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Reassessing the Relationship between Procedural Justice and Police Legitimacy

Jose Pina-Sánchez & Ian Brunton-Smith

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Both authors have contributed equally to this manuscript, with Ian Brunton-Smith leading the analytical part, and Jose Pina-Sánchez undertaking most of the manuscript drafting and literature review.

This study is based on data from the Pathways to Desistance longitudinal study (Mulvey, E. P., 2016. *Research on pathways to desistance: Subject measures, 2000-2010*. Inter-university Consortium for Political and Social Research. doi: 10.3886/ICPSR29961. v2 10.3886. ICPSR29961. v2.). The data can be accessed at the ICPSR online portal (<https://www.icpsr.umich.edu/icpsrweb/NAHDAP/studies/29961/datadocumentation>). The R and Mplus code used to analyse the data has been uploaded to the APA's repository (https://osf.io/hrn8x/?view_only=ed7979de7a6a469ab7963f60c0bbff1c). Findings from our analysis have been disseminated in the IV Criminology Symposium organised by the Sociedad Española de Investigación Criminológica in Granada (Spain) the 27th and 28th of June 2019. The authors have no conflicts of interest to declare.

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Abstract

Objective: A large body of cross-sectional research has identified a positive relationship between perceptions of police procedural justice and legitimacy. Following Tyler's theoretical framework, studies have often interpreted the observed relationship as evidence of an unequivocal causal connection from procedural justice to legitimacy. Here we re-examined the validity of this conclusion by considering the temporal order of that association and the potential biasing effect of time-invariant third common causes.

Hypotheses: (1) Past perceptions of police procedural justice would predict future perceptions of legitimacy; (2) Past perceptions of police legitimacy would predict future perceptions of procedural justice; and (3) Perceptions of police procedural justice and legitimacy would be associated as a result of third common causes.

Method: We fitted random intercepts cross-lagged panel models to seven waves of a longitudinal sample of 1,354 young offenders ($M=16$ years) in the 'Pathways to Desistance' study. This allowed us to explore the directional paths between perceptions of police procedural justice and legitimacy, while controlling for time-invariant participant heterogeneity.

Results: We did not find evidence of the assumed temporal association; lagged within-participant perceptions of procedural justice rarely predicted within-participant perceptions of legitimacy. We did not find evidence of a reciprocal relationship either. Instead, we detected substantial time-invariant participant heterogeneity, and evidence of legitimacy perceptions being self-reproduced.

Conclusions: Our findings challenge the internal validity of the commonly reported positive associations between procedural justice and legitimacy reported in studies using cross-sectional data. Most of such association is explained away after considering time-invariant participant heterogeneity and previous perceptions of legitimacy.

Keywords: procedural justice; legitimacy; longitudinal data; cross-sectional design; police

Public significance statement

Young offenders' perceptions of police fairness did not predict their future perceptions of police legitimacy. Instead, changes in perceptions of police legitimacy seem to be mainly self-reproduced, determined by the individuals' own previous perceptions of police legitimacy. By all means police officers should not abandon principles of fairness in their interactions with young offenders given the many other positive effects they have on cooperation and compliance with the law. What remains unclear is the effectiveness of police fairness as a strategy to foster perceptions of police legitimacy, at least among young offenders.

Procedural justice, understood as the perceived fairness in both the decision process adopted by a particular institution and its interactions with participants under its authority (Tyler, 1990), has become a dominant theory in criminology and legal psychology. A substantial body of evidence has emerged that emphasizes a strong and positive relationship between individual perceptions of procedural justice, their assessments of the legitimacy of criminal justice institutions - such as the police (Gau et al., 2012; Tyler & Wakslak, 2004), courts and tribunals (Tyler & Huo, 2002; Tyler & Rasinski, 1991), and prisons (Beijersbergen et al., 2016; Brunton-Smith & McCarthy, 2016) - and subsequent compliance with law-abiding behavior.

In the context of interactions with the police, researchers have found the positive relationship between procedural justice and legitimacy to be significant across countries and subgroups of the population (Bradford et al., 2014a; Sun et al., 2017; Sunshine & Tyler, 2003). The empirical evidence also appears consistent across competing definitions of legitimacy. For example, Bottoms and Tankebe (2012) disputed whether measures tapping into the 'obligation to obey' with an institution's norms constitute a valid element of legitimacy, leading other researchers to explore 'obligation to obey' and 'trust' dimensions of legitimacy separately (Baker & Gau, 2018; Wolfe et al., 2016). Conversely, other researchers distinguished between 'obligation to obey' and 'moral alignment' with an institution (Hough et al., 2013; Jackson et al., 2012a; 2012b).

