A BICEPHALOUS MODEL OF PROCEDURAL JUSTICE AND WORKPLACE DISPUTE RESOLUTION

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ABSTRACT
Until now, empirical research has been unable to reliably identify the impact of organizational dispute resolution systems (DRSs) on the resident workforce, in part because of the dearth of data tracking employee perceptions pre- and post-implementation. This study begins to fill this major gap by exploiting survey data from a single, geographically-expansive, US firm with well over 100,000 employees in over a thousand locations. The research design allows us to examine employment relations and human resource measures, namely, perceptions of justice, organizational commitment, and perceived legal compliance, in the same locations before and after the implementation of a typical, multistep DRS that begins with informal reporting to local managers and culminates with mandatory arbitration. Holding all time-constant, location-level variables in place, we find that introduction of the DRS is associated with elevated perceptions of informal procedural justice and interactive justice, but diminished perceptions of formal procedural justice. We also find no discernible effect on organizational commitment, but a significant boost to perceived legal compliance by the company. The authors draw upon these findings to offer a revamped model for conceptualizing the relationship among perceptions of justice, perceived legal compliance, and formal versus informal aspects of dispute resolution mechanisms.

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Efforts to understand the effects of DRS implementation in workplaces over the past four decades have addressed the issue of whether DRSs serve legitimate purposes in a non-union setting, or if they are regarded as attempts by management to undermine rightful employee concerns. Put in other terms, a longstanding question posed by the literature is: does a non-union DRS enable legitimate employee expression of “voice,” which, in turn, increases employee loyalty to the company? Some have theorized that employers are motivated by reduced costs associated with defending employee lawsuits (Bingham 1997; Stone 1996). Some posit that DRSs serve as a tool for employers attempting to discourage or avoid unionization (Colvin 2003). If these motivations are transparent, or at least suspected, non-union DRSs are likely to decrease employees’ perceived procedural justice, reducing organizational commitment as well, according to the traditional model. Others laud more altruistic qualities of workplace DRSs, like improved opportunities for remedial voice (Lewin 1999; Sheppard, Lewicki and Minton 1992), reduced exit, and increased perceptions of procedural justice (Gordon and Fryxell 1993; Lind et al. 1993; Spencer 1986). This research suggests that employees can regard employer-implemented DRSs as legitimate even in the absence of collective representation.

In all cases, whether altruistic or self-interested, if employees regard a DRS as an unbiased procedure for resolving conflict, providing a fair measure of due process, and not just as a union-busting, or litigation-cost-reducing tool of the employer, employees should be less likely to sue or unionize. Increased legitimate opportunities for voice and greater perceived procedural justice should increase employees’ organizational commitment and lower turnover, consistent with the “exit-voice-loyalty” trade-off (Folger 1977; Hirschman 1970). This is visually represented in Figure 1 below, which depicts the standard model connecting DRS implementation, procedural justice, and organizational commitment.

Lower turnover and improved procedural justice lead to a more stable and productive workforce in which employees are less likely to shirk and more likely to exhibit so-called organizational citizenship behavior (Cohen-Charash and Spector 2001; Moorman 1991a; Moorman 1991b; Moorman, Niehoff and Organ 1993; Niehoff and Moorman 1993). Resolving conflict internally also reduces costs because litigating disputes is often more expensive than
addressing them internally. Along the same lines, internal DRSs reduce the likelihood that what might otherwise be a lower-cost non-legal issue (e.g., an employee complaint of generic unfair treatment) is transmogrified by a plaintiff lawyer into a higher-cost legal issue (e.g., the unfair treatment was a manifestation of employment discrimination). The standard model predicts that if employees regard the DRS as procedurally just, they will use it to resolve both legal and non-legal issues, be happier, less likely to exit the firm, and be more committed to the organization.

Traditionally, improved procedural justice perceptions associated with opportunities for employee voice in the form of dispute resolution systems correspond with greater employee retention and organizational commitment. This paper suggests a reassessment of the relationship among these important constructs. We posit that formal aspects of a typical DRS correspond with reduced procedural justice and have no effect (or a net negative effect) on organizational commitment, but do correspond with a significant increase in the degree to which employees perceive that the company is complying with the law. The informal components of the very same DRS correspond with increased procedural and interactive justice. The non-universality of these effects seems counter-intuitive under the standard model in which procedural justice is positively associated with perceived legal compliance and organizational commitment. In our model, we suggest that these effects might actually be related such that implementing a typical DRS with formal, legalistic components and informal, interpersonally-based components may increase employees’ trust and loyalty towards local managers who implement the DRS informally, while (1) decreasing employees’ trust and loyalty towards the firm as a whole, and, (2) increasing the degree to which employees perceive the company as complying with the law. We also focus on an often overlooked but critical component of procedural justice: the extent to which employees view the implementation of a workplace DRS as a either (a) a signal that the employer is more likely in compliance with state and federal laws governing the workplace because the employer cares about legal issues and wants to address them, or (b) a signal that the employer is less likely in compliance with such laws because the employer is using the DRS to attempt to circumvent its obligations under the law in an informal, more private, cheaper setting.

The authors believe that this is the first paper to offer an empirical assessment of the impact of implementing a workplace dispute resolution system (DRS) in a non-union setting on currently employed workers, not just on employees who have gone through the DRS as
claimants or participants. In spite of repeated calls for empirical research in this area, as other researchers have observed, very little is known about how implementation of DRSs affect employees’ perceptions of procedural justice and critical related metrics (Budd and Colvin 2008; Mahony and Klaas 2008). We analyze eight years’ worth of data spanning more than 100,000 workers and more than 1,000 locations across the United States provided by a large company, “Gilda’s, Inc.,”1 to be described below. Gilda’s implemented an internal DRS in the middle of the eight years of data. Like most workplace DRSs, the program at Gilda’s starts informally with claim initiation with local supervisors, escalates to formal review by the corporate Human Resources Department, and culminates with final, binding, mandatory arbitration.

These unique data allow the authors to evaluate employee perceptions of justice and beliefs about whether and to what extent the firm implementing the DRS is complying with the law, both before and after its implementation. Findings suggest that implementing a DRS with both formal and informal components creates a “bicephalous” or two-headed effect on the workforce. We bifurcate the effects into “localized” effects—ones that are most salient to the relationship between employees and their local store managers, and “centralized” effects—ones that are most salient to the relationship between employees and the central management of the firm. In the data analyzed, the localized effects of DRS implementation are increased perceived informal procedural justice (focused on local management’s ability and likelihood to successfully resolve employees’ problems) and interactive justice.2 The centralized effects are decreased perceived formal procedural justice (focused on the formal aspects of the organization and the HR Department), and increased perceived legal compliance. Implementation of the DRS has no statistically significant effect on organizational commitment, but it has a strong, positive effect on the degree to which employees perceive that the company studied is complying with laws regulating the workplace (i.e., preventing harassment and discrimination based on protected categories like race, age, and gender). Grounded by the analysis of this novel dataset, the authors develop and partially test a model of the relationship among procedural justice—bifurcated between formal and informal procedural justice—and organizational commitment, interpersonal commitment, and perceived legal compliance. Our

1 “Gilda’s” is a pseudonym used to protect the company’s anonymity.
2 Discussed more below, interactive justice is distinct from procedural justice and measures the extent to which employees believe that their needs are taken into account in making decisions and that employees are provided with adequate explanations when management decisions are made.
findings raise important questions about the degree to which commitment to remain employed at a firm is associated with unilaterally imposed DRSs in a nonunion environment, implying that perhaps part of the fundamental trade-off of formal voice procedures like mandatory arbitration is increased perceived legal compliance for reduced formal perceived procedural justice, with no net effect on organizational commitment. Perhaps the corresponding increase in perceived informal procedural and interactive justice associated with the informal aspect of the DRS are associated with employee commitment, but to local supervisors—what could be described as “interpersonal commitment”—instead of to the organization.

