

**JUSTICE OR JUST BETWEEN US? EMPIRICAL EVIDENCE OF THE TRADE-OFF BETWEEN  
PROCEDURAL AND INTERACTIONAL JUSTICE IN WORKPLACE DISPUTE RESOLUTION**

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**ABSTRACT**

This paper draws on unique data from a single, geographically-expansive, US firm with well over 100,000 employees in over 1,000 locations to examine the relationship between an employer's implementation of a typical dispute resolution system (DRS) and organizational justice, perceived compliance with the law, and organizational commitment. Holding all time-constant, location-level variables in place, we find that introduction of the DRS is associated with elevated perceptions of interactional justice but diminished perceptions of 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 "differential-effects" model for conceptualizing the relationship among organizational justice, perceived legal compliance, and implementation of dispute resolution mechanisms.

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This paper examines the effects of implementation of a typical workplace dispute resolution system (“DRS”) on employees covered by the DRS, the vast majority of whom are never claimants or participants in a claim submitted through the DRS. This is made possible by access to a unique set of survey data from a large national company that, over the eight years of the study, employed over 100,000 workers in over 1,000 store locations. The employer implemented the DRS, described in more detail below, in the middle of the eight-year span, enabling a rare glimpse into pre- and post-implementation perceptions of justice, organizational commitment, and the degree to which employees believe the company is complying with employment laws. We rely on these data and qualitative data collected from individuals working for the employer over a two-year period to present what is essentially a case in which extant theory fails to explain observed outcomes. Specifically, theory predicts that implementation of a workplace DRS should improve employees’ perceptions of procedural justice, but our findings suggest the antithetical result. Additionally, implementation of the DRS in the data studied corresponded with improved perceived interactional justice, and perceived legal compliance, suggesting a potential trade-off in effects associated with the DRS, not previously conceptualized or documented. From this we advance a new conceptual model of the relationship among DRS implementation, organizational justice, and perceived legal compliance. We first offer some background on the existing scholarship examining organizational justice and the connection between it and workplace outcomes and behaviors. We then explain the methods, data, and results of the instant study. It is important to initially explain why DRS implementation is thought to increase procedural justice, which in turn, increases organizational trust, and leads to positive outcomes associated with improved organizational commitment.

### **Organizational Justice**

Organizational justice is the measure of how fairly employees feel treated at work in terms of outcomes and processes. It is commonly sub-classified into three distinct sub-categories: distributive, procedural, and interactional justice (Cohen-Charash and Spector 2001; Colquitt, Conlon, Wesson, Porter, and Ng 2001; Cropanzano, Prehar, and Chen 2002). Distributive justice gauges fairness or equity of outcomes or rewards (Folger 1977). Procedural justice gauges fairness of processes (Lind and Tyler 1988; Thibaut and Walker 1975). Interactional justice measures the perceived fairness of interpersonal treatment (Bies and Moag

1986). Scholarship suggests that the more fairly employees feel they are treated, the more likely they are to be committed to the organization (Hubbell and Chory-Assad 2005), and to display what is often referred to as “organizational citizenship behavior” (Moorman 1991; Moorman, Niehoff, and Organ 1993; Niehoff and Moorman 1993). This includes things like improved productivity, performance, and related measures (Greenberg and Barling 1996). Organizational commitment may also be measured in terms of reduced turnover (Byrne 2005; Spencer 1986). Conversely, when employees feel that their employers treat them *unfairly* in terms of decision-making or outcomes, employees are more likely to feel *less* committed to the organization, are more likely to exit the firm, and are more likely to enact counterproductive work behaviors such as shirking, theft, or neglect of duty (Cohen-Charash and Spector 2001; Folger 1993; Greenberg 1990; Greenberg 1993; Sheppard, Lewicki, and Minton 1992). While researchers have studied all three subcategories of organizational justice, the one thought to be most important in understanding employee reactions to managerial policies and decision-making is procedural justice (Colquitt 2001; Colquitt et al. 2001; Naumann and Bennett 2000). In short, more than four decades of research suggests two things. First, it is important to measure under what circumstances and how much employees feel that they are fairly treated at work in terms of all three varieties of organizational justice, but perhaps most importantly with respect to procedural justice. Second, employees’ perceptions about how fairly they are treated at work correlates with behaviors and outcomes of deep concern to policy makers, politicians, union organizers, employers, researchers, and anyone else generally concerned with workplace governance, efficiency, productivity, and stability.

### **Justice and Workplace Dispute Resolution**

One of the most critical ways of assessing organizational justice is evaluating how organizations address disputes (Blancero, DelCampo, and Marron 2010; Boroff 1991; Colvin, Klaas, and Mahony 2006; Lewin 1987). Improved perceived legitimate opportunities for voice and greater perceived access to procedurally fair and neutral processes of resolving disputes should increase employees’ organizational commitment and lower turnover, consistent with the “exit-voice-loyalty” trade-off (Folger 1977; Hirschman 1970; McCabe and Lewin 1992). Resolving conflict internally also reduces costs because litigating disputes is often more expensive than addressing them internally (Estreicher and Eigen 2010). Greater perceived organizational justice (particularly procedural justice) has been shown to correlate with less

employee complaining about legal issues and less external resort to law (Wallace, Edwards, Mondore, and Finch 2008). Along these 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., alleging that the unfair treatment was a manifestation of employment discrimination). Private mediation and arbitration have been shown to result in high levels of participant satisfaction (Lipsky and Seeber 2006; Lipsky, Seeber, and Fincher 2003). There is a substantial body of literature suggesting that increased participation in dispute resolution processes increases the likelihood that claimants will regard their claims as justly addressed. 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. It is less clear if, whether, and to what extent employees regard the implementation or general availability of a workplace DRS as a fair action taken by the employer, when those employees are not claimants or participants in claims addressed by the DRS.

