the *equità* rule makes for swifter decisions by small claims judges, even if its effect appears to vary across different case types. We believe our empirical strategy plausibly isolates a causal effect of the *equità* adjudication procedure and

[15]For calculating optimal bandwidths under the donut RD specification, we follow Cattaneo et al. (2019) and Almond and Doyle (2011) who provide conditions on the exclusion of observations with a donut approach and the implied new optimal bandwidths. To calculate our new optimal bandwidths, we drop the observations in the so-called donut hole (1032-1168) and then rerun the same two coverage-error rate optimal bandwidth estimators we use in our main specifications. Our optimal bandwidths for the donut RD are 128.21 and 109.72. Given the small sample sizes these bandwidths imply, we also rely on the work of (Barreca et al., 2011) who discuss the fact that they cannot use the donut approach without "substantially" increasing the bandwidth, sometimes including the full sample outside the donut hole, which we also do in Column 1 of Tables 5 and 6.

that we can reasonably rule out alternative explanations such as compositional differences in case type around the threshold, attorney incentives changing with case value, and extra adjudication time due to filing fees.

Assuming the robustness of our methodological approach, our results would seem to confirm to a certain extent the rationale of a recent reform of Article 113 of the Civil Procedural Code enacted by the Italian Parliament which will more than double the level of the *equità* threshold to €2,500 beginning in 2021. In our most conservative scenario, our estimates suggest that allowing judges to apply the *equità* rule would imply saving around 18% of the average time needed to dispose a case. Accordingly, shifting the threshold to the right, *i.e.*, increasing the value below which judges can avoid presenting the legal justification of their decision, could have an impact on the celerity of their decisional process and, thus, a clear reduction in courts' congestion.

One possible improvement of such reform emerging from our analysis would be to adjust the *equità* threshold to different case types. As our estimates suggests, the effect of this adjudication procedure seems to lack uniformity among various kind of lawsuits. Accordingly, higher *equità* threshold for contract cases would turn out to efficiently improve judicial timeliness, while the same cannot be said, for example, for torts cases.

Even if evidence is not conclusive, support for this reform might also come from the fact that a similar improvement in judicial performance (even if mild) would have the further advantage of being achieved at virtually no cost, at least financially (we discuss below the potential drawbacks for this reform). Increasing the *equità* threshold comes with no need to hire more judges or improve the management system of the justice sector. Such a policy need not require a direct increase in public expenditure, something that is generally feared by policy makers nowadays due to, for one, the current European Union's regulation on budgetary deficits.

Nonetheless, the most important question left unanswered is whether Justices of Peace are making simply faster decisions when they use the *equità* rule, or possibly also worse ones. This is a much debated question that unfortunately is left unanswered by our analysis, because of the lack of data regarding the quality of decisions. The dilemma between quality of justice and judicial performance is well-known in the literature (Marciano et al., 2019). Economists tend to put greater emphasis on the "production" of justice, in terms of number of cases solved or their disposition's speed. Other constituents such as lawyers are usually concerned with a judiciary that solves cases "correctly." Accordingly, one could claim that a fast judge is not necessarily delivering good justice. However, taking more time to decide rather than relying on discretion might not necessarily produce better judgements (Guthrie et al., 2007). This issue is of particular interest when

dealing with judicial justification, as in the case of the *equità* rule we study. As correctly emphasized by Cohen (2015), in western liberal-democratic societies, judges' reason-giving is motivated by the need of participation, accuracy and accountability. Nonetheless, these values still need to be "balanced" with other needs, like timeliness, as suggested by the well known legal principle: *justice delayed is justice denied*. Such principle has found numerous applications in a number of countries' legislations: from Article 6 of the European Convention on Human Rights to the US federal statutes regulating "slow" judges (Title 28, §476(a)(3)).