Regardless of the measurement strategy or sample configuration, the observed relationship between procedural justice and legitimacy is almost always found to be positive, significant and strong; at least for the body of research based on cross-sectional data. This was corroborated by Walters and Bolger's (2019) meta-analysis, in which they only detected a negative association between procedural justice and legitimacy in one of the 64 studies reviewed (i.e., Reisig & Mesko, 2009).

With some notable exceptions (Murphy, 2005; Walters, 2018) most researchers have - more or less explicitly - interpreted these findings as evidence of a causal effect of procedural justice on legitimacy. This interpretation is both intuitive and consistent with the theoretical framework.

All that is required is treating procedural justice as a process external to the participant, solely defined by the actions of agents of a given authority, which precedes the formation of legitimacy beliefs. However, making such inferences from a body of research dominated by observational studies, most commonly taking the form of cross-sectional surveys (Murphy et al., 2016) is problematic. Such interpretation of the evidence disregards that perceptions of procedural justice and legitimacy are both subjective reports and, for the case of cross-sectional designs, their temporal order cannot be mapped out.

Nagin and Telep (2017) highlighted some of these problems. Following a comprehensive review of the procedural justice model's application in policing research, they concluded that a credible case for causality has not been made. They identified two dominant issues that existing procedural justice research has not been able to dismiss, third common causes (also known as third variables or confounding factors) and reverse causal paths. As a result, they called for a clearer evidence base on the causal effect of procedural justice.

In this study, we re-examined the relationship between perceived procedural justice and police legitimacy using longitudinal data from Mulvey's (2016) Pathways to Desistance project and random intercepts cross-lagged panel models (Hamaker et al., 2015). This innovative modelling strategy offers the possibility of: (1) investigating the presence of a potential reverse pathway from legitimacy to procedural justice; while (2) accounting for the influence of time-invariant third common causes that may be biasing the relation between procedural justice and legitimacy. Exploiting these two key analytical advantages we shed new light into the two main critiques raised by Nagin and Telep (2017), providing new insights into the complex nature of the procedural justice and legitimacy relationship.

The Evidence under Question

In reference to the literature studying perceptions of police procedural justice based on observational data, Nagin and Telep (2017) highlighted the potential biasing effect of unmeasured community and personal factors. For example, given the high levels of residential

segregation in the United States, and the rather common discriminatory practices towards minorities (particularly Blacks), it should be expected that community factors will have an effect both on perceptions of legitimacy and procedural justice, independent of actual interactions with the police or other agents of the criminal justice system. Similarly, at the individual level, the authors pointed at how people with higher stakes in conformity (Toby, 1957), or investments in conventional social bonds (Hirschi, 1969), perceive fairer treatment and greater legitimacy from the authorities enforcing their compliance (Nagin & Telep, 2017). If these potential third common causes are left uncontrolled, the observed associations between procedural justice and legitimacy may very well be spurious.

Nagin and Telep (2017) also argued that much existing research evidence is also consistent with a reverse causal path – from legitimacy to procedural justice – which stems from the subjective and non-sequential nature of cross-sectional survey research. When captured as self-reported perceptions, procedural justice, as much as legitimacy, is not an objective measure of the quality of treatment dispensed by an authority but a subjectively constructed reality (Jonathan-Zamir et al., 2015). Consider as well how under a cross-sectional design reports of procedural justice and legitimacy are collected at - and make reference to - the same time period and it is easy to see that the perceived actions of a given authority might very well be determined by personal affinity towards that particular authority.

There are grounds to think that this reverse path is even more likely in studies based on samples of the general population, which involve requesting information from people who have not necessarily had previous contacts with the authority in question. In the context of Supreme Court decisions, Gibson (1991) questioned the extent to which ordinary citizens can really assess the quality of treatment provided by the Supreme Court when they do not know how it functions. Instead he argued that opinions are likely to be formed from more general attitudes toward the legitimacy of the institution itself. Worden and McLean (2017) made a similar point in relation to reported interactions with better known institutions like the police, further

suggesting that memory failures might reinforce the problem of reverse causality. Specifically, the authors posited that gaps in citizens' recollections are likely filled by their prior attitudes toward the police.

Harkin (2015) provided further rationales supporting the claim that perceptions of institutional legitimacy are not deduced independently. Drawing on the work of Lukes (2005), he highlighted how authorities seek to 'cultivate' support for their legitimacy, which involves forms of ideological self-promotion, meaning that individual beliefs are often as much a consequence of authority-structures as a cause. Harkin (2015) went on to link this view with Bottoms and Tankebe's (2012) understanding of legitimacy as an ongoing dialogue between power-holders and those under their authority, as opposed to a one-off transaction.