**Theory**

There is a stark contrast in conceptualization of the relationship between employment DRSs and perceived procedural justice in unionized and nonunionized settings. In a unionized setting, there is significant evidence to support the nexus between augmented procedural justice derived from grievance and arbitration procedures and increased commitment to remain at the firm. Theorists disagree on whether improved perceived procedural justice is even attainable in a nonunionized setting where a DRS is unilaterally imposed by an employer as opposed to being the product of collective bargaining, i.e., where employee participation is necessarily less collective and more individualized (Budd 2004; Clegg 1975; Sheppard, Lewicki and Minton 1992). There has been vastly more speculation and hypothesizing than empirical study of the relationship between procedural justice and DRS implementation in the nonunion setting. This may be due in part to the private nature of employment dispute resolution and the difficulty gaining access to data. To the extent that information is reported through clearinghouses like the American Arbitration Association (AAA), it may be reasonably assumed that AAA data capture only the tip of the iceberg, may not be representative of the non-available data, and most importantly for this study, never include any information about how employees perceived workplace metrics related to procedural justice before implementation of a workplace DRS. Because of the general dearth of before and after information on employees’ perceptions of procedural justice, research has focused instead on discerning the characteristics of competing dispute resolution models (Ewing 1989), the causes for grievance initiation (Boroff and Lewin 1997), the effects of grievance activity (Ichniowski 1986), claimant win rates (Colvin 2010; Sherwyn, Estreicher and Heise 2005) and disputants’ perceptions of procedural justice (Aram and Salipante 1981; Fryxell and Gordon 1989), among other important areas.
Some have suggested that implementing a DRS signals a constructive and proactive approach to dispute resolution on the part of employers, generating significant effects on non-disputant employees—the workforce at large (Lewin 1999). However, empirical study of such effects is hampered by the lack of evidence of pre- and post- implementation of the DRS. This is exactly the research gap that this paper aims to fill. Before describing the employer’s DRS and these data, we first outline concepts of justice as related to formal and informal means of employment dispute resolution.

**Justice**

Researchers have sub-classified fairness perceptions into distributive, procedural, and interactive justice, which have been shown to measure distinct constructs (Cohen-Charash and Spector 2001). Distributive justice focuses on the perceived equity in distribution of outcomes or rewards, whereas procedural justice is the perceived fairness of the decision-making processes as opposed to outcomes (Folger and Greenberg 1985; Lind and Tyler 1988). In the workplace, this may be measured as the perceived effectiveness of appealing adverse employment actions such as discipline and discharge, the degree to which management is perceived as accessible and proficient at resolving problems, and offering guidance for dispute resolution, and generally, the extent to which employees believe that the organization is committed to resolving employee disputes quickly and fairly (Cohen-Charash and Spector 2001; Sheppard, Lewicki and Minton 1992).

Procedural justice with respect to workplace DRSs is intrinsically linked to employee “voice” behaviors, attempts to change rather than escape a perceived deterioration of a condition experienced in an organization (Folger 1977; Hirschman 1970). Sheppard, Lewicki and Minton (1992: 139–40) suggest that voice can be “preventative” or “remedial,” the former referring to the solicitation of opinions and suggestions about organizational practices and procedures, and the latter taking the form of appeals to organizational policies and practices after injustice has occurred. Others have distinguished voice along similar lines—as being interest-based versus rights-based (Estreicher and Eigen 2010). Most nonunion DRSs are almost exclusively designed to encourage remedial, interest-based voice without much attention paid to preventative, rights-based voice (Lipsky, Seeber and Fincher 2003). Permitting remedial voice entails ensuring sufficient perceived availability and access. Like procedural justice, it is measured as the degree to which employees feel comfortable reporting
problems to management and their actual likelihood of reporting these problems (Spencer 1986). For the purposes of evaluating DRSs, it seems that comfort initiating claims and the perception that the DRS will address the claim in a neutral, effective and fair way are the two most critical elements by which to gauge the success or failure of a DRS (Folger and Bies 1989; Sheppard, Lewicki and Minton 1992; Tyler and Bies 1990).

Interactive justice is distinct from procedural justice. It measures the extent to which employees believe that their needs are taken into account in making decisions and that employees are provided with adequate explanations when decisions are finalized (Aquino, Lewis and Bradfield 1999; Bies 2001). The quality of the interpersonal treatment that an aggrieved employee receives from the decision maker and the way that the decision maker enacts the formal procedure will heavily influence the aggrieved party's perceptions of whether the procedures are fair (Tyler and Bies 1988). Honesty, courtesy, respectfulness, and appropriate professional decorum affect the way employees regard the fairness of procedures implemented (Sheppard, Lewicki and Minton 1992). Niehoff and Moorman (1993) developed a nine-item, validated measure of interactive justice that gauges employees' beliefs about their managers' interest in their opinions in work-related issues, the degree to which managers listen to their concerns, issues and suggestions, and how much their managers treat them with courtesy and respect among other related measures.

Effective workplace DRSs accord employees adequate opportunities for remedial voice and are regarded as distributively, procedurally, and interactionally just. Because the data available offer evidence of employees' views on how the organization addressed workplace disputes both before and after implementing a formal DRS, this study uniquely identifies the effect of implementation of a DRS on the entire employee population, the vast majority of which are non-disputants that are effectively excised from most existing analyses. It is less informative to compare distributive justice perceptions before and after DRS implementation as compared to procedural and interactional justice perceptions, because distribution of results is partially endogenous to the way in which disputes are addressed. Interactional effects will likely only be applicable in assessing components of the DRS that directly involve immediate supervisors or management, as their interaction reflects interpersonal elements of conflict resolution independent of trust or reliance in the formal mechanisms and non-local decision makers (i.e., "Human Resources," "the company" in the abstract, or formal appeal mechanisms). Procedural justice may be subdivided along similar lines—distinguishing between the extent to
which employees trust the organization (HR, the Company, and formal appellate processes) to formally resolve disputes when they are escalated above local management (formal procedural justice) from the extent to which employees trust and feel comfortable in initiating and addressing disputes with local management (informal, localized procedural justice). The informal component of procedural justice is probably more closely related to interactive justice perceptions. To the extent that DRSs mirror this distinction between formal and informal processes, it makes sense to measure the two separately in order to fully understand the effects of system implementation.

