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RESEARCH REPORT

Work Gets Unfair for the Depressed: Cross-Lagged Relations Between Organizational Justice Perceptions and Depressive Symptoms

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The organizational justice literature has consistently documented substantial correlations between organizational justice and employee depression. Existing theoretical literature suggests this relationship occurs because perceptions of organizational (in)justice lead to subsequent psychological health problems. Building on recent research on the affective nature of justice perceptions, in the present research we broaden this perspective by arguing there are also theoretical arguments for a reverse effect whereby psychological health problems influence perceptions of organizational justice. To contrast both theoretical perspectives, we test longitudinal lagged effects between organizational justice perceptions (i.e., distributive justice, interactional justice, interpersonal justice, informational justice, and procedural justice) and employee depressive symptoms using structural equation modeling. Analyses of 3 samples from different military contexts (N_1 = 625, N_2 = 134, N_3 = 550) revealed evidence of depressive symptoms leading to subsequent organizational justice perceptions. In contrast, the opposite effects of organizational justice perceptions on depressive symptoms were not significant for any of the justice dimensions. The findings have broad implications for theoretical perspectives on psychological health and organizational justice perceptions.

Keywords: organizational justice, depressive symptoms, cross-lagged panel design, occupational health, longitudinal

Organizational justice refers to employees' perceptions of fairness in organizations (Colquitt, Greenberg, & Zapata-Phelan, 2005). Researchers typically distinguish among three dimensions of organizational justice (Colquitt, Conlon, Wesson, Porter, & Ng, 2001). Distributive justice refers to perceptions of the fairness of decision outcomes and resource allocation. Procedural justice refers to perceptions of fairness regarding the processes leading to decision outcomes. Finally, interactional justice refers to interpersonal treatment when incorporating new procedures (Bies, 2005). This latter justice dimension can additionally be divided into interpersonal justice (i.e., employees' perceptions of fair and respectful treatment by a supervisor or authority) and informational justice (i.e., perceptions of how resource allocations are explained by a supervisor or other authority).

Past research has documented substantial correlations between organizational justice dimensions and employee psychological health, typically using depressive symptoms as a main psychological health outcome measure (e.g., Elovainio, Kivimäki, & Helkama, 2001; Tepper, 2001). Researchers have commonly interpreted the correlation between organizational justice perceptions and employee psychological health (i.e., depression) as evidence that unfair treatment in organizations is a stressor that ultimately leads to reduced psychological health (Vermunt & Steensma, 2005). Even though this interpretation is plausible, the correlational nature of available field studies does not allow confirmation of the proposed causal direction; moreover, research supporting causal inferences is rare. The goal of the present research is to provide both a theoretical and an empirical examination of the causal relationship between organizational justice perceptions and employee depressive symptoms. Specifically, we

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extend the theoretical perspective of previous work by arguing there are plausible arguments for expecting that an employee's organizational justice perceptions are influenced by his or her psychological health. Fundamentally, we propose that the relationship between justice and psychological health may be bidirectional. Empirically, we contribute to the literature by using longitudinal cross-lagged panel designs to examine causal directions and to test alternative theoretical propositions. We start by recapitulating theoretical ideas supporting the presumed causal link between organizational justice perception and psychological health. We then proceed with elaborating theory in support of a causal effect of psychological health on organizational justice perceptions.

Organizational Justice → Psychological Health

Research on justice in general (e.g., Scott, Colquitt, & Zapata-Phelan, 2007), and research on the interrelation between organizational justice perceptions and psychological health, in particular, has primarily treated organizational justice perceptions as an independent variable that influences psychological health. In so doing, researchers have theoretically relied on Lazarus and Folkman's (1984) cognitive appraisal model of stress and coping (e.g., Xie, Schaubroeck, & Lam, 2008). Lazarus and Folkman's model refers to work experiences that have detrimental effects on well-being as stressors. Whether an event is regarded as a stressor is determined by two consecutive appraisal processes (i.e., primary and secondary appraisal). In the primary appraisal process, the encountered situation or event is cognitively evaluated for its potential for harm or loss. If individuals perceive the situation as threatening, a secondary appraisal process is initiated, centering on whether one has enough resources to meet the situational demands to prevent threat of harm or loss. If individuals perceive they do not have the required resources to prevent harm or loss to their well-being, Lazarus and Folkman's model proposes that individuals experience stress. Stress over an extended period of time manifests itself through psychological symptoms such as exhaustion, anxiety, or depression (Tepper, 2001).

A precondition for linking Lazarus and Folkman's (1984) model with the organizational justice literature is that organizational justice perceptions qualify as stressors. That is, justice perceptions need to be evaluated as potentially threatening in the primary appraisal process. There are two arguments for assuming that this is the case.

The first argument is that there is conceptual overlap between organizational justice perceptions and specific stressors studied in classic studies on occupational stress. Specifically, three of the four justice dimensions directly relate to stressors studied in previous literature. Procedural justice bears similarities with research examining lack of autonomy as a stressor (Chesney et al., 1981). Interpersonal justice can be related to research examining interpersonal conflict at work (Spector & Jex, 1998) and supervisory misbehavior (Kohli, 1985), whereas informational justice can be linked to research examining lack of necessary information as a stressor (Spector & Jex, 1998). In general, Judge and Colquitt (2004) noted that "justice has the ability to reduce the uncertainty and lack of control that are at the heart of feelings of stress" (p. 396).

The second argument is that organizational justice theories implicitly include stress mechanisms. For instance, referent cognitions theory argues that procedural justice perceptions influence the behavior of employees by causing reactions such as distress (Folger, 1993). Equity theory suggests that individuals evaluate distributive justice by comparing their perceived ratio of inputs to outcomes with those of others. When individuals perceive receiving fewer resources than deserved, equity theory proposes that the imbalance produces distress and motivation to change the situation (Mowday & Colwell, 2003; Walster, Berscheid, & Walster, 1973). Finally, a core tenet of theoretical work on interactional justice is that this form of justice has a particularly strong link to feelings of injustice. Presumably injustice is strongly related to anticipated threat (Bies, 2005) and is thereby similar to what Lazarus and Folkman (1984) have considered to lie at the heart of the primary appraisal processes.

Evidence for the theoretical proposition that justice perceptions impact well-being comes from several sources. The first source stems from experimental procedural justice research. Vermunt and Steensma (2003) conducted a study examining the effects of manipulating justice perceptions on proximal stress measures in laboratory settings. In this study, students performed a complex arithmetic task and were then either treated fairly (got the opportunity to give arguments defending their performance) or unfairly (no opportunity). Results revealed that participants provided with no opportunity to defend their performance had higher stress-related physiological responses than participants in the fair condition.

A second source of evidence comes from a seminal experimental and longitudinal field study conducted by Greenberg (2006). Greenberg investigated the effects of a pay cut (a change in distributive justice) and an interpersonal justice training intervention on the self-rated insomnia of nurses. He found that both the pay cut and training intervention impacted self-rated insomnia.

Finally, a third source of evidence regarding the causal relation between organizational justice perceptions and employee health is a half-lagged longitudinal study of college teachers conducted by Judge and Colquitt (2004). Judge and Colquitt found significant effects of procedural and interpersonal justice on ratings of experienced stress 6 months later. In contrast, informational and distributive justice were not significantly related to future ratings of experienced stress. Although Judge and Colquitt measured experienced stress and not psychological health, their study is nevertheless relevant because it provides evidence that justice perceptions can function as a stressor. One limitation with the study, however, is that the authors did not control for stress at Time 1, thereby diminishing the ability to make causal inferences (Finkel, 1995; Zapf, Dormann, & Frese, 1996).

In sum, the reviewed literature provides both theoretical and empirical evidence that organizational justice perceptions can influence psychological health of employees in organizations. There is, nonetheless, a clear need to further investigate this relationship. In so doing, it may be important to also consider that the effect may be bidirectional.

Psychological Health → Organizational Justice

The following section considers evidence of a causal pathway from psychological health to organizational justice perceptions.

Although Vermut and Steensma (2005) have acknowledged the theoretical possibility of psychological health impacting organizational justice perceptions, a link of this nature has not previously been elaborated in the literature. We propose there are two theoretical arguments that support a directional link of this type.