The *equità* rule can thus be interpreted as some sort of compromise between these two opposing necessities. This does not rule out the possibility that judges use their discretion inappropriately. However, previous studies trying to test the relationship between judicial timeliness and quality of justice in other institutional settings have failed to discover a deterioration of quality deriving from faster judiciaries (Buscaglia and Ulen, 1997; Djankov et al., 2003; Rosales-López, 2008; Dimitrova-Grajzl et al., 2012; Melcarne and Ramello, 2015; Dimitrova-Grajzl et al., 2016; Marciano et al., 2019; Melcarne et al., 2020). It is also worth mentioning that, the fact that a similar institutional setting is applied only to cases involving relatively small stakes mitigates this tradeoff. Accordingly, the potential damages caused by an "excess" of discretion accorded to judges will be limited compared to the improvement in terms of celerity that the whole judicial system would enjoy. Despite difficult (if not impossible) to quantify, when trying to evaluate the overall impact of a reform increasing the *equità* threshold, it is reasonable to expect that such hypothetical downsides are to be easily offset by the gains in timeliness. In fact, according to various indicators[16], judicial ineffectiveness appears to be a much more serious problem in Italy rather than judges' discretion and accountability. Despite its public sectors inefficiencies, Italy is a country in which the principles of the Rule of Law are deeply established and the judiciary's independence and accountability are balanced as in most other western democracies.

An upshot of our results is the discovery of some potentially puzzling litigant behavior. While litigants appear to try to circumvent the €200 filing fee as evidenced by submitting claims just small enough to avoid paying, we do not observe litigants responding similarly to the *equità* threshold. Given how quickly *equità* cases are resolved, it is somewhat surprising that litigants with claims close to the threshold do not try to take advantage of a swifter outcome. One explanation

[16] According to the World Bank's Doing Business 2019 Report, Italy ranks 58^{th} out of 191 countries for its overall institutional qualities favoring economic transactions. However, when it comes to evaluate the celerity of its judicial system (the "time" metric in the Enforcing Contract set of variables) Italy drops to the 173^{rd} position, with an average disposition time of 1,120 days, against a world average below 650. According to a different report, the 2020 Rule of Law Index by the *World Justice Project*, Italy ranks 27^{th} out of 128 countries. When focussing on the effectiveness of the civil justice system, the ranking falls to the 112^{th} position.

is that neither plaintiffs nor their attorneys (who may also opportunistically not reveal similar information when known) have knowledge of the potential benefits associated with this procedure.

The loser-pay-all rule also raises interesting strategic issues. Willingness to submit a claim that requires a €200 filing fee that is close to the €1,033 threshold reveals some optimism in having that fee reimbursed. Unless you have a high level of confidence that you will be reimbursed for your filing fee, it would be a pure financial mistake to submit a claim for, say €1,100, and pay on top of that €200, rather than submit a claim below the €1,033 threshold that requires no filing fee. This is especially curious given the small stakes, and that €200 euros is a relatively large fee in percentage terms to have one's case heard. We hope future research will shed light on these unanswered questions and puzzles within the behavior of litigants, attorney and judges in the world of small-claims.