**Formal and Informal DRS Mechanisms**

The DRS at issue in this study takes a very common form. It begins by directing employees to initiate claims with their local managers informally and culminates in formal, final, and binding mandatory arbitration. To the extent to which informal mechanisms permit greater participation, they should result in greater perceived procedural and interactive justice (Colvin 2003; Thibaut and Walker 1975). Formal mechanisms like private mediation and arbitration have been shown to result in high levels of participant satisfaction (Lipsky and Seeber 2006; Lipsky, Seeber and Fincher 2003). However, the surveys of the outcomes are often compared with measures of participant satisfaction with outcomes in a judicial forum. This is less useful for managers, who want to know the extent to which a DRS can deliver what used to be thought of as “labor peace.”

Mandatory arbitration, whereby employees waive their right to bring lawsuits even for things like alleged violations of federal and state law, as a condition of employment, has been at the center of the debate on whether nonunion workplace dispute resolution is fair to affected employees (Bingham 1997; Estreicher and Eigen 2010; Sherwyn, Tracy and Eigen 1999; Stone 1996). Whether mandatory arbitration enhances or suppresses procedural justice and employee voice is an important but under-analyzed question (Batt, Colvin and Keefe 2002). Some suggest it creates opportunities for due process (Zack 1999), but others are more skeptical of such claims (Schwartz 2009; Stone 1996).

Recent research on employees’ interpretations of mandatory arbitration agreements they signed as a condition of their employment reveals that employees may regard these agreements as attempts by the company to insulate itself from legal liability (Eigen 2008). This may be due in part to the formal, legalistic, and contractual way in which such agreements
are typically presented. Employees might assume that if corporate headquarters insists that they sign something that looks like a legal document, that the company is doing so to protect its legal interests in a way that impinges on employee rights (Eigen 2008). It is therefore possible that employees could interpret a mandatory arbitration component of an employment DRS as a legal means of insulating corporate rights in a way that fosters overall distrust in the corporate entity because it appears to take away important employee legal rights such as the right to a jury trial.

The Employer’s DRS

“Gilda’s, Inc.” is a large company operating in the US. Gilda’s implemented a four-step DRS for all of its employees in all of its locations that went into effect on January 1, 2004. It is our understanding from reviewing records and talking with individuals in Gilda’s corporate HR department that the roll-out of this DRS did not occur at the same time as anything else that might be expected to shock employees’ perceptions of their managers or the company, nor was the introduction of the DRS part of a larger, company-wide initiative to revamp other aspects of work. Prior to the introduction of the DRS, employees were encouraged to report claims of illegal treatment in violation of federal and state law to their managers or to Gilda’s corporate Human Resources Department, which maintained a toll-free, call-in number for employees who wished to make claims anonymously.

The four steps of Gilda’s DRS are as follows: (1.) An employee may initiate a claim and attempt to resolve it with his local manager, (2.) If unresolved to his satisfaction at the local level, an employee may then report his claim to the HR Department, and someone will attempt to resolve the claim, (3.) If unresolved to his satisfaction by the HR Department, a claimant may then escalate his claim and choose between a determination made by a tripartite peer-review panel, or, by a unilateral determination made by another HR Department member, and (4.) If still unresolved to the employee’s satisfaction, he or she may initiate a claim to be brought before final, binding arbitration.

All newly-hired employees complete typical employment forms and sign a contract binding them to resolve any and all legal claims in arbitration. The orientation process is run by local managers based on talking points provided to them by Gilda’s corporate HR Department. New employees also watch a short video that plainly describes the four-step DRS.
Gilda’s four-step DRS is typical of many employment DRSs in two, key ways. First, it covers employee claims ranging from generic unfair treatment or complaints about random non-legal matters to highly legalized claims such as discrimination or harassment under Title VII of the Civil Rights Act of 1964. Second, Gilda’s DRS contains two distinct components: an informal, localized piece (step 1), followed by a formal, adjudicatory one (steps 2–4).

**Data and Methods**

**Survey Instrument and Variable Construction**

At its peak over the 2000 – 2007 sample period, Gilda’s employed over 100,000 workers in over 1,000 store locations. Each April, about one third of these locations were chosen at random to participate in an annual, paper-and-pencil survey on employment relations and HR matters. Occasionally, a few stores might be intentionally added to the annual sample if senior management had a particular interest in a given store. The most likely reason this would occur is because the otherwise random selection of stores had perennially omitted a particular store. Since the survey was administered during regular working hours, it achieved an impressive 80% response rate even in the absence of any financial incentive for completion other than paid time away from one’s usual work-related responsibilities.

While the sheer quantity of data shared by Gilda’s is massive—over 300,000 worker survey responses spread across eight survey years, the data were not originally collected for research purposes. This engenders two types of challenges—creating a cleanly and confidently linked panel dataset of store-years and constructing appropriate employment relations and HR indices. First, it was not always perfectly obvious when observations from a given store could be reliably linked to that store’s observations in other years. Though Gilda’s uses a numbering system for its stores, there were times when identical store numbers were associated with slightly different store descriptions or when identical store descriptions appeared to be associated with multiple store numbers. We resolved as many of these anomalies as possible with help from Gilda’s management and from reliable external sources. Where we were not able to confidently reconcile survey responses, we dropped them from the analysis. Furthermore, while the original sampling frame included mid-level managers, part-time employees, and workers in supporting roles, we excluded them to ensure that all workers
surveyed were frontline workers, doing essentially the same work. In the net, this yielded 215,140 frontline worker responses from 625 different stores over the eight-year period.

The second challenge this company-collected data makes for research is the make-up of the actual survey items. The items themselves were neither developed from pre-validated scales nor kept completely identical across survey instruments. While it is safe to assume that all employees surveyed in a given year in a given store responded to the same instrument, the survey instruments did vary slightly from year to year and even between regions in a given year. In general, each instrument included between 20 and 40 items, all of which were answered on a five-point Likert-type scale in which 1 = “strongly disagree” and 5 = “strongly agree.”

We addressed this inconsistency issue first by identifying each of the distinct survey questions asked over all of the survey instruments, allowing even small changes in wording to constitute a distinction. This exercise yielded about 90 “unique” survey items. The researchers then worked independently to group the items by “theme”, e.g., questions regarding perceptions of procedural justice, questions regarding the quality and frequency of managerial communication. This yielded surprisingly similar and thus, (inter-rater) reliable groupings (Schwab 2005).

Ideally, we could then verify high inter-item reliability by computing something along the lines of a Cronbach’s $\alpha$, where a large $\alpha$ could justify our coalescing the items into a named index. However, none of the groupings yielded any observations that contained values for every single one of the constituent items, thereby preventing our use of the standard estimate of reliability. Consequently, we created scales by taking “row means” consisting of average scores across the items comprising a given index or scale, for however many non-missing items are available, for a given observation. The end result is a set of five employment relations measures that emerge directly from the data, the very same data that the company itself relied on to make and assess its HR strategy. Unfortunately, as will become clear from the descriptive statistics in the next section, the constructs themselves are somewhat correlated with one another, implying low levels of discriminant validity between them (Schwab 2005). However, the five emergent variables span a range of measures that combine to paint a rich picture of the broad employment relations and HR climate pre- and post-DRS.