The Affective Perception Assumption

The first argument suggesting that psychological health leads to perceptions of organizational justice builds on the perspective introduced by Barsky and Kaplan (2007). This perspective, which Barsky and Kaplan labeled the "hot view of organizational justice," considers injustice to be an emotionally laden and subjective experience. According to Barsky and Kaplan, the hot view differs from the prevailing "cold view of organizational justice," which assumes that perceptions of organizational justice are "a cold cognitive response to decision outcomes and specific human resource practices" (p. 286). A core idea underlying the cold view is that justice perceptions reported by employees reflect objective instances of unfair treatment.

Barsky and Kaplan (2007) have challenged the idea that justice perceptions are an individual's cognitive response to environmental events by proposing that justice perceptions are, to a notable degree, influenced by affective states. Evidence from two sources supports this position. First, Barsky and Kaplan reported a meta-analysis on the association among affective states, trait measures, and justice perceptions. This meta-analysis revealed that procedural, distributive, and interactional justice were all associated with state and trait measures of positive and negative affect. The second source of evidence supporting the hot view is an experimental study conducted by van den Bos (2003). This study demonstrated that the induction of a positive or negative mood state affected subsequent judgments regarding justice thereby supporting a causal relationship between affective states and perceptions of justice.

The notion that affective states influence organizational justice perceptions is relevant for understanding causal relations between organizational justice perceptions and employee psychological health. Affect is generally considered a central component of psychological health constructs such as well-being and depression (e.g., Clark, Watson, & Mineka, 1994; Lonigan, Phillips, & Hooe, 2003). Thus, the research by van den Bos (2003) opens the possibility that the affective component underlying psychological health influences perceptions of organizational justice.

This idea is not only consistent with the organizational justice research conducted by Barsky and Kaplan (2007) and van den Bos (2003), it is also consistent with social cognitive research on the lack of self-serving bias in depressed individuals. Through both experimental and nonexperimental studies, social cognitive research has consistently demonstrated that depressed individuals perceive their environment as more threatening and risky than nondepressed individuals (e.g., Alloy & Abramson, 1979; Sacco, 1985). Some longitudinal research also suggests these differences occur because depressed individuals are more accurate and realistic than nondepressed individuals in assessing their environmental surroundings. Specifically, nondepressed individuals are able to buffer stress experiences by generating self-serving biases and evaluating their environment with a considerable amount of positive illusion (e.g., Folkman & Lazarus, 1986; Lewinsohn, Mischel, Chaplin, & Barton, 1980).

The proposition that psychological health influences perceptions of organizational justice is congruent with the aforementioned cognitive appraisal model (Lazarus & Folkman, 1984) when one notes that the same event may be appraised differently by depressed and nondepressed individuals. Folkman and Lazarus (1986) acknowledged this possibility and conducted a key study suggesting that depressive individuals perceive their environments as being particularly threatening and risky. Folkman and Larzarus interpreted these findings as evidence that depressed individuals differ from nondepressed individuals in their primary appraisal of events within the framework of the cognitive appraisal model.

The Affective Reaction Assumption

The second set of arguments suggesting psychological health influences perceptions of organizational justice directly builds upon the affective *perception* assumptions proposed in the first set of arguments. Specifically, the affective *reaction* assumption proposes that well-being causally precedes perceptions of organizational justice because employees with reduced psychological health (i.e., depressed employees) express their elevated levels of negative affect to other organizational members such as coworkers and supervisors. In fact, Folkman and Lazarus (1986) found that depressed individuals felt more negative emotions such as anger, frustration, or disappointment and, respectively, tended to also report more hostile "confrontative" coping (i.e., secondary appraisal; Folkman & Lazarus, 1986, p. 111), including the expression of anger. Similarly, another study reported that depressed individuals made stronger use of emotion-focused coping strategies, including verbal and behavioral anger expressions (Billings & Moos, 1984).

The finding that depressed individuals show elevated threat and hostility is not sufficient to assume a causal mechanism whereby depression leads to perceptions of (in)justice. However, it is reasonable to assume that other organizational members react to the negative affect exhibited by those with depression. For instance, Coyne (1976) found in an experimental study that individuals avoided depressed individuals because depressed individuals elicited aversive feelings after a phone conversation. In a work setting, organizational members may perceive the depressed individual in ways that ultimately limit organizational benefits. Specifically, supervisors might avoid depressed employees. In turn, the avoidant behavior of the supervisor might foster the perception of unfairness. This effect might be more salient in the interactional justice dimensions (i.e., interpersonal and informational justice) and less salient for distributive or procedural justice depending on the organizational level of the respective procedures and allocations of resources.

As with the affective perception assumption, it is important to note that the affective reaction assumption is also consistent with theoretical assumptions made within the cognitive appraisal model (Lazarus & Folkman, 1984). The existence of reciprocal causation between the individual's perception and the reaction from the environment is a key characteristic of the model and is referred to as the model's process orientation. Specifically, it is the idea that "the person and the environment are in a dynamic relationship that is constantly changing and . . . that this relationship is bidirectional, with the person and the environment each acting on the other" (Folkman, 1984, p. 840).

In summary, we propose two theoretical arguments—the affective *perception* assumption and the affective *reaction* assumption—suggesting that psychological health may influence subsequent perceptions of organizational justice. Whereas the affective perception assumption proposes that depressed individuals perceive events more negatively and is a necessary condition for suggesting an effect of psychological health on justice, the affective reaction assumption draws upon the affective perception assumption and complements it with the idea that depressed individuals may actually be treated differently in reaction to their depressed state.

The Present Research

We used three longitudinal data sets from applied field settings to test (a) whether each of the organizational justice perception dimensions influences depressive symptoms over time and (b) whether depressive symptoms have a lagged relation with perceptions of organizational justice. Methodologically, the causal comparisons were conducted using cross-lagged panel longitudinal designs that allow one to directly contrast the two possible causal directions between a pair of variables (Finkel, 1995; Zapf et al., 1996). Even though cross-lagged panel designs have methodological shortcomings (see Finkel, 1995, for an overview) and are less able to establish causality than experimental designs, cross-lagged panel designs are typically considered the optimal way to understand causality in field settings where experimental procedures are not feasible. In the present study, the application of this design was particularly straightforward because theory as well as research using other methodology had already established evidence for the advanced hypotheses.

> *Hypothesis 1:* There is a time-lagged effect of (a) procedural, (b) distributive, and (c) interactional justice perception dimensions on depressive symptoms.

> *Hypothesis 2:* There is a time-lagged effect of depressive symptoms on (a) procedural, (b) distributive, and (c) interactional justice perception dimensions.

Method

Participants and Procedure

Data from the first sample were used to assess the lagged effects of distributive and interactional justice perceptions on depressive symptoms and vice versa. Participants consisted of 625 active duty soldiers deployed on a peacekeeping mission. Soldiers were first surveyed in garrison 30 days prior to their deployment (Time 1 [T1]). The second data collection occurred toward the end of their 6-month peacekeeping mission before returning to garrison (Time 2 [T2]).

Data from the second sample were used to further replicate the lagged effects of distributive and interactional justice perceptions on depressive symptoms and vice versa. In this sample, lagged effects of perceptions of procedural justice were also available at both time points. Participants consisted of National Guard and Army Reserve soldiers (N = 134) who were activated following the terrorist attacks of September 11, 2001. Their task was to augment security on military installations around the Washington, D.C., area. Soldiers were first surveyed 3 months after their initial activation (T1). The second data collection (T2) occurred 3 months later.

Data from the third sample were used to assess the lagged effect of procedural justice on depressive symptoms. The sample consisted of reservists (N = 550) on a 6-month security augmentation deployment to U.S. Military installations in Europe. Soldiers were asked procedural justice items about the initial activation process at T1 prior to deploying; however, the procedural justice items were not reassessed at T2 following the deployment. Thus, data from this third sample only allowed for analyses using an incomplete cross-lagged design (Zapf et al., 1996). Even though such designs are less rigorous than fully lagged designs, this sample is nevertheless suitable to reexamine the effect of procedural justice on depressive symptoms.

Table 1 provides an overview of the three samples. The table provides sample and design characteristics as well as details on the employed organizational justice and depression measures used in all three samples.