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Figure 1: All Cases

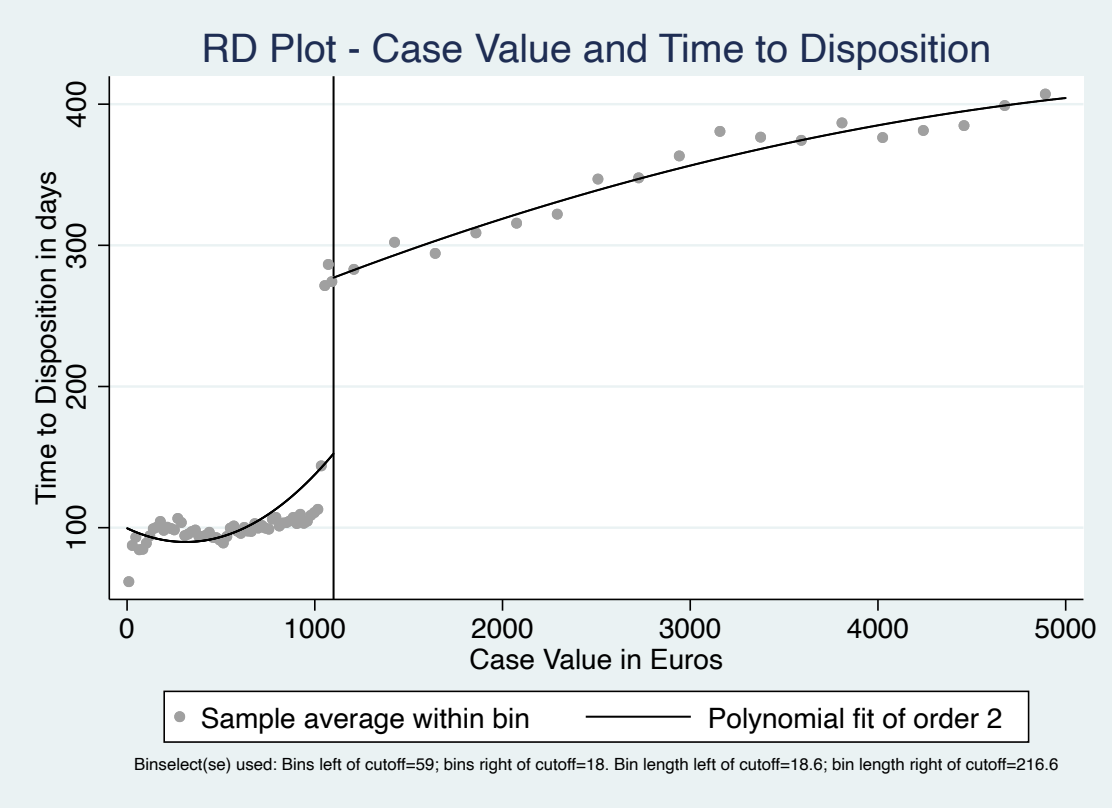
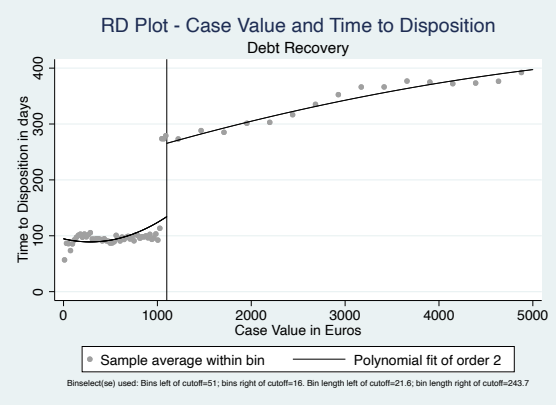
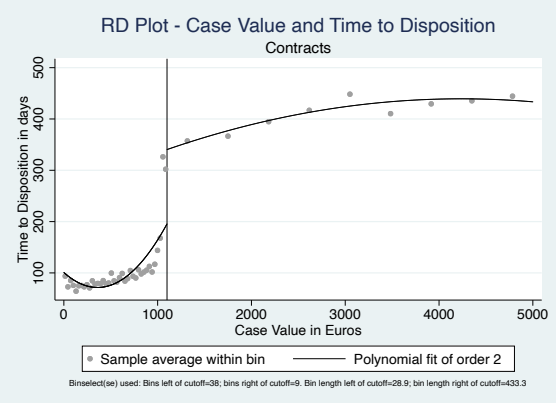