The definitions of these variables as well as the construction of each index are detailed in Table 1. Three justice measures emerged. The first two are formal and informal procedural

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3 The surveys also posed two or three open-ended text questions, but we were not provided the employees’ responses to these questions.
justice, respectively. The former, formal procedural justice, measures the degree to which employees perceive the company itself, the company’s corporate HR Department, and formal mechanisms for appealing adverse employment actions as accessible and viable options for redressing disputes. The latter captures the degree to which employees perceive local store managers and supervisors as accessible and viable options for dispute resolution. They both measure employee comfort initiating claims and the perception that the entity, person (Gilda’s as a whole, the HR Department, or local supervisor or manager) or process addressing the claim (formal or informal) will address the claim in a neutral, effective, and fair manner.

The survey questions making up these two constructs align with descriptions of access and problem solving viability described in several studies outlining critical elements by which to gauge the effectiveness of a workplace voice system with or without a DRS in place (Folger and Bies 1989; Sheppard, Lewicki and Minton 1992; Tyler and Bies 1990). The components being judged in both the formal and informal procedural justice constructs exist before and after implementation of the DRS. Once the DRS is implemented, the formal procedural justice components align with steps 2-4 while the informal justice components align with step 1.

Interactive justice reflects the degree to which workers feel well-treated (given courtesy and respect) and listened to, not by “the company” per se, but by managers and direct supervisors with whom they deal on a personal level. The survey questions making up this construct align closely with validated scales used by Moorman (1991b) and Niehoff and Moorman (1993).

Legal compliance tracks employees’ perceptions not of how they are treated, but rather the degree to which the firm is following the law, particularly with respect to laws prohibiting discrimination and harassment. This allows for a nuanced examination of DRS effectiveness, whereby workers may believe the law is being better followed post-DRS, whether their personal treatment has improved or not.

Finally, in order to vet the traditional model linking DRS implementation with loyalty to the firm, we measure organizational commitment as well. The survey questions making up this construct align well with commonly used validated scales (Cook and Wall 1980; Mowday, Steers and Porter 1979). In the aggregate, we believe that the judgments made in assembling the panel as well those made in constructing the employment relations and HR measures work to attenuate rather than to bias our estimates, engendering a collectively conservative assessment of the impact of Gilda’s DRS.
While the indices were constructed using worker-level data, the multivariate analysis relies on a store-level dataset, created by taking means of each of the resulting indices by store-year. Since stores can be linked from year-to-year but individuals cannot be, the decision to use store-years as the unit-of-analysis allows us to take advantage of powerful techniques for analyzing cross-sectional times series (panel) data. Given the conservative pruning of the data described above, the resulting dataset includes 1,171 store-years representing 635 distinct stores. Table 2 shows the number of stores contributing observations for each of the dependent variables, by year. The employment relations and HR measures serve as dependent variables in the sense that their movement over time should reflect the impact of Gilda’s DRS. The latter is the focal independent variable and is operationalized as a binary variable equal to one for years 2004 onward.

Empirical Strategy

As noted above, it was right in the middle of the observation period—January 1, 2004—that Gilda’s DRS went into effect for all employees at all locations. Therefore, the data allow us to track movements in employment relations variables over time, including the impact of a policy-induced discontinuity beginning with the 2004 observations. While we cannot rule out alternative explanations of the effects of implementing the DRS caused by contemporaneous exogenous shocks, we have no reason to suspect that this is occurring. We use store-level data to analyze the effects of implementing the DRS for reasons described above, but before delving into that analysis, we offer Figure 2 as a simplified visual depiction of the five dependent variables described above—formal procedural justice, informal procedural justice, interactive justice, organizational commitment, and legal compliance—measured as individual level means (not aggregating up to the store level) over the whole time period in the data. Even with this figure, it appears that most of the measurements look different before implementation of the DRS as compared to after. Caution should be taken not to read too much into this graph. Below, we describe methods that account for store-level variation and time effects.
As a first pass, we will simply compare the mean values for our five dependent variables before and after the implementation of the DRS. However, in order to claim that any discontinuity is associated with the implementation of Gilda’s DRS, we would need to control for store-level variables that could explain the otherwise relatively “smooth” variation over time in employment relations outcomes. While we have little information on each store other than its yearly “bag” of survey responses, we can generally match each store’s responses in a given year to its responses in all other years. Consequently, we can control for all store-level, time-constant, unobservable sources of variation in employment relations measures. We can do this first by simply augmenting the differences-in-means tests to account for store-level fixed effects. These tests, while easy to interpret, still allow for the possibility that time-varying unobservable variables are biasing the estimated impact of the DRS on the dependent variables. Therefore, we will also offer a more sophisticated model that at least partially accounts for these time-varying unobservable drivers of employment relations outcomes.

The most transparent and conservative way to do this is to consider same-store values of each dependent variable pre- and post-DRS, net of as many confounding effects as possible. The longitudinal nature of the data facilitates this effort first by allowing us to estimate fixed-effects models to account for time-constant unobservable drivers of employment relations at the store-level (Baltagi 2005). From a practical perspective, this requires a model that includes a vector of 635 store-specific dummies on the right-hand side. Second, reliance on panel data also facilitates a way of dealing with time-varying unobserved variables that could be positively correlated with both the implementation of the DRS and of the dependent variables, factors that could bias the critical estimates of the slope coefficient associated with the DRS. It is important to note that since the DRS was instituted centrally and simultaneously across all stores and by Gilda’s senior management at corporate headquarters, it is likely that many of these unobserved variables would be company- or even economy-wide, not store-specific. Therefore, we can strip the estimates of some of these time-varying unobservables by including an additional vector of binary variables—one for each year—on the right-hand side of the equation. Formally, we model employment relations and HR variables using a multilevel model,
where \( y \) represents any one of the five employment relations variables for store \( i \) in year \( t \) and \( \varepsilon_{it} \) is a zero-expectation error term. The first term on the right-hand side represents a dummy variable for each store, \( d_s \), multiplied by a store-specific intercept, \( \alpha_i \). Since there is no shared intercept term (akin to \( \alpha_o \)), this entire first term can be simplified to \( \alpha_i \), a single intercept term for store \( i \). Similarly, the second term adds a dummy, \( d_T \), for each year other than the first year, 2000, multiplied by a year-specific intercept, \( \alpha_T \). This captures the year-specific mean of the dependent variable, and can thus be simplified to \( \alpha_T \). Consequently, Equation (1) can instead be written as

\[
y_{it} = \alpha_i + \alpha_T + \beta_{it} x_{it} + \varepsilon_{it},
\]

where the key explanatory variable—the binary representing the implementation and ongoing use of the DRS—is represented by \( x_{it} \). Therefore, the focal coefficient estimate is \( \hat{\beta}_{it} \)—the impact of the DRS on the dependent variable, net of unobserved sources of store-specific and year-specific variation. To further error in the conservative direction, we compute standard errors using the generalized Huber-White formula clustered by store, allowing for arbitrary correlations of residuals among store-year observations (Bertrand, Duflo and Mullainathan 2004).\(^4\)

**Results**

Table 3 shows descriptive statistics and correlations for the focal independent variable as well as for the five employment relations measures. Note that about 56% of the observations are post-DRS and that none of the employment relations variables is strongly correlated with the use of the DRS. Interestingly, both formal procedural justice and organizational

\(^4\) In almost all cases and as expected, the standard errors calculated by the Huber-White formula are only minutely greater than the standard errors that otherwise emerge from this model, since the model already accounts for the dependence structure of the data.
commitment reveal a negative, zero-order association with the DRS, while the other three variables are positively, pair-wise correlated with the DRS. Delving more deeply into the employment relations variables, recall that each is measured on a five-point, Likert-type scale in which 1 = “strongly disagree” and 5 = “strongly agree.” Therefore, since the means for these variables are all in the range from 3.24 to 3.88, all are somewhere between the neutral response (“neither agree nor disagree”) and agreement. This stems in part from the positive correlations between some of these variables. For example, informal procedural justice is somewhat strongly correlated with both interactive justice and legal compliance.