Analytical Strategy and Statistical Analyses

In line with recommendations in the literature (Finkel, 1995; Little, Preacher, Selig, & Card, 2007; Williams & Podsakoff, 1989), we used structural equation modeling (SEM) techniques to analyze our cross-lagged panel design. SEM analyses were conducted in two steps. In the first step, we examined whether measurement invariance across time existed for each latent variable (depressive symptoms and each of the organizational justice dimensions) as a prerequisite to examine subsequent models (cf. Finkel, 1995; Meade & Kroustalis, 2006; Vandenberg & Lance, 2000). In the second step, we specified and analyzed structural models. We first examined two-variable models incorporating depressive symptoms and one of each of the justice dimensions to determine the specific effects of each justice dimension on depressive symptoms. For Samples 1 and 2, we also (a) examined the viability of alternative third variable explanations by contrasting the cross-lagged models with third variable explanation models and (b) tested overall models including several justice dimensions to determine whether effects were unique in the sense that they persisted even when the other justice dimensions were controlled.

All SEM analyses were conducted using LISREL 8.72 (Jöreskog & Sörbom, 2005). Because of recent evidence that depressive symptoms are continuous but not normally distributed in the population (e.g., Hankin, Fraley, Lahey, & Waldman, 2005; Huang, Chung, Kroenke, Delucchi, & Spitzer, 2006; Prisciandaro & Roberts, 2005), we used diagonally weighted least square estimation (Jöreskog & Sörbom, 2005) and the Satorra–Bentler chi-square correction (SB-χ^2; Satorra & Bentler, 2001). These techniques are most suited to analyze data from ordinal response variables and are robust to deviations from nonnormality (Bandalos, 2008; Flora & Curran, 2004; Wirth & Edwards, 2007).

For overall model evaluations, we relied on the comparative fit index (CFI), the Tucker–Lewis index (TLI), and the root-mean-square error of approximation (RMSEA) because these three fit indices (a) are frequently reported (Coovert & Craiger, 2000), (b)

Table 1
Sample Characteristics, Design Characteristics, and Measures for the Three Studies

Variable	Sample 1	Sample 2	Sample 3
Sample characteristics			
N	625	134	550
Type of soldiers	Active duty	National Guard and reservists	Reservists
Age (years)			
M	25.79	32.27	31.35
SD	5.59	9.82	9.09
Male (%)	97	90	94
Study design			
Work task	International peacekeeping mission	Security augmentation at U.S. military installations around the Washington, D.C., area	Security augmentation at military installations in Europe
Time lag between Time 1 and Time 2	6 months	3 months	6 months
Justice measures			
Source	Colquitt (2001)[a]	Colquitt (2001)[a]	Colquitt (2001)[a]
No. of items			
Distributive justice	4	4	
Interpersonal justice	4	4	
Informational justice	5	5	
Procedural justice		5[b]	5[b]
Scaling	5-point Likert	5-point Likert	5-point Likert
Depressive symptom measures			
Source	CES-D-SC	PHQ-9	PHQ-9
No. of items	8 (9)[c]	9	9
Scaling	1 = *rarely or none of the time*, 4 = *most or all of the time*	1 = *not at all*, 4 = *nearly every day*	1 = *not at all*, 4 = *nearly every day*

Note. CES-D-SC = Short version of the Center for Epidemiological Studies—Depression Scale (CES-D; Radloff, 1977) developed by Santor and Coyne (1997); PHQ-9 = Patient Health Questionnaire for Depression (Kroenke et al., 2001).
[a] Items were slightly adapted to fit the military context. [b] Two items were deleted from the original scale because these items were not applicable to the military setting. [c] For data analyses, we removed one item ("I enjoyed life") from the CES-D-SC scale because this item showed very high overlap with another item ("I was happy") from the scale (polychoric correlation at Time 1 = .88; Time 2 = .80). Removing the item was also in line with recommendations by several authors who recommended this strategy for building measurement models in structural equation modeling when items are redundant (Aluja et al., 2003; Anderson & Gerbing, 1988; Byrne, 1991, 1993). Removing one of the two items lead to improved model fit for the scale in confirmatory factor analyses: without item "I enjoyed life", see Table 4; with both items, Satorra–Bentler-$\chi^2(125) = 930.191$, $p < .001$, comparative fit index = .919; Tucker–Lewis index = .901, root-mean-square error of approximation = .102. The results for Sample 1 were identical regardless of which item was removed. We report analyses with the item "I enjoyed life" removed.

are found to perform adequately in simulation studies (Beauducel & Wittmann, 2005; L. Hu & Bentler, 1998; Yu, 2002), and (c) are based on χ^2 values and thus can be corrected using the SB-χ^2 correction. Cutoff values were based on recommendations from L. Hu and Bentler (1998) for CFI and TLI as well as from Vandenberg and Lance (2000) regarding RMSEA.

For model comparisons, we relied on model differences in fit indices. The Satorra–Bentler scaled χ^2-difference statistic can commonly be interpreted as an analog to an ordinary χ^2-difference test. However, when model differences are small, the Satorra–Bentler scaled χ^2-difference statistic occasionally becomes negative. In these instances, the statistic cannot be interpreted (Satorra & Bentler, 2001). Additionally, the significance of the χ^2-difference test depends heavily on sample size. Thus, the χ^2-difference test for decisions regarding measurement invariance confounds sample size and effect size. The use of differences in fit indices is generally a more robust and appropriate strategy than using the chi-square difference test (Chen, 2007; Cheung & Rensvold, 2002). Because data in the present study differed considerably in terms of sample size, we primarily relied on the differences in fit indices to evaluate measurement invariance. In so doing, we wanted to ensure that the data were treated consistently across the three samples and that we evaluated measurement invariance on the basis of an established and robust criterion.

Results

Descriptive Statistics

Tables 2 and 3 present intercorrelations, internal consistencies, means, standard deviations, skewness, and kurtosis for the study variables. As expected, the distributions of depression indicators provided evidence for some degree of nonnormality in the population.

Dimensionality of Justice in Samples 1 and 2

Before we examined measurement invariance and structural models, we first investigated the dimensionality of the justice perception measures in Samples 1 and 2. Results indicated that models treating interpersonal and informational justice as two separate factors provided a somewhat better fit to the data than models combining the two dimensions into an integrated interactional factor (see Table 4). However, the correlations between

Table 2
Descriptive Statistics and Intercorrelations for Sample 1

Variable	1	2	3	4	5	6	7	8	9	10
1. Distributive justice T1	—									
2. Interpersonal justice T1	.51	—								
3. Informational justice T1	.48	.81	—							
4. Interactional justice T1	.52	.94	.96	—						
5. Depressive symptoms T1	−.20	−.21	−.17	−.20	—					
6. Distributive justice T2	.31	.18	.18	.19	−.16	—				
7. Interpersonal justice T2	.22	.30	.32	.32	−.13	.42	—			
8. Informational justice T2	.21	.27	.36	.33	−.11	.38	.84	—		
9. Interactional justice T2	.22	.29	.36	.34	−.13	.42	.95	.97	—	
10. Depressive symptoms T2	−.12	−.14	−.09	−.12	.43	−.28	−.28	−.27	−.29	—
Cronbach's α	.91	.92	.93	.95	.84	.92	.93	.93	.96	.84
M	3.55	3.78	3.66	3.72	1.45	3.15	3.47	3.29	3.37	1.75
SD	0.76	0.81	0.79	0.76	0.47	0.94	1.04	1.01	0.98	0.61
Scale skewness	−0.53	−0.52	−0.42	−0.40	1.63	−0.32	−0.39	−0.30	−0.32	0.99
Item skewness										
M	−0.58	−0.67	−0.62	−0.64	1.87	−0.35	−0.49	−0.35	−0.44	1.07
Minimum	−0.63	−0.69	−0.66	−0.69	1.04	−0.42	−0.55	−0.40	−0.66	0.55
Maximum	−0.51	−0.63	−0.53	−0.53	2.71	−0.30	−0.37	−0.27	−0.25	1.47
Scale kurtosis-3	0.95	0.44	0.54	0.43	3.28	0.10	−0.32	−0.38	−0.31	0.34
Item kurtosis-3										
M	0.56	0.52	0.55	0.53	3.22	−0.12	−0.38	−0.49	−0.41	0.18
Minimum	0.44	0.26	0.41	0.26	0.04	−0.33	−0.06	−0.66	−0.55	−0.89
Maximum	0.71	0.64	0.75	0.75	7.44	0.21	−0.25	−0.29	−0.27	1.07

Note. $N = 625$. To ease interpretation, we subtracted 3 from all kurtosis values so that kurtosis-3 values and skewness values of 0 are indicative of a perfect normal curve. All intercorrelations are significant at $p < .05$ (one and two-sided tests). T1 = Time 1; T2 = Time 2.

interactional factors were high and all exceeded .70 (see Tables 2 and 3). Colquitt and Shaw (2005, p. 138) advised researchers to collapse organizational justice dimensions if they correlate higher than .70 even when confirmatory factor analyses favor models separating the dimensions. Given these findings, we opted to conduct two alternatives in subsequent analyses—one set of models treating informational and interpersonal justice as separate dimensions and one set of models collapsing the two dimensions.