All three sub-categories of organizational justice may be useful for assessing the overall fairness of employer-implemented policies or programs like a dispute resolution system, but research has suggested that procedural justice is the most useful and likely the most salient (Konovsky and Folger 1991). Some have suggested that while distributive justice is useful for predicting personal-level evaluations (such as pay satisfaction or outcomes of specific disputes in which an individual was the claimant), procedural justice is perhaps the most useful metric for understanding organizational-level evaluations (like organizational commitment) (McFarlin and Sweeney 1992; Sweeney and McFarlin 1993). Procedural justice has been the focus of many studies of employer policies like DRSs. 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, at least as measured from the employer's perspective (Folger and Bies 1989; Sheppard, Lewicki, and Minton 1992; Tyler and Bies 1990). The consensus reached by prior scholarship is that procedural justice matters, is distinct from the other types of organizational justice, and is more likely to influence overall fairness judgments than distributive justice (Greenberg and Cropanzano 2001; Konovsky and Folger 1991).

Prior research theorizes and predicts a positive correlation between DRS implementation and improved procedural justice perceptions among employees (Blancero,

DelCampo, and Marron 2010; Colvin 2003; Thibaut and Walker 1975; Tyler 1988; Tyler and Bies 1990). However, some believe that this is only true in union environments, or when there is a high degree of organizational trust. 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 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 organizational justice before implementation of a workplace DRS. Research has focused 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 2011; Sherwyn, Estreicher, and Heise 2005), disputants' perceptions of procedural justice (Aram and Salipante 1981; Fryxell and Gordon 1989), and post-grievance outcomes for claimants (Lewin 1987; Lewin and Peterson 1999) among other important measures.

As others have observed, in stark contrast to procedural justice, interactional (or interactive) justice is the most understudied component of organizational justice (Hubbell and Chory-Assad 2005). In spite of this, it is expected to be an important component of organizational justice, and related most closely to how employees perceive their supervisors (Tyler and Blader 2000). Interactional 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; Bies and Moag 1986; Masterson, Lewis, Goldman, and Taylor 2000). While these issues have been examined, sometimes even in the context of managerial trust and related concepts, there are few, if any attempts to understand the interrelationship among interactional justice, procedural justice, and DRS

implementation. One study by Hubbell and Chory-Assad (2005) explores the relationship between procedural and interactional justice and managerial and organizational trust. They find that procedural justice is the strongest predictor of both organizational and managerial trust, distributive justice only predicts managerial trust, and interactional justice does not predict either type of trust. However, this study was not in the context of a DRS implementation.

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; Tyler and Blader 2000). 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 interactional justice that gauges employees' beliefs about their managers' interest in their opinions in work-related issues and how much their managers treat them with courtesy and respect among other related measures. There are other variations on this measure, but for the most part, measures of interactional justice cover the same ground.

### **The Standard Model of DRS Implementation, Organizational Justice, and Commitment**

Figure 1 visually depicts the standard model connecting DRS implementation, procedural justice, and organizational commitment as theoretically and empirically described in the literature. As organizational trust features in past work on these constructs, it is worth briefly explaining how it fits in here. Organizational trust has been described as a "feeling of confidence and support in an employer," and the belief that the employer will follow through on commitments (Gilbert 1998). It is commonly conceptualized as a product of organizational justice (Ambrose and Schminke 2003; Cropanzano, Prehar, and Chen 2002; Folger and Konovsky 1989; Whitener 2001). The logic of this might be cast as follows: *if an employer treats an employee fairly, that employee is more likely to trust the employer*. However, organizational trust may also be the antecedent of organizational justice. The logic of this could be cast as follows: *if an employer is trustworthy, the employee is more likely to regard the employer's treatment as fair*. This is plausible because of asymmetric information in most employment relationships. Employees often lack sufficient baseline information to which they may compare decisions employers render. This makes it more likely for employees to rely on trust in assessing whether the

alternatives to an observed decision are less favorable than the one observable result. Therefore, as suggested by other researchers, it is most likely the case that there is bi-directionality between organizational trust and organizational justice (Colvin, Klaas, and Mahony 2006). To accord this bi-directionality, in Figure 1, organizational trust is shown with double-headed arrows connecting to procedural justice. Organizational trust has been demonstrated to be positively correlated with organizational commitment (Ruppel and Harrington 2000; Whitener 2001), so this is also shown in Figure 1. 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.