As noted above, the most straightforward demonstration of a difference in the values of the dependent variables between the pre-DRS and post-DRS periods would be with a simple difference-in-means accompanied by a $t$-test of that difference. This is precluded by the fact that the same stores appear repeatedly, from one to six times over the 2000-2007 observation period. We therefore calculate the pre- and post-DRS means and the differences between them by backing them out from a random effects regression model that accounts for the dependence between stores. The results can be interpreted as one would normally interpret a difference-in-means. However, the $t$-statistic is replaced by a $z$-statistic owing to the fact that the null hypothesis for a variance components model assumes a normal sampling distribution rather than a $t$-distribution.\footnote{Since the finite sample distribution for a variance components model does not have a simple form, most researchers and most statistics packages rely on the asymptotic (i.e., large-sample) sampling distribution (Skrondal and Rabe-Hesketh 2004).}

Table 4 presents these simple differences-in-differences for each of the dependent variables. Focusing on formal procedural justice, note that its mean value across all stores in the pre-DRS period was 3.30 on a scale ranging from one to five, where one is low and five is high. In the post-DRS period, the mean score for formal procedural justice is 3.21. While this represents a .09 decrease in this dependent variable, recall that this is a simple difference-in-means. It accounts for the non-independence of observations by stores, making the test-of-
significance more conservative. Even so, the .09 reduction in formal procedural justice is highly statistically significant ($p < .001$). The results for the other four dependent variables can be interpreted similarly. The post-DRS observations reveal measurably greater values than their pre-DRS counterparts, all at very high levels of statistical significance, for informal procedural justice, interactive justice, and legislative compliance. On the other hand, mean scores for organizational commitment fall very slightly, by .04. All of these differences-in-means are statistically significant at the $p < .001$ level.

The results from Table 4 can be made slightly more robust by treating the separate stores in the data not as a “nuisance” factor to be accounted for, but as an explicit source of variation in the dependent variables. Along these lines, Table 5 presents differences-in-means that parallel the results in Table 4. However, in this case, the means are backed out of a fixed-effect estimate that includes a vector of binary variables, one for each of the separate stores in the data. These dummy variables “soak up” any part of the variation in the dependent variable that can be accounted for by time-constant, unobserved attributes of each individual store.

The results in Table 5 can be interpreted almost identically to those in Table 4, the only difference being that these results can be thought of as “within-store” effects. Looking at formal procedural justice once again as an illustration, on average, within a given store, perceptions of formal procedural justice fell by .11 between the pre-DRS and post-DRS periods. This difference is slightly more than the difference-in-means when unobserved differences between stores are not accounted for (.09). In this case, the differences are tested with the usual $t$-statistic, since the fixed-effects model yields a finite sample distribution with a shape than can be easily determined based on the degrees of freedom. Overall, the results are qualitatively identical to those in Table 4, implying that pre-post differences in the mean values of the dependent variables cannot be attributed to unobserved factors at the store level. Relative to the pre-DRS period, within the same stores, perceptions of formal procedural justice and measures of organizational commitment decreased while perceptions of informal procedural justice, interactive justice, and legislative compliance all increased.

What the results in Tables 4 and 5 do not account for are time-varying firm- and economy-wide unobservables that could also be correlated with both the DRS dummy and the
dependent variables. The omission of these variables could allow for effects to be wrongly attributed to the DRS. Therefore, the regression estimates in Table 6 allow for a more-nuanced analysis of the employment relations variables, one that teases out the impact of the DRS not only from the other store-level drivers of the dependent variables, as in Table 5, but also from year-specific forces that impinge on the dependent variables. Each model estimates Equation (2) independently for each of the enumerated employment relations variables. Stepping through Model 1 to illustrate, notice that even after controlling for store-fixed and year-fixed effects, the introduction of the DRS is associated with .13 point decrease in the five-point measure of formal procedural justice. While slightly larger than the effects estimated in the absence of year dummies, the estimate does not achieve quite the same level of statistical significance (\( p < .05 \)). The model explains just nine percent of the within-store variance in formal procedural justice across the 444 stores that were able to provide data for this index, and thus, that were included in the estimate. Each store lent, on average, 1.6 observations (\( 725 \div 444 \)) to the estimate of Model 1. Finally, \( \rho \) is the share of the estimated variance of the overall error, \( \epsilon_a \), accounted for by effects at the store level, a measure of intraclass correlation. In this case, 59 percent of the variability in formal procedural justice can be attributed to differences between (versus within) stores. This value for \( \rho \) bolsters our analysis in two ways. First, it substantiates our decision not to estimate conventional OLS models, since OLS requires that \( \rho = 0 \) (Singer and Willett 2003). Second, it justifies our decision to model the dependent variable at the store-level as opposed to the person-level as individual survey responses in a given year do, indeed, cluster within a given store. The remaining four models can be interpreted similarly. Models 2–4 are particularly important for the analysis. Note that in all three cases, relative to Model 1, they explain a measurably larger share of observed variance. Furthermore, in all three cases, the impact of the DRS is positive and highly statistically significant. Interestingly, for informal procedural justice and legislative compliance, the estimated effect of the DRS in these estimates accounting for firm- and economy-wide unobservables by year are actually larger than they were for estimates that do not include year dummies (shown in Table 5). Finally, Model 5 attempts to predict organizational commitment using the exact same model and right-hand side variables as the other four models. It is interesting to note that the same variables that shed light on justice and legal compliance do very little to illuminate the drivers of organizational commitment. Moreover, by comparing the Model 5 estimates to the differences-in-means in Tables 4 and 5, we can see that there is no simple relationship between the DRS
and organizational commitment as there is between the DRS and the other dependent variables.

Nonetheless, in the net, the regression results support the theory that the DRS affected the workforce in a bifurcated manner, decaying at perceptions of formal procedural justice while boosting perceived levels of informal and interactive justice. The DRS, on average, was also associated with increased perceptions on the part of frontline employees that Gilda’s was, indeed, complying with the law. However, once store-level and year-level effects are controlled for, there is no evidence of a simple, DRS-induced change in organizational commitment.