Some of these problems were pre-empted in Tyler's original studies (Tyler et al., 1989; Tyler, 1990) where he recognized the potential presence of a reverse causal pathway. In the context of defendants processed through criminal courts (Tyler et al., 1989), but also in the context of perceptions of the police and judges amongst the general public (Tyler, 1990), they found that previously held views on these authorities influenced subsequent assessments of the fairness of their treatment. Importantly, the authors argued that "[t]his influence is unrelated to the impact of the experience itself, suggesting that people's prior views shape the way that people interpret their experience." (Tyler et al., 1989, p. 643). Unfortunately, these early insights on the fluidity of the relationship between procedural justice and legitimacy were not further explored empirically.

Overcoming the Methodological Impasse

To address the limitations of existing procedural justice research, Nagin and Telep (2017) advocated the use of experimental designs. Only by randomly manipulating exposure to procedurally just treatment by agents of criminal justice authorities, they argued, will it be possible to definitively understand whether procedural justice influences perceptions of legitimacy. The authors lamented the scarcity of such applications to police research, and asserted that the validity of the model has not been credibly established. We believe, however,

that this critique, together with the excessive trust placed on experimental methods, should be more nuanced. Tyler (2017) provided a wide range of examples where research in other areas – e.g. work-settings (Cohen-Charash & Spector, 2001) or court-room proceedings (Thibaut & Walker, 1975) - has corroborated the procedural justice model under experimental conditions. But perhaps it is not just the quantity of the experimental evidence available, but the assumed ‘gold standard’ quality of such evidence that should be reconsidered.

Nagin and Sampson (2019) laid out how the practical difficulties affecting the design of experiments in the social sciences can make their external validity questionable. Applications to examine criminal justice interactions directly are perhaps even more questionable. The power relations involved in individual encounters between citizens and criminal justice authorities make them hard to manipulate experimentally, especially in those instances where interactions are potentially contentious (Worden & McLean 2018). As such, procedural justice experiments have mainly been restricted to police-citizen encounters during traffic stops, where interventions typically feature highly scripted police communications (Tyler, 2017). More generally, the discrete nature of interventions in experimental designs fails to capture the fluid and temporally complex nature of the relationship between procedural justice and legitimacy. As highlighted by Tyler (2017, p. 36) “...it is unrealistic to expect a single encounter with the police to substantially influence views that have developed over a lifetime.” One important implication of that fluid relationship is the potential presence of a bi-directional effect, with procedural justice and legitimacy affecting each other. However, experimental designs are not well-suited to examine the potential effect that legitimacy might have on procedural justice, since, as an inherently subjective construct, legitimacy can be extremely difficult, perhaps impossible, to manipulate experimentally.

Clearly there is not a single research design that is uniquely valid, but rather each approach is defined by a different mix of strengths and weaknesses. Elevating experimental designs to a gold

standard position risks ignoring their limitations and missing the important opportunities afforded by alternative approaches.

The Potential of Longitudinal Designs

One approach that has not been fully exploited in the procedural justice literature is longitudinal designs. As demonstrated by Tyler's early work (Tyler, 1990; Tyler et al. 1989), repeat observations across time can be used to examine the temporal order of the relationship between procedural justice and legitimacy. One way to do so is through cross-lagged correlations (Kenny, 1975); where 'lagged' refers to past observations, while 'cross' reflects that it is the past observation of one of the two constructs, for example procedural justice, which is used to predict future observations of the other, legitimacy; and vice versa, past observations of legitimacy are used to predict future values of procedural justice. This research design offers two important advantages. It opens up the possibility of exploring the presence of a likely reverse path from legitimacy to procedural justice, and if adequately expanded, it can enhance the internal validity of findings based on cross-sectional designs.

However, like experimental studies, longitudinal designs should not be considered a panacea. Repeatedly interviewing the same participants and charting how their views change is costly and time consuming, and researchers must often wait many years for the fruits of their labors to manifest. This inevitably means that longitudinal studies are unable to capture the latest theoretical developments; with the need to adopt a consistent measurement strategy across multiple waves of data collection trumping the potential gains from incorporating new dimensions or concepts. Instead they should be considered as another tool enabling researchers to further understand how procedural justice and legitimacy are linked.

Granger causality. In establishing a causal effect three conditions are required: the alleged cause and effect have to be correlated, the cause must precede the effect, and the temporal correlations must reflect a true connection. By establishing whether past perceptions of procedural justice predict future perceptions of legitimacy we can approximate the first two

conditions. Specifically, we can determine the presence of Granger causality (Zyphur et al., 2019), a probabilistic conceptualization of causality heavily relied upon in neuroscience (Bressler, 2011) and similar subjects where it is not easy to conduct experimental designs.