[—INSERT FIGURE 1 ABOUT HERE.—]

### **The Differential Effect Model**

This paper suggests a reassessment of the relationship among the important constructs discussed above. We first note that many DRSs, like the one examined in this study, consist of multiple steps beginning with reporting to local managers and culminating in final, binding arbitration (Estreicher and Eigen 2010). Other research suggests that it makes sense to pay attention to the possible differences in judgments about justice that could come from formal versus informal DRS components. For instance, Tyler and Blader (2000) note that judgments about justice may be characterized differently for perceptions that come from observing formal rules versus observing informal actions by authorities. DRSs often consist of components relating to perceptions about how fair the organization is (the more formal components of the DRS), *and* components relating to perceptions of how fair local managers are (the less formal components of the DRS). It therefore makes sense to account for this bifurcation when conceptualizing a model describing the effects of DRS implementation. Building on concepts emergent from the literature and grounded in the empirical analysis presented herein, we have done just this. Our model suggests that the act of implementing a DRS with both informal and formal components creates a set of trade-offs. Instead of a unidirectional, universal effect stemming from DRS implementation to increased perceived procedural justice to increased organizational commitment, we theorize that DRS implementation may instead be associated with differential effects that simultaneously increase one form of organizational justice

(interactional) while diminishing another form of organizational justice (procedural). Local managers charged with implementing the DRS and explaining it to their employees are incentivized to act in ways that bolster their perceived trustworthiness and fairness as an *alternative* to resorting to the company's newly implemented formal internal process. Indeed, Colvin's (2004) qualitative analysis of a nonunion dispute resolution system much like the one under study here revealed behavioral changes along these lines on the part of managers. After all, local managers do not want to be regarded as failing to handle disputes arising at their locations, and claims that they fail to manage end up being resolved by the DRS's centrally administered formal componentry. Abundant claim escalation might be perceived as suggesting that local managers are doing something personally wrong. So, local managers likely do what they can to increase interactional justice perceptions among their employees in order to decrease the likelihood of employees resorting to the more formal components of the DRS. One result of this structural disincentivization for local managers to champion the formal components of the DRS as procedurally fair could be diminished procedural justice as perceived by employees. In other words, because local managers are the gate-keepers of the DRS, implementation of a DRS could yield increased perceived interactional justice at the expense of diminished perceived procedural justice. Applying existing research and theory, if our differential effects model of organizational justice holds and DRS implementation is associated with an increase in interpersonal justice, there should be a corresponding increase in interpersonal commitment. The simultaneous diminished perceptions of procedural justice should have no effect (or a net negative effect) on organizational commitment.<sup>1</sup>

### **Perceived Legal Compliance**

Aside from this potential differential effect on organizational justice, we suggest examining the connection between DRS implementation and perceived legal compliance by the organization. The connection between justice and legal compliance is well established in other contexts, but is relatively understudied in the organizational justice literature (Bies and Tyler 1993; Tyler 2006). Local managers and employees could regard the company's newly implemented process as being established to protect the company's interests. Because local managers do not want to be perceived as failing to damn the tide of disputes that percolate up from their locations (Colvin 2004), the very act of implementing a DRS at a national

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<sup>1</sup> Our differential effects model is only partially tested by the data described below; we do not have the ability to test whether the connection between interpersonal justice and interpersonal commitment exists.

organization might have the effect of setting the stage for local managers to step to the side of their employees. The “us” in the proverbial “us versus them” could shift closer to including managers and employees together. The “them” becomes more saliently the amorphous corporate entity implementing the formal DRS apparatus. This sets the stage for implementation of the DRS to be associated with an increase in the degree to which employees perceive that the company is complying with the law.

Perceived legal compliance is an often overlooked but critical component of procedural justice (Gopinath and Becker 2000; Tyler 1988). We suggest that perceived legal compliance may not positively correlate with procedural justice in the context of DRS implementation in a non-union workplace. Employees may regard implementation of a workplace DRS as a signal that their employer is more likely in compliance with state and federal laws because the employer cares about legal issues and wants to address them. If employees regard DRS implementation as providing a fair benefit above and beyond the set of legal protections and right to which they feel entitled under the status quo, then perceived legal compliance and procedural justice should both increase with DRS implementation. Alternatively, however, employees could fundamentally distrust their employer. They would regard DRS implementation as a signal that the employer is protecting its interests and taking precautions to avoid liability. In this case, employees would distinguish between an employer protecting its interests by complying with minimum laws regulating the workplace, and fairness interests above and beyond those minimums. Employees might then regard the employer as more likely to comply with the letter of the law—for instance, avoiding overtly discriminating based on gender or race. In this case, perceived legal compliance should increase, while procedural justice should decrease following DRS implementation. This view is plausible in light of recent findings by Feldman and Tyler (2012) on perceived procedural justice when employers are legally mandated to implement fair procedures. The authors hypothesize that employees distinguish between “mandates that provide a basic level of security in the workplace and additional procedural mechanisms that they view as more discretionary.”