[—INSERT TABLE 6 ABOUT HERE.—]

Discussion

This paper presents evidence of the effects of implementing a workplace DRS with bifurcated formal and informal components. For the most part, these effects are positive as one might anticipate and predict based on the standard model. For instance, implementation of Gilda’s DRS is associated with improved perceived informal procedural justice and improved perceived interactive justice. Employees reported feeling more comfortable reporting problems to their immediate supervisors with the DRS than when no DRS was in place, and they reported feeling that their local managers care about their concerns and are more interested in their opinions on work-related issues with the DRS in place. Essentially, limitations of this natural experiment notwithstanding, the evidence in this paper suggests that implementation of Gilda’s DRS improved “localized” employee voice.

Our analysis leads us to advance a revised, bicephalous model of the effects of formal and informal procedural justice that predicts that localized positive effects of workplace DRS implementation come with a price of corresponding centralized negative effects. Specifically, implementation of the DRS is associated with reduced perceptions of formal procedural justice. Furthermore, the DRS has no discernible effect on organizational commitment, at least not in a way that could be teased out using the same techniques as those applied to the other variables. One would expect formal procedural justice to move in the same direction as perceptions of informal procedural justice and interactive justice. However, our model posits that employees draw a sharp distinction between their trust and comfort with their immediate supervisors and their trust and comfort with Gilda’s as an organization. Figure 3 illustrates the proposed
bicephalous model connecting DRS implementation, procedural justice, and organizational commitment.

This seeming inconsistency could be a function of two things. First, it might be the result of the way in which local managers explain Gilda’s DRS to employees and the fact that local supervisors are perceived by Gilda’s corporate HR Department as being better at their jobs if fewer employee claims are escalated. They are encouraged and rewarded for resolving claims at the local level. Immediate supervisors might be incentivized to encourage their employees to trust them to resolve claims instead of allowing HR or Gilda’s as a corporate entity to do so. Perhaps local managers actively encourage employees to trust them, which could be interpreted implicitly as being encouraged to distrust Gilda’s DRS or the company writ large.

Implementation of the DRS is most strongly associated with an increase in resident employees’ perceptions that the Company is in compliance with the law. This might seem counter-intuitive at first given the lack of effect on organizational commitment and the decrease in perceived formal procedural justice, which could be interpreted as a lack of trust in Gilda’s to fairly resolve disputes internally. Why would employees regard the Company as more compliant with the law if it is less fair in resolving disputes? One possible explanation is the heightened salience of legal compliance that might be assumed from the Company’s new requirement to sign a form-contract requiring acquiescence to final, binding, mandatory arbitration. This is especially likely to be the case here since the form explicitly mentions coverage of employment discrimination and harassment and other statutory claims. Employees may regard the formal processes associated with the DRS and Gilda’s requirement that they contract away their right to a jury trial as a signal that the Company takes its legal rights and obligation very seriously. This explanation derives from employees’ perceptions of the formal aspect of the DRS.

Alternatively, employees might believe the Company complies with the law because of the heightened trust and comfort with local management. Employees might feel that they are treated fairly by local supervisors, they have increased opportunities for voice, and that their concerns are genuinely heard by local management. In turn, employees may be more likely to
perceive local management as being compliant with laws that prohibit discrimination based on gender, race, or age. This explanation accords with research that suggests that employees over-broadly characterize “unfair” behavior as “illegal” (Bies and Tyler 1993). It also implies the converse: that they over broadly characterize “fair” behavior as “legal.” It is hard to tell if the augmented legal compliance perceptions are related to the positive effects of informal procedural justice or the negative effects of formal procedural justice. Further research should explore which accounts for the observed effects.

This study offers rare insight into employees’ perceptions of important workplace metrics before and after the implementation of a workplace DRS. The findings suggest that more attention should be paid to the bifurcated component parts of dispute resolution systems generally. Our findings also support complementarity models of dispute resolution like that offered by Bendersky (2003), which suggest that varying the degree of input into the process and the flavor of dispute resolution system at-hand differentially impacts upon the non-disputant population. Lastly, this research indicates the need to focus research efforts on understanding differential effects of organizational change on perceptions of local versus nonlocal management, particularly as related to the exit-voice-loyalty model. As other studies have shown, employees may differ in the degree to which they feel committed to organizations locally versus globally (Gregersen and Black 1992) and organizationally versus personally (with respect to their immediate supervisors) (Becker et al. 1996). As illustrated by the full version of the bicephalous model shown in Figure 4, it could be that there is a trade-off between formal and informal procedural justice that results in less organizational commitment, but greater interpersonal commitment (to immediate supervisors and managers), concurrently with increased perceived legal compliance by the organization.

Limitations

This study has a number of limitations. Like most studies that exploit data from a single organization, one can reasonably question the generalizability of our findings. However, as noted earlier, the four-step process instituted at Gilda’s—where disputes are first dealt with informally, and then with increasing formality, until the final step, binding arbitration—very closely resembles the archetypal DRS found in non-union firms (Colvin 2010; Eigen, Menillo and Sherwyn 2012). Indeed, this is part of what made Gilda’s such an attractive site for
undertaking the first analysis of this type, in which a wide range of workforce and work-related variables could be held in place. To the extent that legitimate challenges to the generalizability of our findings persist, we believe that the internal validity allowed for by the organizationally-bounded nature of our data is a worthwhile dividend, particularly given the dearth of pre/post analysis of the impact of a DRS.

Closely related to the issue of generalizability—or external validity—is the notion of construct validity. As mentioned above, the data relied upon in this analysis were not originally collected for research purposes, and therefore, do not reflect the level of care and meticulousness that researchers take when crafting survey instruments and constructing variables for analysis. Most importantly, Gilda’s and its survey vendor made limited appeal to pre-validated scales. They also allowed for slight changes year-to-year and between regions in both the precise phrasing of and in the composition of survey items, challenges that we addressed as conservatively and transparently as possible. Notwithstanding these issues, we defend the use of these data in two ways. First, the questions asked on the annual survey likely reflected issues with which Gilda’s management were most concerned. The information provided in the survey responses—the exact same data that we reply upon in our analysis—underpinned the firm’s justification for creating a structured approach to dispute resolution in the first place and fed the firm’s dashboard for assessing the effectiveness of the DRS. Second, while items from validated scales would have been preferred, some have argued that such survey items—typically developed in a very different context than the one in which they are ultimately employed—often create “frame-of-reference” problems (Hunter and Pil 1995). As in previous studies that make use of study-specific survey items (e.g., Bidwell 2009; Litwin 2011), we can at least be more assured that survey respondents understood the questions they were being asked and the situational context to which the questions were referring.

A separate issue that comes into play is that of nonrandom sample attrition. Specifically, given the design of the dataset and what theory tells us about dispute resolution systems, it is possible that some workers reacted to the new system “with their feet.” That is, they exited the work situation rather than staying in the organization and revealing changes in their perceptions of justice. To the extent that those most likely to sense injustice or least committed to the organization were more likely to leave the organization, their absence in the post-DRS period could bias the estimated effects of the DRS. Note that we do not try to decompose the DRS’s ability to “change minds” from its instrumentality over the composition
of the workforce. Rather, we demonstrate changes in within-store, workforce means in the dependent variables over the sample period. Nonetheless, one can re-run all of the analyses in the paper on the subset of respondents in the post-DRS period that were reportedly working for Gilda’s in the pre-DRS period. Indeed, all of the estimates run on this limited dataset are qualitatively identical to the ones run on the full dataset and presented above.