The absence of experimental conditions (randomization in particular) makes it impossible to establish, irrefutably, whether temporal correlations between procedural justice and legitimacy reflect a true connection between the two constructs, or whether the observed correlation is spurious (driven by third common causes). Yet, under the right modelling approach, we can minimize this risk. One simple way to do so is by examining temporal correlations while controlling for contemporaneous and stability (also known as auto-regressive) effects (Rogosa, 1980). When assessing the effect of past perceptions of procedural justice on legitimacy, this involves also taking account of current perceptions of procedural justice (the contemporaneous effect) and past perceptions of legitimacy (the stability effect) as predictors. By controlling for the former we can eliminate third common causes associated with potential methods effects, such as self-acquiescence bias, social desirability bias, or interviewer effects; through the latter we can estimate changes in legitimacy (Adachi & Willoughby, 2014), independent of each participant's initial perceptions at the start of the study.

However, Hamaker et al. (2015) showed that if the construct under examination is trait-like and time-invariant in nature - as we would expect from the community, demographic and personality factors thought to influence procedural justice and legitimacy - then the inclusion of stability parameters will fail to adequately control for that effect and the estimates of the cross-lagged model will still be biased. One way of dealing with this problem is to add "[...] a long list of potentially influential covariates to the model" (Jackson and Pósch, 2019, p. 15).

In this paper, we suggest an alternative strategy, based on Hamaker et al. (2015) differentiation of *between-* and *within-*person effects. This involves partitioning procedural justice and legitimacy into two parts, one that could be attributed to stable differences *between* participants (capturing, for example, systematic differences in the strength of the association that are due to a person's

race), and another capturing changes *within* participants across time (such as the expected effect that experiencing positive procedural justice interactions will have in increasing police legitimacy). Crucially, under such an approach, all time-invariant unobserved heterogeneity between participants is comprehensively controlled for (see also Bell & Jones, 2015; and Hamaker & Muthén, 2019). That is, the influence of any stable differences between participants across the window of observation, which might be biasing the relationship between procedural justice and legitimacy if left uncontrolled, is effectively eliminated.

Evidence from the longitudinal literature. Multiple longitudinal studies have shown perceptions of procedural justice to be positively associated with beliefs of legitimacy. This has been found in the context of police interactions (Murphy et al., 2008; Tyler & Fagan, 2008; Van der Toorn et al., 2011), using the Pathways to Desistance survey, where perceptions of police and court legitimacy were reported (Augustyn, 2015; Fagan & Piquero, 2007; Lee et al. 2011), and in interactions with other criminal justice authorities; see, e.g., Penner et al. (2014) and Sprott and Greene (2010), who used longitudinal samples of young offenders in probation and appearing in court.

However, it would not be appropriate to compile the findings from this longitudinal literature into a summary estimate since the modelling strategies employed are widely heterogeneous. Only one study (Kaiser & Reisig, 2017) used a between/within partition to explore the procedural justice model, but they did not include cross-lagged effects to examine the temporal ordering of the procedural justice and legitimacy association. Of the remaining studies, many did not incorporate lagged procedural justice effects, examining only perceptions of procedural justice and legitimacy measured at the same time. Few among those that examined lagged procedural justice effects also controlled for both stability effects on legitimacy and for the contemporaneous association between procedural justice and legitimacy. And none of these examined the possibility of the reverse pathway, using lagged perceptions of legitimacy as predictors of legitimacy.

To our knowledge, Walters (2018) and Trinkner et al. (2019) are the only studies since Tyler et al. (1989) and Tyler (1990) that have explored the potential effect of legitimacy on procedural justice in a criminal justice setting. Using data from the Pathways to Desistance study, Walters (2018) found that legitimacy beliefs towards police and court authorities at age 18 predicted procedural justice perceptions at age 19, whereas the opposite pathway - from procedural justice to legitimacy - was not found to be significant. Longitudinal studies exploring individual interactions with non-criminal justice authorities have detected a similar reverse path in the relationship between procedural justice and legitimacy (see, e.g., Abdelzadeh et al., 2015, and Grimes, 2016, who studied perceptions of government and teachers legitimacy). By contrast, Trinkner et al. (2019) used longitudinal data combined with an experimental design where a vignette depicting different police-citizen interactions with varying features of procedural justice was shown to participants. The authors showed how procedural justice descriptions of the scene were not determined by previous general perceptions of police legitimacy expressed by participants, which they take as evidence of the absence of such a reverse path. The current evidence base is therefore mixed.

In summary, longitudinal designs offer important avenues to inspect in further detail the validity of the evidence for a positive effect of procedural justice on legitimacy. These possibilities have not yet been fully exploited. Only a few studies have sought to replicate Tyler's original insights pointing at a reverse pathway. And stability and contemporaneous effects are not regularly considered to reduce the presence of third common causes.