Research on mandatory arbitration of employment disputes lends further support for the notion that employees’ perceived legal compliance could increase when procedural justice decreases (with DRS implementation). Researchers generally find positive justice perceptions associated with voluntary dispute resolution processes or those in unionized settings that were the result of collective bargaining. However, whether mandatory arbitration can similarly

enhance perceptions of justice in a non-union setting when an employer unilaterally implements the DRS 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 suggests another reason why procedural justice perceptions might decrease while perceived legal compliance increases with DRS implementation in a non-union setting. A qualitative study of employees who signed mandatory arbitration agreements as a condition of their employment suggests 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. 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, even while simultaneously augmenting the degree of perceived legal compliance by the company on things like sexual harassment and discrimination. This is consistent with Feldman and Tyler's recent work describe above (2012). Figure 2 presents the differential effects model depicting the interrelationship among DRS implementation, organizational justice, and perceived legal compliance.

[—INSERT FIGURE 2 ABOUT HERE.—]

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 affects organizational justice and organizational commitment for that vast share of employees who never bring any complaints through the system (Budd and Colvin 2008; Mahony and Klaas 2008). Much has been said about employee/claimants and their experiences with dispute resolution. Much has been written about the difference between claimants and non-claimants. But very little is known about the impact (if any) of implementation of a workplace DRS on current employees who know about the DRS's implementation but will likely never use it or participate in a claim brought through the DRS. Casting this wider net of employees is useful because research has

mostly focused on claimants and participants, or has compared those smaller groups of employees to non-claimants post-implementation of a DRS. The authors believe that this is the first attempt to offer an empirical assessment of the impact of implementing a workplace dispute resolution system in a non-union setting on this wider cast net of all currently employed workers using pre- and post-DRS implementation data on organizational justice, organizational commitment, and perceived legal compliance of a group of employees in the private sector large enough to render meaningful statistical analysis possible.

We focus on employees' perceptions of justice and other survey data reporting their opinions—but not behavior such as turnover—for three reasons. First, for employees who have not initiated disputes themselves, one would not expect there to be any significant effect of implementation of a DRS on exiting behavior. The low-skilled nature of the work of frontline employees' at Gilda's disadvantages these employees in the outside labor market, suggesting that, in this context, exit rates would be a flawed measure of the impact of the DRS (Edwards and Scullion 1982). Second, behavioral measures like firm exit are more likely explained by factors other than DRS implementation, such as how badly employees need the job or whether their expectations for the job are short- or long-term. Third, employees cannot exit the firm anonymously, but they can (and do, in the data we analyzed) report their honest views about the critical constructs like organizational justice, organizational commitment, and perceived legal compliance. Because these constructs are critically linked to other important measures described above, the survey data we use are uniquely suited to the present analysis.

### **The Employer's DRS**

As noted above, we analyze eight years' worth of data from a single company that operates in the US, "Gilda's, Inc."<sup>2</sup> It employs over 100,000 workers in more than 1,000 store locations. The company 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 Human Resources (HR) department that the rollout 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

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<sup>2</sup> "Gilda's" is the pseudonym chosen to protect the company's anonymity. The authors are bound by a confidentiality agreement with Gilda's that restricts our ability to offer more background information about the organizational and workplace context surrounding the DRS implementation.

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 HR department, which maintained a toll-free, call-in number for employees who wished to make claims anonymously.

Like many workplace DRSs, the program at Gilda's starts informally with claim initiation with local supervisors, escalates to formal review by corporate-level HR, and culminates in final, binding, mandatory arbitration. More specifically, 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 corporate HR, and someone will attempt to resolve the claim, (3.) If unresolved to his satisfaction by HR, a claimant may then escalate his claim and choose between a determination made by a peer-review panel consisting of three fellow employees, 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. Ultimately, local managers are responsible for explaining the DRS to their employees.

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 635 different stores over the eight-year period.

The second challenge this company-collected data makes for research is the roster of actual survey items and the indices that we worked to create from them. 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."<sup>3</sup> 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

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<sup>3</sup> The surveys also posed two or three open-ended text questions, but we were not provided the employees' responses to these questions.

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 its constituent items, thereby preventing our use of the standard estimate of reliability, or of similar data reduction techniques such as factor analysis.<sup>4</sup> Consequently, we created scales by taking “row means”—averaging scores across the items comprising a given index or scale, for however many non-missing items are available, for a given observation.<sup>5</sup> The end result is a set of four 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. Moreover, the four 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. Procedural justice relies on eight items to assess employees’ perceptions that they 1.) have access to formal mechanisms for appealing adverse employment actions, and 2.) are afforded the opportunity to speak up in their dealings with the company and its agents. This comports with the way others have defined and measured procedural justice (Aquino, Tripp, and Bies 2006; Colquitt, Noe, and Jackson 2002; Naumann and Bennett 2000). It is also harmonious with the more informal manifestations of voice that are of increasing interest to organizational justice researchers (Detert and Trevino 2010; Klaas, Olson-Buchanan, and Ward 2012) and that interviews revealed were at play in the workplaces under study. So, procedural justice measures employee comfort initiating claims as well as the perception that the entity (e.g., Gilda’s as a whole, the HR department), person (e.g., a local supervisor or manager), or process addressing the claim, be it informal (i.e., step 1) or formal (i.e., steps 2-4) (Tyler and Blader 2000), does so in a neutral, effective, and fair manner. Therefore, the survey questions also align with descriptions of access and problem solving viability described in several studies

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<sup>4</sup> Factor analysis, for example, relies upon list-wise deletion, which in this case, yields an empty dataset.