Finally, while our models are careful to control for time-constant, unobserved factors at the store level, the nature of the “treatment”—putting the DRS into effect—does not allow for an econometric means by which to fully control for time-varying drivers at the store level. This would only be feasible if the DRS itself went into effect at different times in different stores, allowing us to “partial out” the impact of unobserved variables, be they spatial or temporal, using an event study-type framework (McWilliams and Siegel 1997). Therefore, we must rely on our own observation as well as consistent reports from Gilda’s HR professionals and managers that there were no systematic changes in the work environment within stores over the observation period. Indeed, as noted above, it is the richness of this qualitative information that made the research setting so appealing despite the aforementioned challenges that it creates for generalizability.

**Conclusion**

We offer the bicephalous model linking employment dispute resolution with justice, organizational commitment, interpersonal commitment, and perceived organizational legal compliance as a means of sparking conversations about the relationship among these constructs. Of course, as mentioned above, we do not set out to fully test the model. It is offered to reconceptualize the relationship among these constructs. In addition to advancing theory, dispute system design might benefit from contemplating how to plan for an environment of diminished trust in centralized management or the firm itself as the cost of augmented trust in localized management. Perhaps this is precisely what unintentionally yielded the positive results in Gilda’s system in tandem with the increased perception of legal compliance. Is such a trade-off avoidable? Is it possible to flip in the other direction—augmented, global, organizational trust in exchange for decreased, localized trust for effective and fair dispute resolution? Lastly, this research raises important questions about the relationship between

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6 The survey asks employees to self-report how many years they have been Gilda’s employees.
organizational commitment and procedural justice. These and other questions emergent from this study deserve continued attention given the vast number of organizations contemplating the implementation of a DRS, often without knowledge of the trade-offs this study suggests.
REFERENCES


### Table 1. Variable Definitions and Construction

<table>
<thead>
<tr>
<th>Variable</th>
<th>Definition in italics, with actual survey questions underneath</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Dispute Resolution System (DRS)</strong></td>
<td>Is the DRS in place? (binary variable in which 0 = &quot;no&quot; and 1 = &quot;yes&quot;)</td>
</tr>
<tr>
<td><strong>Formal Procedural Justice</strong></td>
<td>The Company, its HR department, and formal mechanisms for appealing adverse employment actions are accessible and viable options for redressing disputes.</td>
</tr>
<tr>
<td></td>
<td>This location has an effective means of appealing discipline and discharge actions.</td>
</tr>
<tr>
<td></td>
<td>My human resource manager or director of administration is good at solving &quot;people problems.&quot;</td>
</tr>
<tr>
<td></td>
<td>If I can’t get a problem resolved in my store, HR continues to be accessible to me and provides guidance.</td>
</tr>
<tr>
<td></td>
<td>I believe that this company is committed to resolving associate concerns or problems quickly and in a fair manner.</td>
</tr>
<tr>
<td><strong>Informal Procedural Justice</strong></td>
<td>Local store managers and supervisors are accessible and viable options for redressing disputes.</td>
</tr>
<tr>
<td></td>
<td>If I have a problem, there is someone in senior management I can go to who will assist me.</td>
</tr>
<tr>
<td></td>
<td>My manager is good at solving &quot;people problems.&quot;</td>
</tr>
<tr>
<td></td>
<td>If someone’s behavior made me uncomfortable at work, I would definitely report the problem.</td>
</tr>
<tr>
<td></td>
<td>If I have a problem or concern, I would feel most comfortable going to my direct supervisor.</td>
</tr>
<tr>
<td></td>
<td>If I have a problem, there is someone in management I can go to who will assist me.</td>
</tr>
<tr>
<td></td>
<td>If I have a problem, there is a manager in my location I can go to for help.</td>
</tr>
<tr>
<td><strong>Interactive Justice</strong></td>
<td>Local store managers and supervisors listen to and interact with employees showing them courtesy and respect.</td>
</tr>
<tr>
<td></td>
<td>My manager is interested in my opinion on work-related issues.</td>
</tr>
<tr>
<td></td>
<td>My manager regularly shows me that he/she cares about me.</td>
</tr>
<tr>
<td></td>
<td>My general manager listens to employees’ concerns, issues, and suggestions.</td>
</tr>
<tr>
<td></td>
<td>My direct supervisor is interested in my opinion on work-related issues.</td>
</tr>
<tr>
<td></td>
<td>My direct supervisor treats me with courtesy and respect.</td>
</tr>
<tr>
<td></td>
<td>My direct supervisor regularly shows me that he/she cares about me.</td>
</tr>
<tr>
<td></td>
<td>Once my schedule is posted, my manager only makes changes that I agree to.</td>
</tr>
<tr>
<td><strong>Legal Compliance</strong></td>
<td>The workplace is in compliance with the law, particularly laws against harassment and discrimination.</td>
</tr>
<tr>
<td></td>
<td>I feel my work environment is free from harassment and discrimination.</td>
</tr>
<tr>
<td></td>
<td>My supervisor manages everyone in my group equally regardless of their race, age, or sex.</td>
</tr>
<tr>
<td></td>
<td>My direct supervisor manages everyone in my group equally regardless of their race, age, or sex.</td>
</tr>
<tr>
<td></td>
<td>I think the company is doing a great job of providing a workplace free from intimidation,</td>
</tr>
<tr>
<td></td>
<td>Racial, ethnic, and gender-based comments/jokes are not tolerated at this location.</td>
</tr>
<tr>
<td></td>
<td>I feel I am treated fairly and with respect regardless of my race, gender, or age.</td>
</tr>
<tr>
<td></td>
<td>I feel my work environment is free from discrimination based upon gender, race, age, or sexual orientation.</td>
</tr>
<tr>
<td><strong>Organizational Commitment</strong></td>
<td>Employees feel committed to the organization.</td>
</tr>
<tr>
<td></td>
<td>Work like mine greatly encourages me to do my best.</td>
</tr>
<tr>
<td></td>
<td>The supervision I receive is the kind that greatly encourages me to give extra effort.</td>
</tr>
<tr>
<td></td>
<td>I expect to be working here one year from now.</td>
</tr>
<tr>
<td></td>
<td>My job is considered important in this company.</td>
</tr>
<tr>
<td></td>
<td>I believe in what we are doing in this store.</td>
</tr>
<tr>
<td></td>
<td>I feel positive about the opportunity here to advance my career.</td>
</tr>
</tbody>
</table>