Measurement Invariance

We examined two types of measurement invariance: configural (or form invariance) and metric invariance (or loading invariance). These types of measurement invariance are considered to be preconditions for adequately testing cross-lagged effects (Finkel, 1995; C. Hu & Cheung, 2008; Little, Preacher, et al., 2007; Steenkamp & Baumgartner, 1998).[1] Configural invariance indicates that items can be assigned to factors as theoretically suggested. Metric invariance indicates that the relation between the latent variable and the items is constant over time.

Table 5 provides tests of configural and metric invariance for all measures. Following recommendations in the literature (Finkel, 1995; Little, Preacher, et al., 2007; Williams & Podsakoff, 1989), the models allowed measurement errors for the same items to correlate over time. As indicated by Table 5, models specifying the same factor structure at both T1 and T2 provide a good fit to the data and provide evidence of configural invariance. Setting the loadings of all items equal over time to demonstrate metric invariance only marginally changed model fit. Specifically, for all measures, the difference between a model specifying metric invariance and a model allowing different item loadings at T1 and T2 was less than the cutoff values recommended by Cheung and Rensvold (2002; ΔCFI $\leq .01$) as well as Chen (2007; ΔCFI $\leq .005$ and ΔRMSEA $\geq .010$ for samples sizes smaller than 300, and ΔCFI $\leq .010$ and ΔRMSEA $\geq .015$ for $N > 300$). Together, these results provide compelling evidence of both configural and metric invariance for all measures across time.

Structural Models and Tests of the Hypotheses

We started by specifying bivariate cross-lagged panel models. These models combined each of the justice dimensions (procedural, distributive, interactional, informational, and interpersonal justice) with depressive symptoms to test the lagged effects of justice perception dimensions on depressive symptoms and vice versa. A bivariate cross-lagged panel model includes four different types of indicators: first, the correlation between the variables at

[1] There are other types of measurement invariance besides configural and metric invariance (see Vandenberg & Lance, 2000, for a thorough review). These types of invariance are either theoretically important or important as a precondition when one wants to test hypotheses on latent mean change (C. Hu & Cheung, 2008; Little, Preacher, et al., 2007; Steenkamp & Baumgartner, 1998; Vandenberg & Lance, 2000). In contrast to configural and metric invariance, however, these types of invariance are not relevant for testing relationships between variables or for ensuring that the meaning of constructs is identical over time as in cross-lagged panel models (C. Hu & Cheung, 2008; Little, Preacher, et al., 2007; Steenkamp & Baumgartner, 1998).

Table 3
Descriptive Statistics and Intercorrelations for Samples 2 and 3

Variable	1	2	3	4	5	6	7	8	9	10	11	12
1. Distributive justice T1	—											
2. Interpersonal justice T1	.30	—										
3. Informational justice T1	.31	.79	—									
4. Interactional justice T1	.32	.93	.96	—								
5. Procedural justice T1	.38	.28	.40	.37	—							
6. Depressive symptoms T1	−.30	−.25	−.27	−.28	−.34 (−.20)	—						
7. Distributive justice T2	.38	.08	.17	.14	.25	−.38	—					
8. Interpersonal justice T2	.10	.24	.23	.25	.12	−.23	.25	—				
9. Informational justice T2	.16	.28	.40	.37	.19	−.28	.31	.83	—			
10. Interactional justice T2	.14	.28	.34	.33	.17	−.27	.30	.94	.97	—		
11. Procedural justice T2	.23	.14	.27	.23	.45	−.31	.44	.29	.36	.35	—	
12. Depressive symptoms T2	−.16	−.17	−.18	−.18	−.28 (−.15)	.42 (.46)	−.38	−.12	−.21	−.18	−.28	—
Cronbach's α	.93	.92	.95	.95	.92 (.88)	.87 (.88)	.94	.95	.94	.96	.90	.92 (.92)
M	3.56	3.86	3.64	3.74	2.81 (3.19)	1.53 (1.28)	3.27	3.71	3.40	3.54	2.67	1.59 (1.30)
SD	0.98	0.87	0.96	0.87	0.91 (0.76)	0.57 (0.41)	1.00	0.94	0.99	0.93	0.86	0.70 (0.51)
Scale skewness	−0.52	−0.68	−0.73	−0.57	−0.38 (−0.31)	1.44 (2.17)	−0.63	−0.65	−0.55	0.06	−0.23	1.45 (2.52)
Item skewness												
M	−0.61	−0.86	−0.78	−0.82	−0.30 (−0.49)	2.34 (2.66)	−0.63	−0.86	−0.63	0.03	−0.12	1.69 (2.88)
Minimum	−0.85	−0.91	−1.00	−1.00	−0.39 (−0.60)	0.68 (1.33)	−0.77	−0.95	−0.73	−0.48	−0.16	0.80 (1.53)
Maximum	−0.45	−0.82	−0.66	−0.66	−0.22 (−0.39)	8.19 (6.04)	−0.52	−0.75	−0.54	0.63	−0.08	3.90 (5.85)
Scale kurtosis-3	−0.16	0.33	0.47	0.24	−0.34 (0.68)	2.05 (5.58)	0.06	0.15	−0.14	−0.58	−0.31	1.57 (7.38)
Item kurtosis-3												
M	−0.14	0.52	0.25	0.37	−0.62 (0.11)	10.39 (9.47)	−0.29	.39	−0.27	−0.74	−0.58	2.65 (9.97)
Minimum	−0.64	0.31	−0.07	−0.07	−0.77 (−0.13)	−0.66 (1.53)	−0.44	.16	−0.48	−0.95	−1.01	−0.64 (1.46)
Maximum	0.61	0.76	0.66	0.76	−0.47 (0.49)	73.70 (42.13)	−0.14	.63	0.13	−0.54	−0.03	14.71 (36.98)

Note. Values for Sample 3 are in Parentheses. $N = 134$ for Sample 2, and $N = 550$ for Sample 3. To ease interpretation, we subtracted 3 from all kurtosis values so that kurtosis-3 values and skewness values of 0 are indicative of a perfect normal curve. For Sample 2, at $|r| = .15$, intercorrelations are $p < .05$, one-sided, and at $|r| = .17$, intercorrelations are $p < .05$, two-sided. For Sample 3, at $|r| = .15$, intercorrelations are $p < .05$, two-sided. T1 = Time 1; T2 = Time 2.

Table 4
Dimensionality of Justice Perceptions in Samples 1 and 2

Time	SB-χ^2	CSB	*df*	*p*	CFI	TLI	RMSEA
			Sample 1				
Time 1							
DJ, IPJ, IFJ	67.15	5.91	62	.31	1.000	1.000	.012
DJ, IAJ	156.05	6.25	64	.00	.994	.993	.048
Model difference	34.63[a]		2	.00	.006	.007	−.036
Time 2							
DJ, IPJ, IFJ	114.86	4.05	62	.00	.997	.997	.037
DJ, IAJ	488.99	4.29	64	.00	.980	.975	.103
Model difference	140.37[a]		2	.00	.017	.022	−.066
			Sample 2				
Time 1							
DJ, IPJ, IFJ, PJ	135.98	4.58	129	.32	.999	.998	.020
DJ, IAJ, PJ	211.01	4.43	132	.00	.984	.982	.067
Model difference	—[b]		3	—[b]	.015	.016	−.047
Time 2							
DJ, IPJ, IFJ, PJ	146.23	3.22	129	.14	.997	.996	.032
DJ, IAJ, PJ	276.38	3.62	132	.00	.973	.969	.091
Model difference	25.69[a]		3	.00	.024	.027	−.059

Note. $N = 625$ for Sample 1, and $N = 134$ for Sample 2. SB-χ^2 = Satorra–Bentler chi-square correction; CSB = correction factor for the SB-χ^2 difference test; CFI = comparative fit index; TLI = Tucker–Lewis index; RMSEA = root-mean-square error of approximation; DJ = distributive justice; IPJ = interpersonal justice; IFJ = informational justice; IAJ = interactional justice; PJ = procedural justice.
[a] The value is a Satorra–Bentler scaled χ^2-difference statistic estimated using the following formula derived from Satorra and Bentler (2001): $\Delta\text{SB-}\chi^2 = (\text{SB-}\chi^2_{\text{Model 0}} \times \text{CSB}_{\text{Model 0}} - \text{SB-}\chi^2_{\text{Model 1}} \times \text{CSB}_{\text{Model 1}})/[(df_{\text{Model 0}} \times \text{CSB}_{\text{Model 0}} - df_{\text{Model 1}} \times \text{CSB}_{\text{Model 1}})/(df_{\text{Model 0}} - df_{\text{Model 1}})]$. [b] The value was negative. When the difference between models is small, it occasionally happens that the Satorra–Bentler scaled χ^2-difference statistic estimated using the procedures described in Satorra and Bentler (2001) becomes negative. In these instances, the statistic cannot be interpreted (Satorra & Bentler, 2001).