<sup>5</sup> That is, the summative score is divided by the number of items for which the sum was calculated. For example, if only five of eight items had non-missing values, then the five items would be summed and divided by five. This is consistent with the way index values get imputed from the usual construction of a summative index using Cronbach’s  $\alpha$  if one or more items underlying the index are missing for a particular observation.

outlining critical elements by which to gauge the effectiveness with which disputes are addressed in a workplace with or without a DRS in place (Folger and Bies 1989; Sheppard, Lewicki, and Minton 1992; Tyler and Bies 1990).

Recall that interactional justice is distinct from procedural justice in that interactional justice explicitly excludes voice, even one's expression of her opinion to her immediate supervisor. Rather, it captures the extent to which store managers and supervisors treat employees with respect and sensitivity. So, it reflects the degree to which workers feel well-treated, not by "the company" per se, but by managers and direct supervisors with whom they deal on a personal level. In other words, in the course of everyday managerial decision-making, are one's needs being taken into account (Aquino, Lewis, and Bradfield 1999; Bies 2001; Bies and Moag 1986)? It is constructed from four survey items that align closely with validated scales used by Moorman (1991) 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 regarded as following the law, particularly with respect to laws prohibiting discrimination and harassment. It is an often overlooked, but critical component of workplace governance (Balsler 2000; Wallace, Edwards, Mondore, and Finch 2008). 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 literature frequently relies on turnover as a measure of organizational commitment (Byrne 2005; Spencer 1986). However, as explained above with respect to other behavioral measures, both practicality and the study context dictated that we rely on perceptual measures of organizational commitment. Indeed, the six survey questions making up this construct align well with commonly used validated scales (Cook and Wall 1980; Mowday, Steers, and Porter 1979).

[—INSERT TABLE 1 ABOUT HERE.—]

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, 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.

[—INSERT TABLE 2 ABOUT HERE.—]

In the aggregate, while cognizant of the challenges as well as the opportunities presented to us by these data, 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.

### **Empirical Strategy**

Gilda’s DRS went into effect for all employees at all locations right in the middle of the observation period—January 1, 2004. 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 for the effects of implementing the DRS caused by contemporaneous exogenous shocks, we have no reason to suspect that this is occurring. As noted above, our use of store-level data to analyze the effects of implementing the DRS avails us of fixed-effects (FE) models to account for time-constant unobservables at the store level. We detail these methods below.

As a first pass, we will simply compare the mean values for our four 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 FE 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,

$$(0) \quad y_{it} = \sum_{k=1}^{635} d_k \alpha_k + \sum_{T=2001}^{2007} d_T \alpha_T + \beta_{it} x_{it} + \varepsilon_{it} \quad ,$$

where  $y$  represents any one of the four 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,  $d_k$ , multiplied by a store-specific intercept,  $\alpha_k$ . Since there is no shared intercept term (akin to  $\alpha_0$ ), 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 (0) can instead be written as

$$(0) \quad y_{it} = \alpha_i + \alpha_t + \beta_{it}x_{it} + \varepsilon_{it} \quad ,$$

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  $\hat{\beta}_{it}$ , which measures the impact of the DRS on the dependent variable, net of unobserved sources of store-specific and year-specific variation. To further err in a 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).<sup>6</sup>

## Results

Table 3 shows descriptive statistics and correlations for the focal independent variable as well as for the four employment relations measures. Note that about 56% of the observations are post-DRS. Interestingly, both procedural justice and organizational commitment reveal a negative, zero-order association with the DRS, while interactional justice and legal compliance 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 3.49 to 3.88, all are somewhere between the neutral response (“neither agree nor disagree”) and agreement.

[—INSERT TABLE 3 ABOUT HERE.—]

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-

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<sup>6</sup> 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.

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, violating the independence assumption that underpins a standard  $t$ -test. 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.<sup>7</sup>

[—INSERT TABLE 4 ABOUT HERE.—]

Table 4 presents these simple differences-in-differences for each of the dependent variables. First focusing on procedural justice, note that its mean value across all stores in the pre-DRS period was 3.59 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 procedural justice is 3.37. This represents a .21 decrease in this dependent variable. It accounts for the non-independence of observations by stores, making the test-of-significance more conservative. Even so, the .21 reduction in procedural justice is highly statistically significant ( $p < .001$ ). The results for the other three 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 interactional justice and legislative compliance. On the other hand, mean scores for organizational commitment fall very slightly, by .06, also statistically significant at the  $p < .001$  level.

The results reported in 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

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<sup>7</sup> 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).

variable that can be accounted for by time-constant, unobserved attributes of each individual store.