Overview of Current Study and Hypotheses

In this study we examined whether associations between procedural justice and legitimacy reported in the literature based on observational data can be interpreted as evidence supporting Tyler's procedural justice model. Specifically, driven by Nagin and Telep's (2017) recent critique, we explored the potential presence of a reverse path and third common causes. We focused on the context of young offender and police interactions captured by the Pathways to Desistance

(Mulvey, 2016). Although these data are now comparatively dated (the first interviews were completed in 2000), it is arguably the longitudinal dataset most commonly used in the procedural justice literature (see, e.g., Kaiser & Reisig, 2017; Piquero et al., 2005; Walters, 2018). We accessed data through the Inter-university Consortium for Political and Social Research, following approval from the Universities of Leeds and Surrey Research Ethics Committees. We analyzed this data using the random intercepts cross-lagged panel model introduced by Hamaker et al. (2015) to better account for third common causes. We tested the following three hypotheses:

H1: Past perceptions of police procedural justice predict future perceptions of legitimacy.

H2: Past perceptions of police legitimacy predict future perceptions of procedural justice.

H3: Perceptions of police procedural justice and legitimacy are spuriously associated as a result of third common causes.

Method

Participants

The Pathways to Desistance is composed of 1,354 young offenders (between the ages of 14 and 17 years at the time of their committing offense) from Philadelphia and Maricopa County, contacted from November 2000 to March 2003 following guilty verdicts or charges for serious offenses in the juvenile or criminal court systems in the two jurisdictions. Interviews took place shortly after their adjudication/conviction. Participants were re-interviewed at six-month intervals for the first three years and one-year intervals for the following four years, resulting in eleven waves of data spread across seven years. Data collection concluded in April 2010. Attrition rates were low throughout, with 84% of the original sample (1,134 participants) successfully re-interviewed in the final wave of the study.

Materials

We limited our analysis to two constructs: perceptions of police procedural justice and legitimacy.

Each construct served as both an independent and dependent variable to examine a potential bi-directional path in which each variable acted as a predictor of the other.

Procedural justice. Pathways to Desistance used 19 questions covering perceptions of fairness and equity adapted from Tyler (1990) and Tyler and Huo (2002) to measure police procedural justice (listed in Appendix A). Most of those questions employed a five-point Likert scale: 1 = strongly disagree, 2 = disagree, 3 = neither agree nor disagree, 4 = agree, 5 = strongly agree; reverse coded when expressed in negative terms. The first 14 questions refer to arrest and other direct interactions with the police taking place during the time interval considered in each wave. The remaining five questions refer to more general perceptions of procedural justice in interactions where the respondent is not directly involved.

Existing studies have relied on an aggregated index based on the combined score from all 19 items, with higher values representing higher perceptions of procedural justice, however, we believe this is problematic. It is questionable whether questions referring to direct and indirect contacts should be conflated in the same index. Especially since after first contact with criminal justice authorities - which made participants eligible for the study – most participants do not report additional contacts with the police across later survey waves. This means that the composition of the global measure of procedural justice varies across participants and waves.

We created a new index using four measures (items 16 to 19) identified as metric invariant (described in subsection ‘Measurement models’ below), which we assumed to be tapping into the same underlying concept across the window of observation. Collectively, these refer to perceptions of equality of treatment shown by the police in their interactions with others. To expand the coverage of our analysis we also used item 15 as a second proxy for procedural justice. Item 15 is the only other procedural justice item asked to all respondents at each survey wave. Formulated as: *‘Of the people you know who have had a contact with the police, how much of their story did the police let them tell?’*; this item taps the concept of voice in their interactions with the

authorities (a core dimension of the concept of procedural justice). Table 1 reports the descriptive statistics for each of the variables used in the analysis.

Legitimacy. In line with Tyler's work, the construct of legitimacy recorded in the Pathways to Desistance seeks to capture confidence in an institution's professionalism, trust in its good intentions, and belief that its norms are entitled to be obeyed. Like procedural justice, most studies relying on this dataset have used an index of legitimacy constructed as the mean of responses to eleven Likert-scale questions (Appendix A). All items were asked to each respondent at each survey wave, however, other important measurement problems still affect this index. Most notably, five of the items referred to perceptions of the courts legitimacy meaning that the overall index is not solely a measurement of police legitimacy, but of the criminal justice system more broadly. In addition, one item seems to reflect manifestations of police procedural justice rather than perceived legitimacy ('Overall, the police are honest') and another was not metric invariant (described below).

To maintain the focus on perceptions of police legitimacy we used the remaining four legitimacy items referring to the following statements: *'I have a great deal of respect for the police'*, *'I feel proud of the police'*, *'People should support police'*, and *'Police should hold suspect until they have evidence'*.

(Table 1 here)

Procedure

We restricted our analysis to the first seven waves of data recorded in the Pathways to Desistance (covering the 2000 to 2006 period). This choice is in response to the interval between surveys, which was expanded from six to twelve months after wave 7. By examining the first seven waves we focus on consistent short-term temporal associations between procedural justice and legitimacy. The mean age of the participants covered within this shorter window of observation is 16 years.