T1; second, the stability of each construct across time; third, the two cross-lagged effects; finally, the correlation between the disturbance terms of the two latent variables at T2. Figure 1 shows a cross-lagged structural equation model including all the described elements. Drawing inferences from a cross-lagged panel model primarily centers on interpreting the cross-lagged effects. However, the correlation between the error terms of the two latent variables at T2 is also of interest as it represents the shared variance between the constructs not accounted for by the stabilities and the lagged effects (Finkel, 1995).

Model fits for the bivariate cross-lagged panel models are provided in Table 6. As indicated by Table 6, all cross-lagged models provided a good fit to the data. We consequently proceeded by examining the cross-lagged effect estimates. As shown in Tables 7 and 8, there were significant lagged effects from depressive symptoms to all justice dimensions as suggested by Hypotheses 2a, 2b, and 2c. In contrast, Hypotheses 1a, 1b, and 1c were not supported, as there were no effects from any of the organizational justice perceptions to depressive symptoms.

Additional Analyses

The strength of inferences from nonexperimental research can be strengthened by ruling out potential alternative, or third variable, explanations. Zapf et al. (1996) noted that cross-lagged designs control for previous levels of a variable, thereby ruling out the possibility that constant background variables such as age or gender influence estimates of cross-lagged effects. The design, however, cannot rule out the possibility that cross-lagged effects result from a nonconstant third variable. This issue has been labeled the common factor explanation of cross-lagged effects by Link and Shrout (1992; see also Finkel, 1995; Zapf et al., 1996). Link and Shrout's common factor model can be specified and contrasted with a cross-lagged model to determine whether common factors operating behind the two variables might explain the cross-lagged effects. To specify the model, the common factor does not need to be measured (Finkel, 1995; Link & Shrout, 1992; Zapf et al., 1996). Rather, it can be specified as a second-order factor of the two measured variables in the cross-lagged panel model (see Figure 2).

Contrasting the model fit of the common factor model with the model fit of the cross-lagged models allows researchers to test whether a common factor assumption explains the relationship between two variables. There are two variants of the common factor model (Link & Shrout, 1992). In the first, and more restrictive variant, the common factor at T1 influences the common factor at T2. Thus, the two common factors are assumed to be one variable with a certain degree of stability. In the second, and less restrictive variant, the common factors at T1 and T2 only correlate. Thus, they need not necessarily be the same variable but can—at least in theory—be two different variables. To be conservative, we opted for the less restrictive version of the common factor model depicted in Figure 2.

Model fits for all fully lagged bivariate panel models of Samples 1 and 2 as well as the alternative common factor models are provided in Table 6. Notice that the common factor models provided a less adequate fit than the cross-lagged models for all justice dimensions. As indicated by Table 6, there were considerable differences in CFI, TLI, and RMSEA. These additional analyses provide evidence that the cross-lagged models reflect causal processes.

Table 5
Measurement Invariance Analyses

	Sample 1							Samples 2 and 3 (Sample 3 in parentheses)						
Justice/symptom type	SB-χ^2	CSB	*df*	*p*	CFI	TLI	RMSEA	SB-χ^2	CSB	*df*	*p*	CFI	TLI	RMSEA
Distributive justice														
Free loadings	25.17	3.83	15	.05	.998	.996	.033	29.22	3.58	15	.02	.991	.983	.084
Loadings invariant	31.80	3.44	19	.03	.997	.996	.033	34.74	2.91	19	.02	.990	.986	.079
Model difference	6.58[a]		4	.16	.001	.000	.000	—[b]		4	—[b]	.001	−.003	−.005
Interpersonal justice														
Free loadings	6.49	5.13	15	.97	1.000	1.000	.000	7.81	4.76	15	.93	1.000	1.000	.000
Loadings invariant	6.38	8.08	19	1.00	1.000	1.000	.000	10.57	6.70	19	.94	1.000	1.000	.000
Model difference	0.95[a]		4	.92	.000	.000	.000	2.41a		4	.66	.000	.000	.000
Informational justice														
Free loadings	31.48	5.38	29	.34	1.000	1.000	.012	17.89	5.80	29	.95	1.000	1.000	.000
Loadings invariant	32.48	5.36	34	.54	1.000	1.000	.000	22.80	4.87	34	.93	1.000	1.000	.000
Model difference	0.91[a]		5	.97	.000	.000	.012	—[b]		5	—[b]	.000	.000	.000
Interactional justice														
Free loadings	576.80	5.04	125	.00	.985	.981	.076	399.36	0.27	125	.00	.964	.955	.128
Loadings invariant	569.95	5.73	134	.00	.985	.983	.072	221.80	0.65	134	.00	.988	.987	.070
Model difference	23.38[a]		9	.01	.000	−.002	.004	5.95[a]		9	.75	−.024	−.032	.058
Procedural justice														
Free loadings								29.28	6.39	29	.45	1.000	1.000	.009
Loadings invariant								35.41	5.82	34	.40	.999	.999	.018
Model difference								7.58[a]		5	.18	.001	.001	−.009
Depressive symptoms														
Free loadings	196.78	3.27	95	.00	.987	.984	.041	163.28 (188.61)	11.44 (15.71)	125	.01 (.00)	.986 (.995)	.982 (.994)	.048 (.030)
Loadings invariant	212.06	3.13	103	.00	.986	.984	.041	180.35 (197.01)	13.64 (15.24)	134	.00 (.00)	.982 (.995)	.980 (.994)	.051 (.029)
Model difference	13.85[a]		8	.09	.001	.000	.000	13.40[a] (4.48[a])		9	.15 (.88)	.004 (.000)	.002 (.000)	−.003 (.001)

Note. N = 625 for Sample 1, N = 134 for Sample 2, and N = 550 for Sample 3. SB-χ^2 = Satorra–Bentler chi-square correction; CSB = correction factor for the SB-χ^2 difference test; CFI = comparative fit index; TLI = Tucker–Lewis index; RMSEA = root-mean-square error of approximation.
[a] The value is a Satorra–Bentler scaled χ^2-difference statistic estimated using the following formula derived from Satorra and Bentler (2001): $\Delta\text{SB-}\chi^2 = (\text{SB-}\chi^2_{\text{Model 0}} \times \text{CSB}_{\text{Model 0}} - \text{SB-}\chi^2_{\text{Model 1}} \times \text{CSB}_{\text{Model 1}})/[(df_{\text{Model 0}} \times \text{CSB}_{\text{Model 0}} - df_{\text{Model 1}} \times \text{CSB}_{\text{Model 1}})/(df_{\text{Model 0}} - df_{\text{Model 1}})]$. [b] The value was negative. When the difference between models is small, it occasionally happens that the Satorra–Bentler scaled χ^2-difference statistic estimated using the procedures described in Satorra and Bentler (2001) becomes negative. In these instances, the statistic cannot be interpreted (Satorra & Bentler, 2001).