[—INSERT TABLE 5 ABOUT HERE.—]

The results in Table 5 may 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 procedural justice once again as an illustration, on average, within a given store, perceptions of procedural justice fell by .22 between the pre-DRS and post-DRS periods. This difference is about the same as the difference-in-means when unobserved differences between stores are not accounted for (.21). 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 that 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. The one partial exception, however, is organizational commitment. While the before-and-after difference remains statistically significant, it loses precision in Table 5 relative to Table 4, suggesting that some unobserved characteristic at the store-level is at least partially responsible for the apparent correlation between organizational commitment and the DSR. Nonetheless, relative to the pre-DRS period, within the same stores, perceptions of procedural justice and measures of organizational commitment decreased while perceptions of interactional justice and legislative compliance both 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 (0) 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 .30 point decrease in the five-point measure of

procedural justice. Not only is this slightly larger than the effect estimated in the absence of year dummies, the estimate achieves the same level of statistical significance ( $p < .001$ ). The model explains 37 percent of the within-store variance in procedural justice across the 492 stores that were able to provide data for this index, and thus, that were included in the estimate. Each store lent, on average, 1.9 observations ( $942 \div 492$ ) to the estimate of Model 1. Finally,  $\rho$  is the share of the estimated variance of the overall error,  $\varepsilon_{it}$ , accounted for by effects at the store level, a measure of intraclass correlation. In this case, 59 percent of the variability in 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 three models can be interpreted similarly. The implementation of the DRS is associated with increased perceptions of interactional justice and of legal compliance. Interestingly, in the case of 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 4 attempts to predict organizational commitment using the exact same model and right-hand side variables as the other three 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 4 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 three 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 procedural justice while boosting perceived levels of interactional 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

By offering evidence of the effects of implementing a typical workplace DRS with both formal and informal components, this paper presents a case that calls into question the extant model connecting DRS implementation, procedural justice, and organizational commitment. Implementation of Gilda's DRS is associated with improved perceived interactional justice, improved perceived legal compliance, reduced perceived procedural justice, and no effect on organizational commitment. Limitations of this natural experiment notwithstanding, the evidence in this paper suggests that implementation of Gilda's DRS improved frontline employees' perceptions of interactional justice. Perhaps the trade-off for this benefit of implementing a DRS such as Gilda's is the decrease in procedural justice found in this study. These findings lend support for parts of the model depicted in Figure 3.

As mentioned above, our findings might reflect 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, consistent with received case study research of nonunion systems implemented elsewhere (Colvin 2004). 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 employees' perceptions that the Company is in compliance with the law. This might seem counterintuitive 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. As discussed above, this is consistent with recent research (Eigen 2008; Feldman and Tyler 2012). Why would employees regard the Company as more compliant with the law if it is less fair in resolving disputes? One possible explanation is that being made to sign a form-contract requiring acquiescence to final, binding, mandatory arbitration makes the Company's intent to be "legally compliant" generally more salient to them. 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, and is consistent with the approach suggested by Tyler and Blader (2000). Future research should continue to explore how bifurcated processes consisting of both formal processes invoking legal rights adjudication like mandatory arbitration *and* informal procedures like reporting complaints to local managers invoke bifurcated organizational justice findings like those described in this study.

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 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 interactional justice or the negative effects of procedural justice. Further research should explore which accounts for the observed effects.

The Gilda's case 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. Specifically, it may make sense for researchers to more carefully discern informal from formal processes because the latter are more closely associated with the corporate entity and the former with individual local managers responsible for administering DRSs and handling disputes at their earliest stages. Our findings also lend indirect support for 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 the non-disputant population. Lastly, this study 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, Billings, Eveleth, and Gilbert 1996). As illustrated by the full version of the differential-effects model shown in Figure 3, it could be that there is a trade-off between formal and informal procedural justice similar to the observed trade-off between interactional and 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 case 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 2011; 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 analyses 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 study 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 rely 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 from 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.<sup>8</sup> Indeed, when we do, 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

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<sup>8</sup> The survey asks employees to self-report how many years they have been Gilda's employees.

information that made the research setting so appealing despite the aforementioned challenges that it creates for generalizability.

## Conclusion

This paper exploited unique, longitudinal data from a single company, allowing the authors to evaluate employee perceptions of organizational 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 from the case suggest that implementing a DRS has the theorized differential effects on organizational justice. In the data analyzed, the effects of DRS implementation are decreased procedural justice and increased interactional justice and increased perceived legal compliance. Implementation of Gilda's DRS has no statistically significant effect on organizational commitment. These 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 perceived procedural justice, with no net effect on organizational commitment. Perhaps the corresponding increase in interactional justice associated with the DRS's implementation is linked to employee commitment, but to local supervisors—what could be described as “interpersonal commitment”—instead of to the organization.

To explain these findings, we offer the differential effects model linking employment dispute resolution with organizational justice, organizational commitment, interpersonal commitment, and perceived legal compliance as a means of sparking conversations about the relationship among these important constructs. We hope to cast some doubt on the unidirectional, standard model of DRS implementation. As noted above, we do not set out to fully test the differential effects model we propose. It is offered to augment our understanding of the relationship among these constructs built on decades of research, and to push future research in more productive directions. 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 perceptions 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 organizational commitment and procedural justice. These and other questions emerging from this study deserve continued attention given the vast number of organizations contemplating implementation of a DRS, often without knowledge of the trade-offs this study uncovers.