The first part of the analysis involved the specification of measurement models to generate more robust indexes of procedural justice and legitimacy that satisfy conditions of measurement invariance (items that are consistently measuring the same underlying concept at each wave). We then tested our three hypotheses in two stages. First, we report simple cross-lagged correlations between the measures of procedural justice and legitimacy. We used these as a benchmark to determine the unadjusted association between the two constructs across time. To assess whether the observed associations remain significant after controlling for time-invariant participant heterogeneity, we estimated two random intercepts cross-lagged panel models, one for each of the measures of procedural justice used.

In addition, to facilitate comparisons with other studies in the literature based on the Pathways to Desistance, we also replicated the random intercepts cross-lagged panel model using the original indexes of procedural justice and legitimacy based on the aggregation of items using simple means (see Appendix B). We estimated all models in Mplus using maximum likelihood estimation and adjusting for missing data (assumed missing at random; Rubin, 1987).

Measurement models. When using multiple indicators to represent latent constructs, confirmatory factor analysis is a robust approach that effectively summarizes the correlations amongst items and corrects for measurement error. In longitudinal data analysis it is important, however, to ensure that the latent structure exhibits measurement invariance over time (Widaman et al., 2010). Specifically, the magnitude of factor loadings must be similar at each time point. This ensures that the meaning of procedural justice and legitimacy remain consistent over time, and that the observed relationship between them is not biased as a result of changes in the measurement process throughout the window of observation. To assess this, we compared a model with factor loadings freely estimated at each time point to a model where the loadings are fixed at the same value, with a non-significant change in model fit indicating metric invariance.

The four procedural justice items - collectively tapping into equality of treatment - exhibited no significant change in model fit when factor loadings were constrained to equality (p value = 0.06,

$\chi^2 = 8.0$, with 28df) confirming metric invariance. We initially explored five items tapping into police legitimacy, but the loadings for one item on stop and search practices (item 6 in Appendix A) varied substantially across waves. We therefore restricted the legitimacy measurement model to four items which exhibited metric invariance ((p-value = .08, $\chi^2 = 26.8$, with 18df). Table 2 reports the factor loadings for the latent measures of procedural justice and legitimacy.

(Table 2 here)

Exploratory analysis. We began the analysis of the relationship between procedural justice and legitimacy by looking at the (unadjusted) bivariate cross-lagged correlations. We used Pearson's correlation coefficients – assuming linearity - throughout to facilitate comparisons across measures of procedural justice. We report these results in a matrix including correlations between procedural justice and legitimacy measured at the same time, and with each other's previous time point measures.

Longitudinal models. In the main part of the analysis, we estimated random intercepts cross-lagged panel models (Hamaker et al., 2015). Figure 1 visually represents the composition of our first random intercepts cross-lagged panel model, where both procedural justice and legitimacy are simultaneously estimated using confirmatory factor analysis and four items for each of those two constructs (when using the original aggregate indexes for procedural justice and legitimacy, and when using the single procedural justice item representing 'voice', we replaced the measurement models with an observed indicator).

In essence, this approach can be thought of as an extension of a standard cross-lagged panel model (Finkel, 1995). Participants' assessments of legitimacy are predicted by perceptions of procedural justice measured in the previous interview (pathway a), whilst also controlling for prior levels of legitimacy (pathway b) and the current association with procedural justice (pathway c). Simultaneously, we explored the potential reverse pathway, with prior assessments

of legitimacy related to current perceptions of procedural justice (pathway d), while controlling for prior levels of procedural justice (pathway e) and current associations with legitimacy (pathway c). Together, pathways a and d represent the cross-lagged coefficients, pathways b and e the stability coefficients, and pathways c the contemporaneous coefficients. To determine whether the lagged effects of procedural justice and legitimacy vary throughout our window of observation, all structural pathways are freely estimated across each wave. We also allowed the residual error for each indicator variable to covary with itself across measurement occasions, which ensures estimates of the lagged pathways are not biased upwards by shared measurement error (Williams & Podsakoff, 1989). Following Hamaker et al (2015), we distinguished the between-person and within-person levels of procedural justice and legitimacy by estimating them as separate latent variables. This ensures that estimates of within-person changes over time (the stability coefficients) are not confounded with differences between participants (Zyphur et al., 2019). The between-person levels of procedural justice and legitimacy ('PJ *between*' and 'Leg *between*' in Figure 1) are measured by the wave specific procedural justice and legitimacy items, with factor loadings constrained to one and means allowed to vary over time. The within person levels of procedural justice and legitimacy ('PJ *within*' and 'Leg *within*') consequently represent the individual's temporal deviations from their expected score on each measure.