*Note: With the exception of the DRS dummy, all variables are ordered categoricals on a Likert-type scale such that 1 = "strongly disagree" and 5 = "strongly agree."*
<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Formal Procedural Justice</td>
<td>93</td>
<td>7,740</td>
<td>11</td>
<td>605</td>
<td>0</td>
<td>0</td>
<td>132</td>
<td>9,594</td>
</tr>
<tr>
<td>Informal Procedural Justice</td>
<td>93</td>
<td>7,925</td>
<td>98</td>
<td>6,746</td>
<td>129</td>
<td>9,908</td>
<td>132</td>
<td>9,571</td>
</tr>
<tr>
<td>Interactive Justice</td>
<td>93</td>
<td>7,930</td>
<td>98</td>
<td>6,739</td>
<td>133</td>
<td>10,122</td>
<td>133</td>
<td>9,648</td>
</tr>
<tr>
<td>Legal Compliance</td>
<td>93</td>
<td>7,928</td>
<td>100</td>
<td>6,793</td>
<td>133</td>
<td>10,124</td>
<td>133</td>
<td>9,650</td>
</tr>
<tr>
<td>Organizational Commitment</td>
<td>93</td>
<td>7,938</td>
<td>100</td>
<td>6,800</td>
<td>133</td>
<td>10,129</td>
<td>133</td>
<td>9,656</td>
</tr>
</tbody>
</table>

Note: Each cell contains the number of non-missing observations for each dependent variable, by year.
### Table 3. Means, Standard Deviations, and Correlations for Study Sample

<table>
<thead>
<tr>
<th>#</th>
<th>Variable</th>
<th>Mean</th>
<th>SD</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>Dispute Resolution System</td>
<td>.56</td>
<td>.50</td>
<td>1.00</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2</td>
<td>Formal Procedural Justice</td>
<td>3.24</td>
<td>.34</td>
<td>-.13</td>
<td>1.00</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>3</td>
<td>Informal Procedural Justice</td>
<td>3.80</td>
<td>.31</td>
<td>.57</td>
<td>1.00</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>4</td>
<td>Interactive Justice</td>
<td>3.88</td>
<td>.31</td>
<td>.46</td>
<td>.72</td>
<td>1.00</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>5</td>
<td>Legal Compliance</td>
<td>3.80</td>
<td>.32</td>
<td>.51</td>
<td>.69</td>
<td>.61</td>
<td>1.00</td>
<td></td>
<td></td>
</tr>
<tr>
<td>6</td>
<td>Organizational Commitment</td>
<td>3.63</td>
<td>.25</td>
<td>-.14</td>
<td>.75</td>
<td>.60</td>
<td>.51</td>
<td>.43</td>
<td>1.00</td>
</tr>
</tbody>
</table>

Note: Sample sizes differ by cell, but range from \( n = 798 \) to \( n = 1,171 \) store-years.

Key: All correlation coefficients are significant at \( p < .001 \).

### Table 4. Mean Values for Dependent Variables Before and After the Implementation of a Dispute Resolution System, Not Accounting for Unobserved Differences Across Stores

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>Pre-DRS</th>
<th>Post-DRS</th>
<th>Difference</th>
<th>( z )-statistic</th>
</tr>
</thead>
<tbody>
<tr>
<td>Formal Procedural Justice</td>
<td>3.30</td>
<td>3.21</td>
<td>-.09</td>
<td>-4.29 ***</td>
</tr>
<tr>
<td>Informal Procedural Justice</td>
<td>3.70</td>
<td>3.89</td>
<td>.19</td>
<td>11.51 ***</td>
</tr>
<tr>
<td>Interactive Justice</td>
<td>3.76</td>
<td>3.99</td>
<td>.23</td>
<td>14.56 ***</td>
</tr>
<tr>
<td>Legislative Compliance</td>
<td>3.72</td>
<td>3.89</td>
<td>.16</td>
<td>11.20 ***</td>
</tr>
<tr>
<td>Organizational Commitment</td>
<td>3.66</td>
<td>3.60</td>
<td>-.06</td>
<td>-4.84 ***</td>
</tr>
</tbody>
</table>

Notes: Table reports pre-DRS and post-DRS means calculated as the linear combination of the constant and the coefficient on a DRS dummy for random-effects regression estimates of each of the dependent variables. Statistical significance accounts for the clustering of observations by store.

Key: * \( p < .05 \), ** \( p < .01 \), *** \( p < .001 \).
### Table 5. Mean Values for Dependent Variables Before and After the Implementation of a Dispute Resolution System, Accounting for Unobserved Differences Across Stores

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>Pre-DRS</th>
<th>Post-DRS</th>
<th>Difference</th>
<th>t-statistic</th>
</tr>
</thead>
<tbody>
<tr>
<td>Formal Procedural Justice</td>
<td>3.31</td>
<td>3.20</td>
<td>-.11</td>
<td>-4.14***</td>
</tr>
<tr>
<td>Informal Procedural Justice</td>
<td>3.69</td>
<td>3.90</td>
<td>.21</td>
<td>10.89***</td>
</tr>
<tr>
<td>Interactive Justice</td>
<td>3.73</td>
<td>3.99</td>
<td>.26</td>
<td>15.14***</td>
</tr>
<tr>
<td>Legislative Compliance</td>
<td>3.70</td>
<td>3.87</td>
<td>.17</td>
<td>10.72***</td>
</tr>
<tr>
<td>Organizational Commitment</td>
<td>3.65</td>
<td>3.61</td>
<td>-.04</td>
<td>-3.03**</td>
</tr>
</tbody>
</table>

**Notes:** Table reports pre-DRS and post-DRS means calculated as the linear combination of the constant and the coefficient on a DRS dummy for fixed-effects regression estimates of each of the dependent variables. Statistical significance accounts for the clustering of observations by store.

**Key:** * p < .05, ** p < .01, *** p < .001.

### Table 6. Fixed-Effects Estimates of the Impact of a Dispute Resolution System on Employment Relations Outcomes

<table>
<thead>
<tr>
<th></th>
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</tr>
</thead>
<tbody>
<tr>
<td>Dispute Resolution System</td>
<td>-.13**</td>
<td>.30***</td>
<td>.14***</td>
<td>.36***</td>
<td>.001</td>
</tr>
<tr>
<td></td>
<td>(-2.22)</td>
<td>(6.10)</td>
<td>(3.91)</td>
<td>(9.60)</td>
<td>(0.03)</td>
</tr>
</tbody>
</table>

**Note:** Table reports coefficient estimates and associated t-statistics from longitudinal regression models on panel data, with standard errors clustered by store location. All models include dummy variables for each survey year and for each store. Therefore, models estimate fixed-year and fixed-store effects, where ρ is a measure of intraclass correlation. The $R^2$ reported is the "within-" $R^2$ meaning that it does not reflect the explanatory power arising from the large vector of store dummies or from "between-store" differences in the independent variables.

**Key:** * p < .10, ** p < .05, *** p < .01.
FIGURES

Figure 1. Standard Model Connecting DRS Implementation, Procedural Justice, and Organizational Commitment

Figure 2. Observed Means of Dependent Variables by Year (Individual Level Data)
Figure 3. Bifurcated Model Connecting DRS Implementation, Procedural Justice, and Organizational Commitment—Simplified Version

Figure 4. Bifurcated Model Connecting DRS Implementation, Procedural Justice, and Organizational Commitment—Full Version