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TABLES

**Table 1. Variable Definitions and Construction**

<b>Variable</b>	<b>Definition in italics, with actual survey questions underneath</b>
Dispute Resolution System (DRS)	<i>Is the DRS in place? (binary variable in which 0 = "no" and 1 = "yes")</i>
Procedural Justice	<i>Employees have access to formal mechanisms for appealing adverse employment actions and are afforded voice in their dealings with the company and its agents.</i>
	If I have a problem, there is someone in senior management I can go to who will assist me.
	This location has an effective means of appealing discipline and discharge actions.
	If I can't get a problem resolved in my store, HR continues to be accessible to me and provides guidance.
	I believe that this company is committed to resolving associate concerns or problems quickly and in a fair manner.
	My direct supervisor is interested in my opinion on work-related issues.
	My general manager listens to employees' concerns, issues, and suggestions.
	My manager is interested in my opinion on work-related issues.
Interactional Justice	<i>Local store managers and supervisors treat employees with respect and sensitivity.</i>
	My manager regularly shows me that he/she cares about me.
	My direct supervisor treats me with courtesy and respect.
	My direct supervisor regularly shows me that he/she cares about me.
	Once my schedule is posted, my manager only makes changes that I agree to.
Legal Compliance	<i>The workplace is in compliance with the law, particularly laws against harassment and discrimination.</i>
	I feel my work environment is free from harassment and discrimination.
	My supervisor manages everyone in my group equally regardless of their race, age, or sex.
	My direct supervisor manages everyone in my group equally regardless of their race, age, or sex.
	I think the company is doing a great job of providing a workplace free from intimidation, threats, and other behaviors that could impact my safety.
	Racial, ethnic, and gender-based comments/jokes are not tolerated at this location.
	I feel I am treated fairly and with respect regardless of my race, gender, or age.
I feel my work environment is free from discrimination based upon gender, race, age, or sex(ual orientation).	
Organizational Commitment	<i>Employees feel committed to the organization.</i>
	Work like mine greatly encourages me to do my best.
	The supervision I receive is the kind that greatly encourages me to give extra effort.
	I expect to be working here one year from now.
	My job is considered important in this company.
	I believe in what we are doing in this store.
	I feel positive about the opportunity here to advance my career.

*Note:* With the exception of the DRS dummy, all variables are continuous and on a Likert-type scale such that 1 = "strongly disagree" and 5 = "strongly agree."

**Table 2. Observation Counts for Each Dependent Variable, by Year**

Variable	2000		2001		2002		2003		2004		2005		2006		2007	
	stores	workers														
Procedural Justice	93	7,932	98	6,746	129	9,901	132	9,602	91	6,309	115	8,648	141	9,988	260	16,593
Interactional Justice	91	7,244	98	6,642	133	10,033	133	9,537	100	6,850	129	9,734	223	15,758	260	16,544
Legal Compliance	93	7,928	100	6,793	133	10,124	133	9,650	100	6,946	129	9,749	223	15,802	229	14,382
Organizational Commitment	93	7,938	100	6,800	133	10,129	133	9,656	100	6,951	129	9,780	223	15,830	260	16,610

*Notes:* 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**

#	Variable	Mean	SD	1	2	3	4	5
1	Dispute Resolution System	.56	.50	1.00				
2	Procedural Justice	3.49	.34	-.23	1.00			
3	Interactional Justice	3.88	.32	.37	.27	1.00		
4	Legal Compliance	3.80	.32	.24	.51	.60	1.00	
5	Organizational Commitment	3.63	.25	-.14	.73	.49	.43	1.00

*Notes:* Sample sizes differ by cell, but range from  $n = 1,028$  to  $n = 1,171$  store-years.  
*Key:* All correlation coefficients are significant at  $p < .0001$ .

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

Dependent Variable	Pre-DRS	Post-DRS	Difference	z-statistic
Procedural Justice	3.59	3.37	-.21	-12.22***
Interactional Justice	3.67	3.98	.31	19.37***
Legislative Compliance	3.64	3.84	.20	12.60***
Organizational Commitment	3.65	3.59	-.06	-4.81***

*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**

<b>Dependent Variable</b>	<b>Pre-DRS</b>	<b>Post-DRS</b>	<b>Difference</b>	<b><i>t</i>-statistic</b>
Procedural Justice	3.59	3.37	-.22	-10.69 <sup>***</sup>
Interactional Justice	3.66	3.98	.32	17.15 <sup>***</sup>
Legislative Compliance	3.66	3.83	.17	10.79 <sup>***</sup>
Organizational Commitment	3.63	3.61	-.03	-2.20 <sup>*</sup>