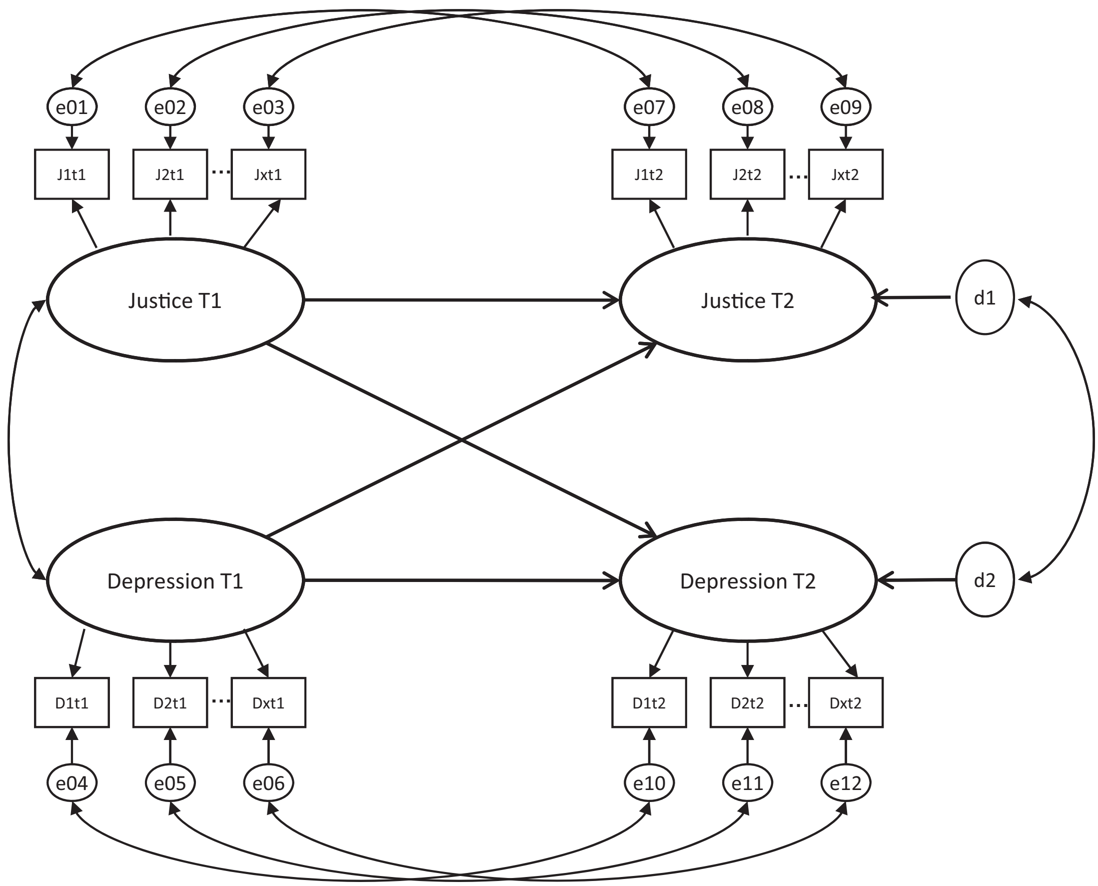

Figure 1. Cross-lagged structural equation model. As shown in the graph, a bivariate cross-lagged panel model includes four different types of indicators: first, the correlation between the latent variables at Time 1; second, the stability of each construct across time; third, the two cross-lagged effects; finally, the correlation between the disturbance terms of the two latent variables at Time 2. T1 = Time 1; T2 = Time 2; J1t1, J2t1, and Jxt1 = items for justice construct at T1; J1t2, J2t2, and Jxt2 = items for justice construct at T2; D1t1, D2t1, and Dxt1 = items for depression at T1; D1t2, D2t2, and Dxt2 = items for depression at T2; e01–e12 = error variances of the items; d1 = error disturbance for justice construct at T2; d2 = error disturbance for depression at T2.

For Samples 1 and 2, we also tested overall models including lagged effects for all available justice perception measures to investigate to what degree the documented effects of justice dimensions in the bivariate models were overlapping. Models treating interpersonal and informational justice as an integrated interactional justice factor provided a good fit in both studies and estimates of lagged effects largely similar to the bivariate models. Models treating interpersonal and informational justice as separate dimensions, in contrast, suffered from multicollinearity between justice measures. In Sample 1, the collinearity led to inadequate model fit not permitting further interpretation. In Sample 2, the fit was adequate; however, as a result of the collinearity, the lagged effect of depressive symptoms on procedural justice was no longer significant (see Table 8). This finding indicates that the effects of the justice dimensions were partly overlapping with the other justice dimensions (not surprising findings given the collinearity between justice measures).[2]

Discussion

The present research examined the causal link between perceptions of organizational justice and depressive symptoms. The longitudinal designs of the three studies allowed us to estimate the contribution of both possible causal directions on the relationship between different justice dimensions and depression in field settings.

Psychological Health → Organizational Justice

Building on recent theoretical and empirical work regarding the affective nature of organizational justice perceptions (Barsky & Kaplan, 2007; van den Bos, 2003), we suggested the theoretical

[2] We did not estimate common factor models for the models including several justice dimensions because common factor models cannot reasonably be specified for models containing several dimensions.

Table 6
Structural Model Fit and Model Comparisons

Model	Sample 1						Samples 2 and 3 (Sample 3 in parentheses)					
	SB-χ^2	*df*	*p*	CFI	TLI	RMSEA	SB-χ^2	*df*	*p*	CFI	TLI	RMSEA
Model 1: DJ and DS	385.93	244	.00	.989	.987	.031	402.52	291	.00	.975	.972	.054
Common factor model	2,578.69	245	.00	.816	.793	.124	1,048.35	292	.00	.830	.811	.140
Model 2: IPJ and DS	136.99	244	1.00	1.000	1.000	.000	277.25	291	.71	1.000	1.000	.000
Common factor model	2,092.18	245	.00	.858	.840	.110	744.98	292	.00	.879	.865	.108
Model 3: IFJ and DS	427.86	291	.00	.991	.990	.027	306.25	342	.92	1.000	1.000	.000
Common factor model	2,940.09	292	.00	.831	.812	.121	440.46	343	.00	.978	.975	.046
Model 4: IAJ and DS	1,192.09	519	.00	.979	.978	.046	369.51	586	1.00	1.000	1.000	.000
Common factor model	4,090.44	520	.00	.890	.881	.105	1,353.13	587	.00	.901	.894	.099
Model 5: DJ, IAJ, and DS	1,540.04	803	.00	.983	.982	.038						
Model 6: DJ, IPJ, IFJ, and DS	6,838.32	790	.00	.862	.850	.111						
Model 7: PJ and DS							300.88 (314.94)[a]	342 (226)[a]	.95 (.00)	1.000 (.993)[a]	1.000 (.992)[a]	.000 (.027)[a]
Common factor model							838.84	343	.00	.890	.879	.104
Model 8: DJ, IAJ, PJ, and DS							1,454.63	1351	.03	.992	.992	.024
Model 9: DJ, IPJ, IFJ, PJ, and DS							1,036.43	1236	1.00	1.000	1.000	.000

Note. $N = 625$ for Sample 1, $N = 134$ for Sample 2, and $N = 550$ for Sample 3. SB-χ^2 = Satorra–Bentler chi-square correction; CFI = comparative fit index; TLI = Tucker–Lewis index; RMSEA = root-mean-square error of approximation; DJ = distributive justice; DS = depressive symptoms; IPJ = interpersonal justice; IFJ = informational justice; IAJ = interactional justice; PJ = procedural justice.
[a] The model is a half-lagged model not including measures for procedural justice at the second measurement occasion.

likelihood that psychological health problems such as depressive symptoms could causally proceed perceptions of organizational justice. Specifically, we proposed that the emotional state of individuals might influence how they perceive aspects of the organization such as justice. We referred to this process as the affective *perception* assumption. The affective perception assumptions might be supplemented by a second process whereby depressed individuals are actually treated differently in reaction to their depressed state leading to subsequent perceptions of injustice among these individuals. We referred to this second process as the affective *reaction* assumption.

Our results across different samples support the proposition that ratings of depressive symptoms influence subsequent perceptions of organizational justice. Despite variations in context and justice dimensions across our samples, the findings provided consistently significant evidence congruent with the proposition that depressive symptoms influence perceptions of justice rather than vice versa. The fact that we found this effect for all organizational justice perception dimensions further suggests that these effects do not rely on specific mechanisms for each of the justice dimensions but originate from a more general principle. Thus, overall it appears that the link from depression to perceptions of justice is quite robust.

Our finding that depressive symptoms influence subsequent perceptions of organizational justice has noteworthy implications. Although research on organizational justice has primarily looked at organizational justice as an independent variable, researchers have recently begun to consider organizational justice perceptions to be important outcome variables in part because research suggests justice perceptions are highly important for the life of individuals (Scott et al., 2007). From this perspective, the present findings suggest that organizations can facilitate perceptions of organizational justice in part by addressing the well-being of their employees. Programs that improve the mental health of employees (e.g., Adler, Bliese, McGurk, Hoge, & Castro, 2009) should not only improve employee well-being but may subsequently also enhance employees' perceptions of organizational justice.