Figure 2: Subsamples of Cases

(a) Debt Recovery



(b) Contracts



(c) Torts

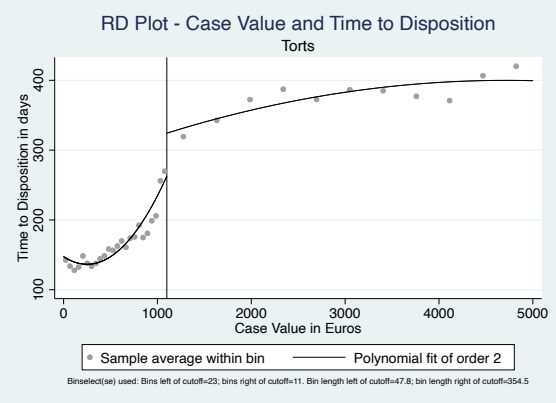
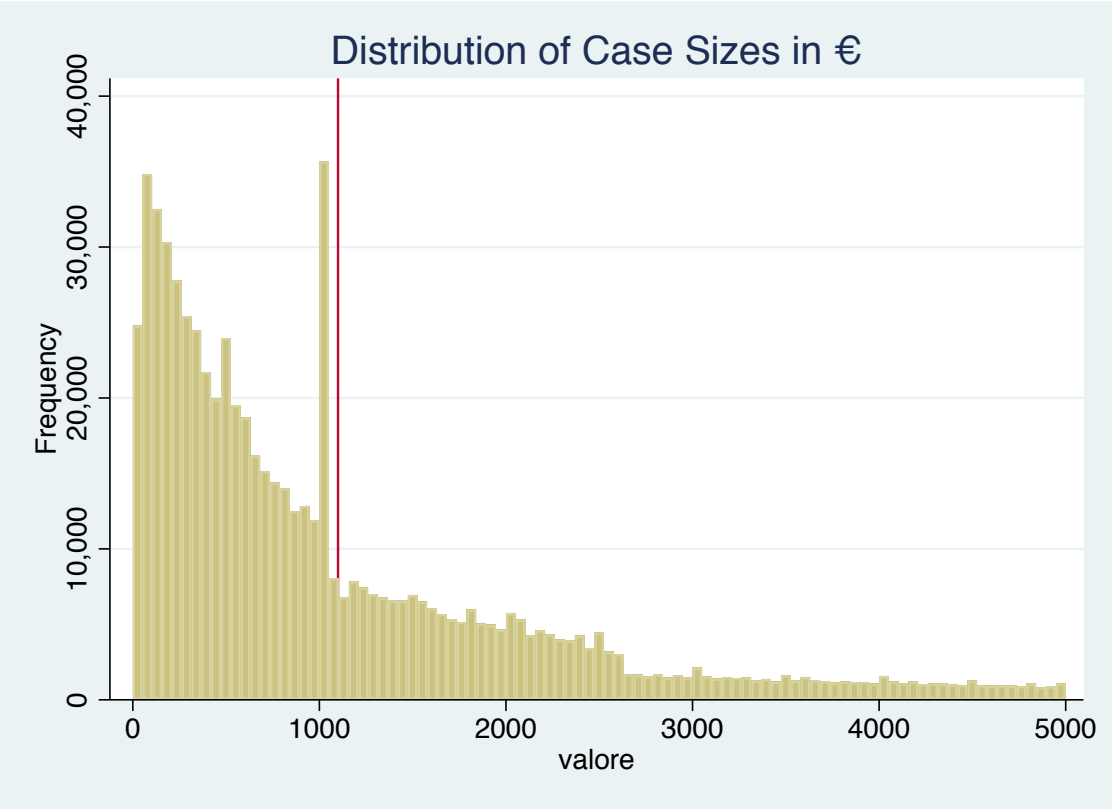