*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**

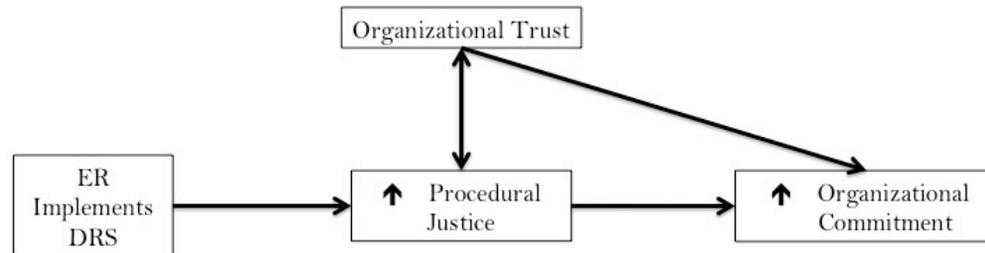
	Model 1:	Model 2:	Model 3:	Model 4:
Independent Variable	Procedural Justice	Interactional Justice	Legal Compliance	Organizational Commitment
Dispute Resolution System	-.30*** (-6.13)	.33*** (9.11)	.36*** (9.60)	.001 (0.03)
<i>n</i> store years	942	1,049	1,032	1,053
<i>n</i> distinct stores	492	527	527	528
$\rho$	.59	.54	.63	.54
$R^2$	.37	.51	.25	.03

*Notes:* 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  $\rho$  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.

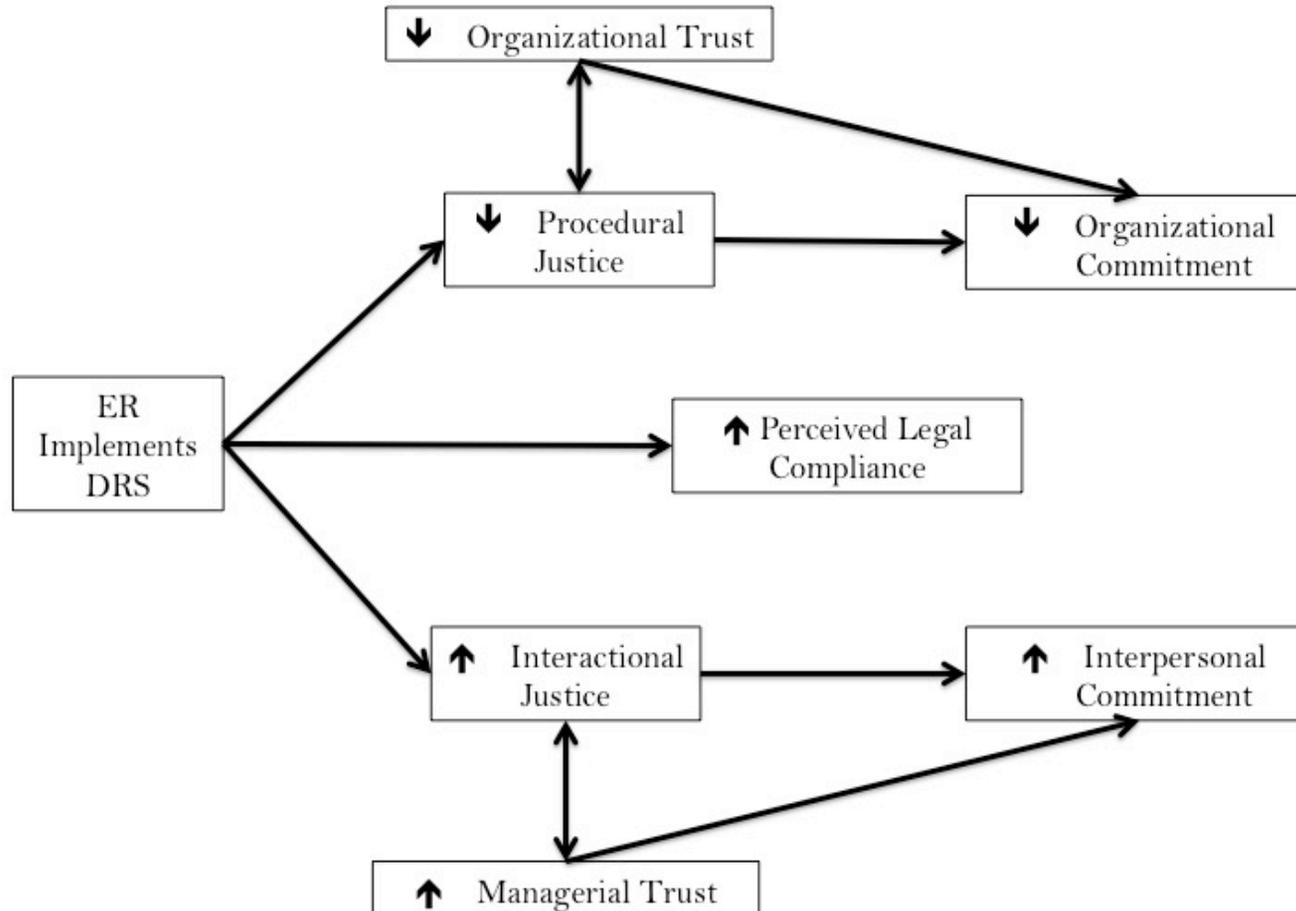
*Key:* \*  $p < .05$ , \*\*  $p < .01$ , \*\*\*  $p < .001$ .

FIGURES

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



*Figure 2. Differential Effects Model Connecting DRS Implementation, Organizational Justice, Legal Compliance, and Organizational Commitment*



**Figure 3. Observed Means of Dependent Variables by Year (Individual Level Data)**