Another practical implication of the present findings may be that organizations should make supervisors aware of justice perception issues among individuals dealing with depression and should seek to implement policies that attempt to mitigate negative organizational justice perceptions. For instance, supervisors might be encouraged to emphasize providing detailed and formalized communication feedback to individuals with depressive symptoms to help lessen negative perceptions of organizational justice. At a minimum, organizations should also recognize that there may be a tendency for both supervisors and other employees to treat depressive employees in ways that could lead the depressed employee to experience negative instances of organizational justice.

Organizational Justice → Psychological Health

Although we found a considerable effect between depressive symptoms at T1 and organizational justice at T2, the effect in the opposite direction was not significant for any justice dimension. These findings run counter to the theoretical perspective in much of the existing literature.

Null findings generally demand a certain degree of interpretational caution (Kluger & Tikochinsky, 2001); nonetheless, we

Table 7
Model Estimates for Sample 1

Effect	Model 1	Model 2	Model 3	Model 4	Model 5
Stabilities					
Distributive justice (DJ)	.31 (.05)**				.32 (.05)**
Interpersonal justice (IPJ)		.30 (.04)**			
Informational justice (IFJ)			36 (.04)**		
Interactional justice (IAJ)				.34 (.04)**	.35 (.04)**
Depressive symptoms (DS)	.48 (.04)**	.48 (.03)**	.49 (.03)**	.48 (.03)**	.48 (.03)**
Cross-lagged effects of justice					
DJ → DS	−.03 (.03)				−.02 (.05)
IPJ → DS		−.04 (.04)			
IFJ → DS			−.01 (.04)		
IAJ → DS				−.02 (.04)	−.01 (.05)
Cross-lagged effects of DS					
DS → DJ	−.13 (.04)**				−.13 (.04)**
DS → IPJ		−.12 (.04)**			
DS → IFJ			−.10 (.04)**		
DS → IAJ				−.11 (.04)**	−.11 (.04)**
Predictor correlations Time 1					
DS ↔ DJ	−.24 (.04)**				−.24 (.04)**
DS ↔ IPJ		−.25 (.04)**			
DS ↔ IFJ			−.20 (.04)**		
DS ↔ IAJ				−.23 (.04)**	−.23 (.05)**
DJ ↔ IPJ					
DJ ↔IFJ					
DJ ↔ IAJ					.59 (.04)**
IPJ ↔ IFJ					
Disturbance correlations Time 2					
DS ↔ DJ	−.21 (.03)**				−.22 (.03)**
DS ↔ IPJ		−.22 (.03)**			
DS ↔ IFJ			−.23 (.03)**		
DS ↔ IAJ				−.23 (.03)**	−.23 (.03)**
DJ ↔ IPJ					
DJ ↔ IFJ					
DJ ↔ IAJ					.37 (.04)**

Note. $N = 625$. All values are standardized coefficients with standard errors in parentheses.
** $p < .01$.

believe the null findings regarding the effects of organizational justice perceptions on depressive symptoms have several immediate implications. First, previous justice research has often described organizational injustice as having strong negative consequences for employee well-being. The present research suggests that the link between justice and well-being is either weak in field settings or substantially more complex than frequently portrayed. Consequently, previous recommendations regarding interventions should be modified. Specifically, on the basis of our findings, we doubt that interventions designed to enhance organizational justice will universally have a considerable impact on employees' subsequent depressive symptoms; rather, the findings suggest that encouraging employees to seek treatment for depressive symptoms will have the added benefit of enhancing perceptions of organizational justice.

Second, we believe the results illustrate the risks with inferring causality from cross-sectional data. It is important to develop theories about the processes underlying relationships observed in cross-sectional data; however, these initial hypotheses need to be tested and modified using longitudinal data. In cases such as the relationship between well-being and justice, results from longitudinal analyses require modifying previous assumptions of causality.

Future research on the causal link between justice and well-being is clearly warranted. At a minimum, research needs to replicate the causal direction observed in the current study. Assuming this link is replicated, theories need to be developed to predict when, and under which circumstances, effects of justice on health surface in field settings, and to test these moderating mechanisms using proper longitudinal tests (e.g., Little, Bovaird, & Card, 2007). In addition, a fruitful goal for future research would be to conduct longitudinal studies that incorporate proper longitudinal tests of mediating mechanisms (e.g., Maxwell & Cole, 2007). We believe that the theoretical ideas described in this article provide a good basis for operationalizing and measuring mediating variables. Examinations of this type may not only be useful for gaining a deeper understanding of effects of depressive symptoms on justice perceptions but may also provide a deeper understanding of why the opposite effect did not emerge in the present research.

Limitations

A potential limitation of the present research is that our findings may lack generalizability beyond the military context. On the positive side, military samples are frequently used in organizational research (e.g., Bartone, 1999; Ippolito, Adler, Thomas, Litz, & Hölzl, 2005)

Table 8
Model Estimates for Samples 2 and 3

	Sample 2							Sample 3
Effect	Model 1	Model 2	Model 3	Model 4	Model 7	Model 8	Model 9	Model 7[a]
Stabilities								
Distributive justice (DJ)	.30 (.11)**					.29 (.15)†	.29 (.15)*	
Interpersonal justice (IPJ)		.25 (.11)*					.26 (.13)†	
Informational justice (IFJ)			41 (.11)**				.37 (.13)**	
Interactional justice (IAJ)				.32 (.11)**		.32 (.13)*	.	
Procedural justice (PJ)					.40 (.09)**	.42 (.09)**	.43 (.12)**	
Depressive symptoms (DS)	.47 (.11)**	.47 (.10)**	47 (.10)**	47 (.10)**	.44 (.12)**	.43 (.12)**	.43 (.16)**	.53 (.06)**
Cross-lagged effects of justice								
DJ → DS	−.05 (.11)					.01 (.18)	.00 (.17)	
IPJ → DS		−.05 (.11)					−.07 (.26)	
IFJ → DS			−.06 (.11)				.04 (.31)	
IAJ → DS				−.06 (.11)		−.02 (.20)		
PJ → DS					.12 (.13)	−.11 (.17)	−.11 (.20)	−.07 (.06)
Cross-lagged effects of DS								
DS → DJ	−.32 (.09)**					−.32 (.09)**	−.32 (.09)**	
DS → IPJ		−.22 (.10)*					−.21 (.11)†	
DS → IFJ			−.17 (.10)†				−.17 (.10)†	
DS → IAJ				−.20 (.11)†		−.19 (.10)*		
DS → PJ					−.19 (.08)*	−.19 (.11)†	−.17 (.11)	
Predictor correlations Time 1								
DS ↔ DJ	−.32 (.09)**					−.32 (.09)**	−.32 (.10)**	
DS ↔ IPJ		−.27 (.10)**					−.25 (.10)**	
DS ↔ IFJ			−.24 (.11)*				−.25 (.09)*	
DS ↔ IAJ				−.26 (.11)*		−.26 (.09)**		
DS ↔ PJ					−.36 (.09)**	−.35 (.11)**	−.34 (.11)**	−.22 (.07)**
DJ ↔ IPJ							.34 (.12)**	
DJ ↔ IFJ							.36 (.11)**	
DJ ↔ IAJ						.36 (.11)**		
DJ ↔ PJ						.42 (.12)**	.42 (.12)**	
IPJ ↔ IFJ							.87 (.04)**	
IPJ ↔ PJ							.28 (.12)**	
IFJ ↔ PJ							.41 (.11)**	
IAJ ↔ PJ						.38 (.11)**		
Disturbance correlations Time 2								
DS ↔ DJ	−.23 (.11)*					−.23 (.12)†	−.23 (.14)	
DS ↔ IPJ		−.01 (.09)					−.01 (13)	
DS ↔ IFJ			−.08 (.09)				−.09 (.11)	
DS ↔ IAJ				−.05 (.10)		−.05 (.11)		
DS ↔ PJ					−.14 (.07)†	−.14 (.08)†	−.16 (.09)†	
DJ ↔ IPJ							.18 (.11)	
DJ ↔ IFJ							.20 (11)†	
DJ ↔ IAJ						.19 (.10)†		
DJ ↔ PJ						.30 (.10)**	.33 (.11)**	
IPJ ↔ IFJ							.76 (.11)**	
IPJ ↔ PJ							.20 (.14)	
IFJ ↔ PJ							.23 (.13)†	
IAJ ↔ PJ						.22 (.11)†		

Note. $N = 134$ for Sample 2, and $N = 550$ for Sample 3. All values are standardized coefficients with standard errors in parentheses.
[a] The model is a half-lagged model not including measures for procedural justice at the second measurement occasion.
† $p < .05$, one-sided. * $p < .05$. ** $p < .01$.

and have been found to yield results similar to nonmilitary samples (e.g., Bliese, 2006; Tucker, Sinclair, & Thomas, 2005; Wynd & Ryan-Wenger, 2004). Another limitation regarding generalizability might be that an underlying bias in the data accounts for why we did not find the expected effect of organizational justice on depressive symptoms. To address this concern, we ran supplementary analyses using other measures of organizational stressors to determine whether we could find evidence of the generally accepted link in which stressors causally precede strains.