Figure 3: Cases' Value Distribution



95 bins of size 52.63

Figure 4: Ideal Distribution of Cases without Manipulation

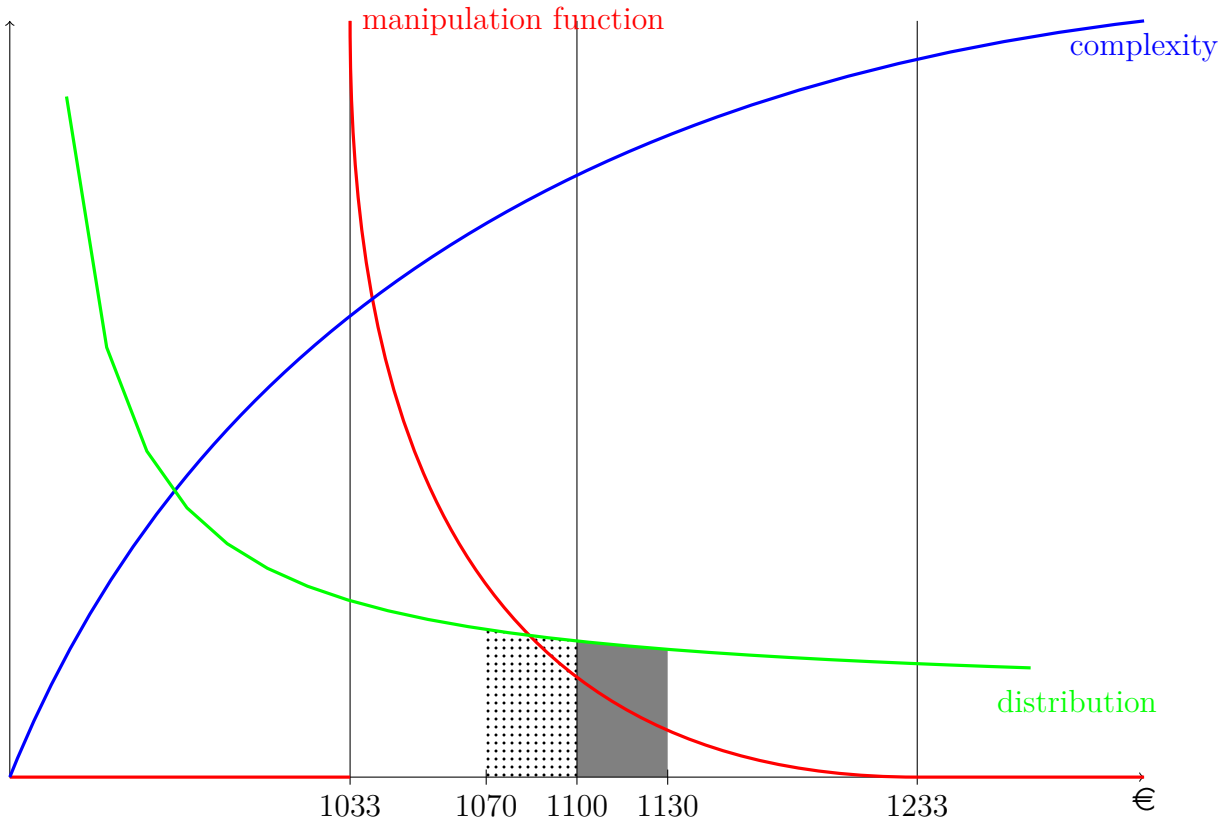


Figure 5: Actual Distribution of Cases with Manipulation

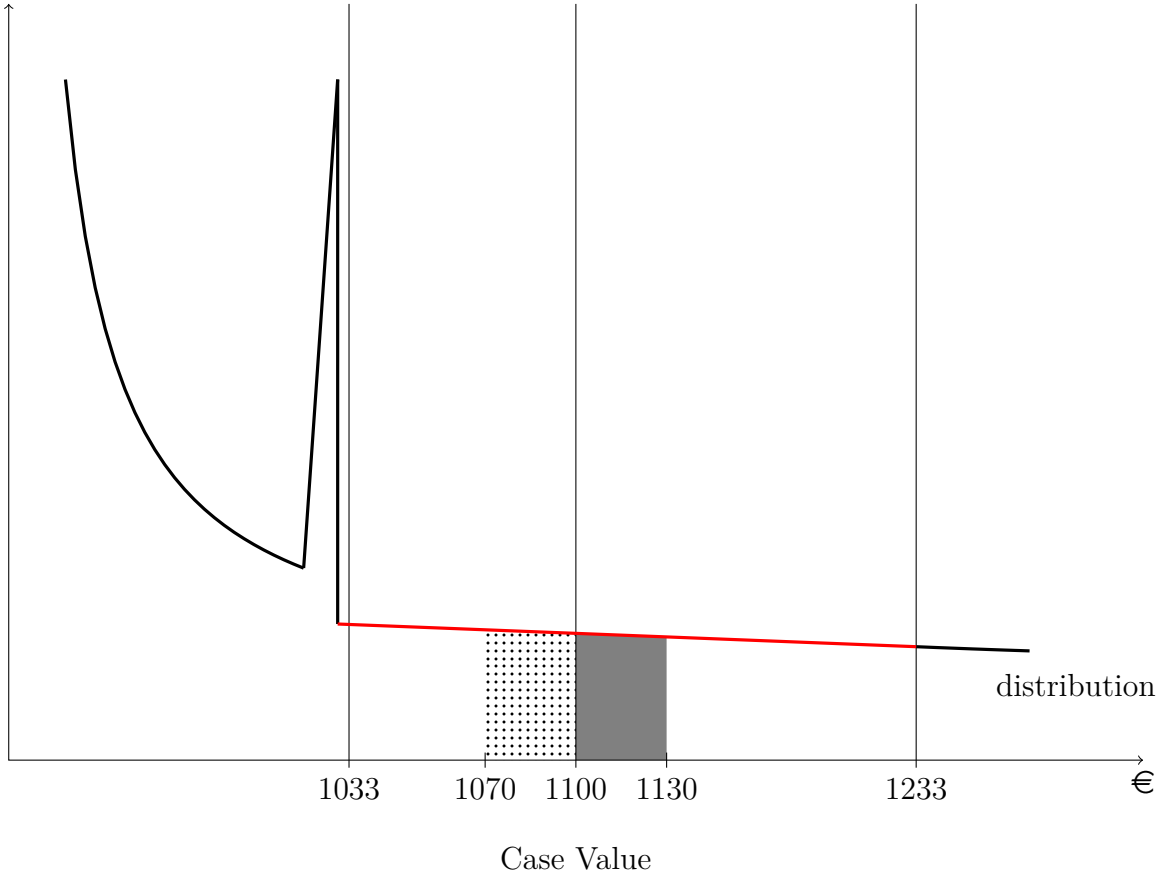


Figure 6: Cases' Value Distribution

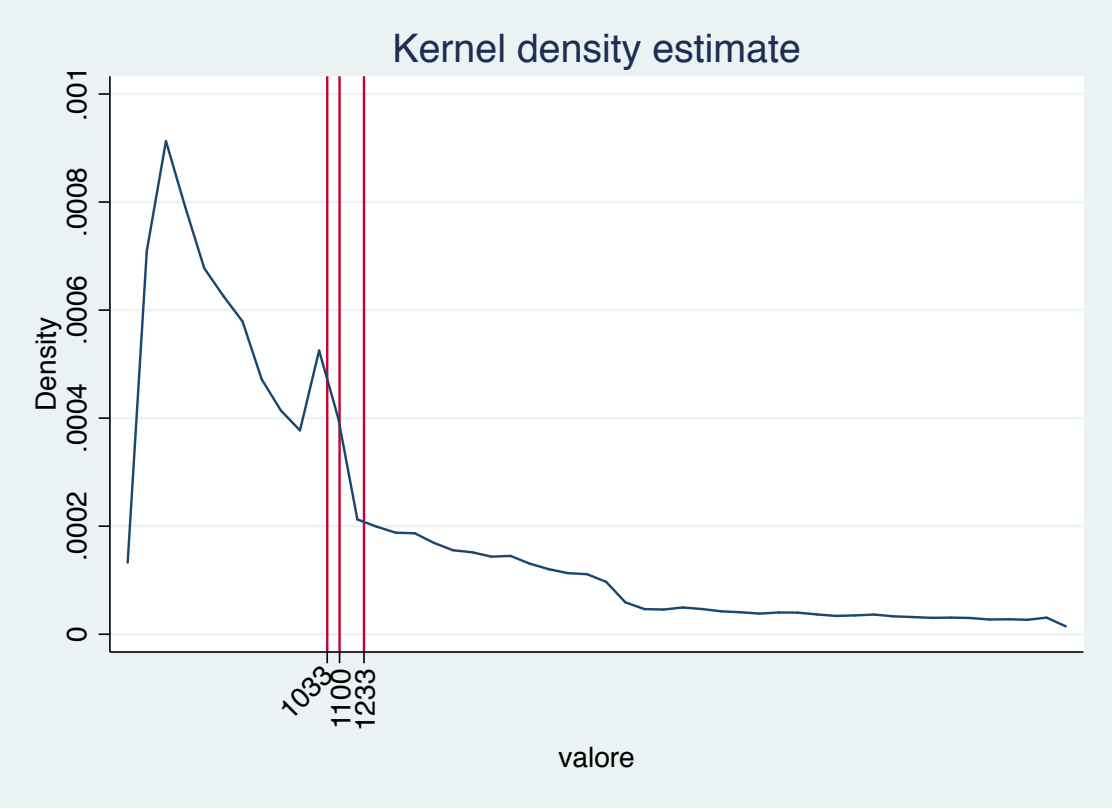


Table 1: Summary Statistics: Justice of Peace Data (Turin, Italy 1995-2017)

		Disposition Time		Case Value in €	
Case type	N	Mean	Median	Mean	Median
Debt recovery	533,031	162	43	988	655
Torts	41,576	302	161	2,052	1,673
Contracts	42,175	208	98	1,169	890
Other cases	35,392	181	110	833	416
All	652,174	175	85	1,059	703

Table 2: Summary Statistics Around the Threshold

	Full Sample		Optimal Bandwidth Estimators			
			cersum = 31.530		cerrd = 36.890	
	Below	Above	Below	Above	Below	Above
Disposition Time (in days)	103.01	326.51	279.50	288.42	281.87	284.71
Number of Attorneys	0.96	1.14	1.15	1.09	1.14	1.10
Number of Litigants	2.06	2.18	2.16	2.12	2.16	2.12
Day of the Week	3.12	3.12	3.10	3.16	3.11	3.17
Day of the Month	15.85	15.98	15.80	15.89	15.79	15.84
N	442,809	209,365	5,154	4,004	5,685	4,791

All figures represent the average value of the variable of interest in the considered sample above or below the threshold.