(Figure 1 here)

Importantly in the random intercepts specification of the cross-lagged panel model, the stability parameters (pathways b and e) no longer represent the stability of the rank order of individuals' procedural justice and legitimacy ratings from one occasion to the next. Instead they capture the amount of within-person carry-over effect between each wave. Positive stability parameters indicate that occasions when a person's procedural justice (or legitimacy) perception are higher than expected they are likely to be followed by occasions on which he or she again

scores above their expected perception (Hamaker et al., 2015). Conversely, a negative effect suggest that occasions when someone scores below their expected perception are followed by subsequent occasions where they also score below expected. In other words, these stability parameters indicate the consistency of the rank-order of individual deviations from their expected mean at each wave. We therefore account for both temporal stability and time-invariant, trait-like stability, giving us a clearer picture of the extent to which third common causes might be affecting the potential direct effect of procedural justice on legitimacy and vice versa.

Researcher seeking to replicate this analytical procedure could do so by accessing the data from Pathways to Desistance, available at the ICPSR portal. We have also uploaded the R and Mplus code used to estimate all the findings reported in this article here, https://osf.io/hrn8x/?view_only=ed7979de7a6a469ab7963f60c0bbff1c.

Results

Exploratory Analysis Results

Table 3 reports the cross-lagged correlation matrix for the two measures of procedural justice with legitimacy. All correlation coefficients are positive and statistically significant. Consistent with the expectations of procedural justice theory, this includes the correlations between legitimacy and prior perceptions of procedural justice. However, we also found similar sized – in many instances stronger - correlations for procedural justice with previous perceptions of legitimacy, suggesting the presence of a reverse path.

(Table 3 here)

Longitudinal Models Results

The results from our random intercepts cross-lagged panel models (Table 4) tell a very different story. After we specified contemporaneous and stability coefficients and correctly took into account the presence of stable trait-like differences between participants, we no longer found clear evidence of significant cross-lagged effects.

H1: Past perceptions of police procedural justice predict future perceptions of legitimacy. We detected one statistically significant cross-lagged effect of procedural justice on legitimacy in the model using *treatment*, although contrary to expectations this effect is negative. The only expected effect of procedural justice is found in waves 5 and 7 in the model using *voice*. That is, only two of the twelve cross-lagged effects of procedural justice on legitimacy point in the expected direction, which lead us to reject Hypothesis 1.

H2: Past perceptions of police legitimacy predict future perceptions of procedural justice. We did not find substantial evidence of a reverse path either. Only one of the twelve cross-lagged effects of legitimacy on procedural justice was statistically significant (wave 6 of the *voice* model), which leads us to reject Hypothesis 2.

These two results were corroborated when we replicated our random intercepts cross-lagged panel model using the aggregate measures of procedural justice and legitimacy commonly employed in previous studies of the Pathways to Desistance (Appendix Table B1). We only identified one cross-lagged coefficient where procedural justice had the expected positive effect on legitimacy (wave 2), while we also found a statistically significant positive cross-lagged effect from legitimacy to procedural justice (wave 3).

(Table 4 here)

H3: Perceptions of police procedural justice and legitimacy are spuriously associated as a result of third common causes. The contrast between the significant unadjusted cross-lagged coefficients reported in Table 3 and the adjusted and rarely significant cross-lagged

coefficients reported in Table 4 provides support for Hypothesis 3. Two key factors seem to be behind these differences, strong stability effects, and the substantial residual correlation between procedural justice and legitimacy, over and above the within-person correlations. The latter points at the presence of time-invariant third common causes, while the former constitutes the main predictor of both procedural justice and legitimacy.

Specifically, the positive and substantial effect sizes observed for the stability coefficients indicate that people whose prior reported levels of legitimacy and procedural justice were higher than average also exhibited higher than expected subsequent levels of procedural justice and legitimacy. Conversely, those who reported lower average values at earlier times reported lower than expected values on subsequent occasions. Displayed visually (Figure 2), we can see a clear growth pattern throughout the window of observation, particularly for legitimacy which roughly doubles in size from wave 2 to 7.

(Figure 2 here)

In short, most of the correlations between procedural justice and legitimacy observed in the exploratory analysis seem to be derived from either time-invariant third common causes, or changes in individuals' perceptions of legitimacy across time. However, Hypothesis 3 was not fully supported, since all contemporaneous coefficients except one (wave 2 of the model using *voice*) are also positive and statistically significant. These coefficients represent the association in the procedural justice and legitimacy within-participant change whilst controlling for their previous levels of procedural justice and legitimacy. As such, they could be capturing time-invariant third common causes, such as interviewer effects. However, they could also be capturing short lived effects of procedural justice on legitimacy. Unfortunately, in this case, the direction in which this association operates cannot be disentangled since they refer to values of procedural justice and legitimacy measured in the same time-period.