Sample 1 contained T1 and T2 data for an eight-item measure of perceived organizational support (Eisenberger, Cummings, Armeli, & Lynch, 1997); the scale was recoded so that higher scores indicated lower perceived organizational support. In Sample 1, perceived lack of organizational support at T1 had a lagged effect on depression at T2 (Standardized Coefficient [Std. Coef.] = –.19, $p < .001$), providing a causal link that supports the idea that depression at a latter time is caused by previous exposures to a stressor.

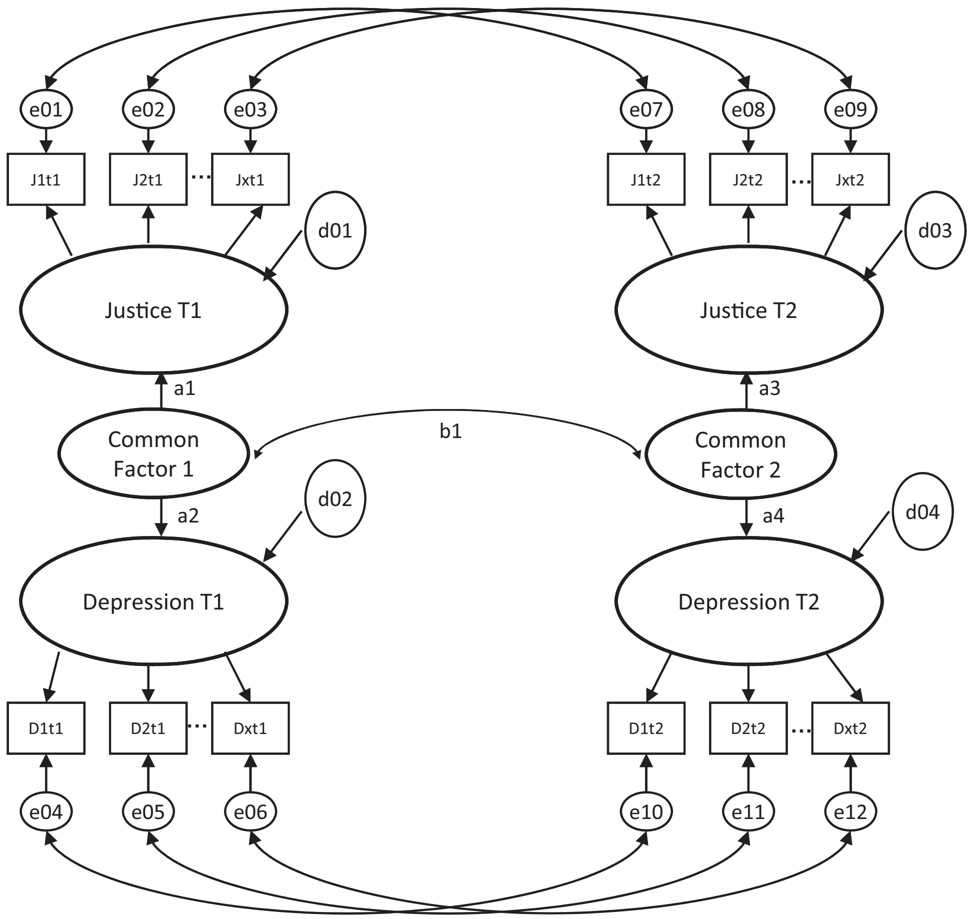

Figure 2. Common factor model as an alternative to the cross-lagged structural equation model in Figure 1. d01–d04 = disturbance terms of the latent variables; b1 = correlation between the common factor at Time 1 (T1) and Time 2 (T2); a1–a4 = effects of the (second order) common factor on the first-order latent variables; e01–e12 = error variances of the items.

For Samples 2 and 3, data from a modified version (Bliese & Castro, 2000) of Camman, Fichman, Jenkins, and Klesh's (1983) Work Overload Scale were available. Whereas Sample 2 contained T1 and T2 data on work overload, Sample 3 only contained T1 data, limiting lagged effect analyses. In both samples, however, work overload at T1 had a lagged effect on depression at T2 (for Sample 2, Std. Coef. = –.30, p = .02; for Sample 3, Std. Coef. = –.12, p = .03, one-tailed). We also found a significant lagged effect of depressive symptoms on work overload in Sample 2 (Std. Coef. = −.24, p = .02), suggesting reciprocal causation—a finding supported by the occupational stress literature (e.g., Demerouti, Bakker, & Bulters, 2004; Panaccio & Vandenberghe, 2009; Zapf et al., 1996).

Importantly, across the three samples, the supplementary analyses found evidence of significant lagged effects where a typical stressor such as work overload at T1 was predictive of depressive symptoms at T2. These supplementary findings suggest that the samples used in the present study were capable of detecting lagged effects where T1 stressors were related to T2 strain either independently or in a reciprocal relationship.[3] Findings from these supplementary stressors support the idea that the lack of lagged effects between T1 justice and T2 strain reflect substantive rather then methodological or sample-based causes.

A final limitation is related to the methodological difficulties with any type of cross-lagged panel designs. Particularly, cross-lagged panel analyses and accompanied tests of common-factor explanations using SEM cannot conclusively rule out all potential

[3] Model fits were as follows: cross-lagged model in Sample 1, SB-χ^2(456, N = 611) = 805.50, p < .001, CFI = .985, TLI = .984, RMSEA = .035; common-factor model in Sample 1, SB-χ^2(457, N = 611) = 783.59, p < .001, CFI = .986, TLI = .985, RMSEA = .034; cross-lagged model in Sample 2, SB-χ^2(244, N = 134) = 321.03, p < .001, CFI = .984, TLI = .982, RMSEA = .049; common-factor model in Sample 2, SB-χ^2(245, N = 134) = 901.68, p < .001, CFI = .865, TLI = .848, RMSEA = .142; and cross-lagged model in Sample 3, SB-χ^2(185, N = 547) = 232.94, p = .010, CFI = .996, TLI = .996, RMSEA = .022. Note that the lagged effects regarding lack of perceived organizational support for Sample 1 need to be interpreted with some caution because a common-factor model provided an equally good fit to the data as the cross-lagged model. Common factor results of this type, however, are frequently found in the occupational stress literature (Zapf et al., 1996).

alternative explanations for documented lagged effects (these issues have been thoroughly discussed elsewhere; for references, see Finkel, 1995; Zapf et al., 1996). Future longitudinal studies should therefore seek to develop hypotheses of potential alternative explanations for lagged effects of depressive symptoms on justice perceptions and to thoroughly test these hypotheses. That said, the possibility of alternative explanations should not lead one to undervalue evidence from cross-lagged panel analyses. There is considerable consensus in the literature that cross-lagged panel analyses using SEM are generally more suited to infer causality in field settings than any type of cross-sectional design. This is particularly the case when research builds upon reasonable evidence from experimental and quasi-experimental designs such as the present research.

Conclusion

The call for lagged and fully cross-lagged studies has repeatedly been sent out to applied researchers (Frese, Garst, & Fay, 2007; Zapf et al., 1996), but the proportion of studies employing these type of designs still remains low in applied research (e.g., Casper, Eby, Bordeaux, Lockwood, & Lambert, 2007; Hülsheger, Lang, & Maier, 2010). The present research provides an example of the value of these designs as a way to advance occupational health research and contributes to the literature by advancing new theoretical perspectives regarding causal relations between organizational justice perceptions and depressive symptoms.

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