Table 3: Baseline Model

	Full sample		bandwidth=31.53		bandwidth=36.89	
	(1)	(2)	(3)	(4)	(5)	(6)
above_1100	147.0***	138.0***	32.93**	32.75**	35.73***	36.69***
	(1.258)	(1.243)	(13.13)	(13.08)	(12.27)	(12.22)
valore−1100	0.0553***	0.0522***	-7.283	91.29	-69.32	-1.917
	(0.00120)	(0.00119)	(98.68)	(99.02)	(65.91)	(66.18)
interaction	-0.00356	-0.00404*	-1.719	1.814	-2.960	-0.140
	(0.00242)	(0.00238)	(2.719)	(2.742)	(2.162)	(2.181)
valore^2	-2.56e-06***	-3.33e-06***	0.00341	-0.0422	0.0318	0.000556
	(4.63e-07)	(4.57e-07)	(0.0454)	(0.0456)	(0.0304)	(0.0305)
#attorneys		85.80***		69.43***		66.05***
		(0.670)		(8.673)		(8.224)
#parties		1.423*		-2.179		1.498
		(0.742)		(8.686)		(8.094)
Year FE	✓	✓	✓	✓	✓	✓
N	652,174	652,174	9,158	9,158	10,476	10,476

Each regression includes the following control variables: amount above €1,110 threshold, the interaction between this amount and above €1,100 dummy, a quadratic polynomial of the case value, the number of litigants in each case, the number of attorneys in each case and year-fixed effects. Columns (2) and (3) use coverage error-rate optimal bandwidths (Imbens and Kalyanaraman, 2012). Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Different Case Types Subsamples

	Full sample	bandwidth=31.53	bandwidth=36.89
	(1)	(2)	(3)
	Panel A - *Contracts*		
above_1100	189.35 ***	108.5***	110.32***
	(5.26)	(39.39)	(37.77)
Controls	✓	✓	✓
Year FE	✓	✓	✓
N	42,175	1,015	1,152
	Panel B - *Torts*		
above_1100	73.49***	57.66	57.64
	(6.778)	(37.79)	(36.19)
Controls	✓	✓	✓
Year FE	✓	✓	✓
N	41,576	892	954
	Panel C - *Debt Recovery*		
above_1100	140.21***	-7.177	1.353
	(1.309)	(16.63)	(15.39)
Controls	✓	✓	✓
Year FE	✓	✓	✓
N	533,031	6,634	7,703

Each regression includes the following control variables: amount above €1,110 threshold, the interaction between this amount and above €1,100 dummy, a quadratic polynomial of the case value, the number of litigants in each case, the number of attorneys in each case and year-fixed effects. Columns (3) and (4) use coverage error-rate optimal bandwidths (Imbens and Kalyanaraman, 2012). Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Donut RD - All case types

	full sample	bandwidth=109.724	bandwidth=128.207
	(1)	(2)	(3)
above_1100	161.87***	187.71***	137.14***
	(1.371)	(27.804)	(18.783)
Controls	✓	✓	✓
Year FE	✓	✓	✓
N	611,511	18,134	24,918

Each regression includes the following control variables: amount above €1,110 threshold, the interaction between this amount and above €1,100 dummy, a quadratic polynomial of the case value, the number of litigants in each case, the number of attorneys in each case and year-fixed effects. Columns (2) and (3) use coverage error-rate optimal bandwidths (Imbens and Kalyanaraman, 2012). Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Donut RD - Different case types subsamples

	full sample (1)	bandwidth=109.724 (2)	bandwidth=128.207 (3)
	Panel A - *Contracts*		
above_1100	228.16 ***	42.45	90.86
	(5.963)	(122.05)	(76.267)
Controls	✓	✓	✓
Year FE	✓	✓	✓
N	38,671	1,478	2,024
	Panel B - *Torts*		
above_1100	120.52***	188.63	129.15
	(8.217)	(134.239)	(92.629)
Controls	✓	✓	✓
Year FE	✓	✓	✓
N	37,198	1,231	1,587
	Panel C - *Debt Recovery*		
above_1100	159.08***	190.17***	135.66***
	(1.431)	(28.947)	(19.746)
Controls	✓	✓	✓
Year FE	✓	✓	✓
N	503,306	14,688	20,383

Each regression includes the following control variables: amount above €1,110 threshold, the interaction between this amount and above €1,100 dummy, a quadratic polynomial of the case value, the number of litigants in each case, the number of attorneys in each case and year-fixed effects. Columns (2) and (3) use coverage error-rate optimal bandwidths (Imbens and Kalyanaraman, 2012). Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.