Discussion

In this study we have re-examined the internal validity of the commonly reported positive effect of procedural justice on police legitimacy. Following Nagin and Telep's (2017) critique of the literature, composed in its majority of observational studies, we have focused on exploring the presence of biasing effects resulting from a potential reverse path – from legitimacy to procedural justice - and third common causes. To do so we employed a new random intercepts cross-lagged panel model approach and seven waves of data from the Pathways to Desistance, a well-known longitudinal study capturing young offenders' perceptions of their interactions with the police. Contrary to expectations individual changes in perceptions of police legitimacy are not predicted by previous perceptions of procedural justice. We did not find evidence supporting a reverse path from legitimacy to procedural justice either. Instead, the observed association between procedural justice and legitimacy appears to be mainly explained by third common causes. We found that this can take the form of time-invariant differences between participants, and individual changes in perceptions of police legitimacy across time, which explain a growing share of future individuals' perceptions of police legitimacy.

The Evidence from the Observational Literature Needs Nuancing

In assessing the implications of our findings it is important to keep in mind that they stem from the analysis of a single survey, one with a very specific sampling strategy targeted at young offenders from just two counties in the US. This limits its external validity. Nevertheless, the important role of time-invariant participant heterogeneity and prior perceptions of legitimacy in our study, coupled with the fact that these are rarely controlled for in the literature, suggests a more cautious approach when assessing the evidence based on observational designs. It seems likely that some of the positive associations reported in the literature will not remain statistically significant when these two components are correctly controlled for, and that the strength of reported associations is likely overestimated.

Beyond the comparison of unadjusted and adjusted correlations reported in our analysis, we can further illustrate the questionable effect attributed to procedural justice by comparing our findings to other studies that have employed data from the Pathways to Desistance. Such comparisons are far from perfect since differences will remain in the window of observation, and the specific measures employed. However they can still shed new light on the significance of the modelling strategy adopted. For example, McLean et al. (2019) used a similar measure of procedural justice tapping into equality of treatment, but did not adjust for previous perceptions of legitimacy or time-invariant participant heterogeneity. The authors estimated the association between perceptions of procedural justice and legitimacy measured at the same time point roughly five times bigger than the contemporaneous effects that we reported, which in our case remained statistically significant but substantively small, ranging from 0.03 to 0.04.

Even larger reductions in effect size can be observed when comparing results using the original procedural justice and legitimacy indexes (reported in Table B1) with other studies using these measures. For example, under a similar modelling approach to McLean et al. (2019), where no lagged legitimacy effects or time-invariant participant heterogeneity were considered, and procedural justice and legitimacy are measured at the same time-point, Augustyn (2015) reported an association roughly ten times stronger than the association we observed. Importantly, this is after the author controlled for fifteen predictors, suggesting that the strategy to control for third common cause bias using a series of theoretically relevant variables may not be sufficient.

Self-Reproduced Legitimacy and Procedural Justice

Although not part of the initial hypotheses we set out to examine, it is worth emphasizing the important self-reproducing effects that we observed in the perceptions of procedural justice and legitimacy. Mazerolle et al. (2012; 2013a) first suggested these self-reproducing mechanisms, which Walters (2018) has recently confirmed. The novelty here stems from our focus on within-participant trajectories. This allowed us to demonstrate how it is not simply that previous perceptions of procedural justice are the main predictors of current views, but also that

participant trajectories diverge in time. Those who are more likely to hold negative views see their views reinforced negatively with time, whilst those holding positive views become more positive.

This illustrates the importance of early life perceptions and resonates well with much of the literature from developmental criminology on legal socialization (Cohn & White, 1990; Fagan & Tyler, 2005; 2007), but also with some of the evidence examining the formation of perceptions of legitimacy in the criminal justice system. For example, Fine and Cauffman's (2015) reported an increase in perceptions of criminal justice legitimacy in the transition to adulthood across white offenders, while a negative trend was detected for black offenders; see also Fine et al. (2017) who demonstrated similar divergent trends in legitimacy as a result of young offenders being rearrested.

Caveats

On the potential presence of a direct effect. Whilst we found no evidence that past perceptions of procedural justice predict changes in perceptions of legitimacy, we cannot rule out entirely that procedural justice increases legitimacy. The six-month interval between interviews used in the survey, coupled with the statistically significant contemporaneous effects detected between procedural justice and legitimacy, mean it is possible that such an effect is present, albeit short-lived, dissipating before legitimacy is measured again in the following wave. A more short-term effect of procedural justice would be consistent with most of the experimental research that shows how procedural justice practices from the police have a positive effect with regards to encounter specific perceptions of a particular interaction but not with more general perceptions of the police (Lowrey et al., 2016; Maguire et al., 2017; Mazerolle et al., 2013b).

An alternative interpretation for the observed contemporaneous effects between procedural justice and legitimacy would be to see them as overlapping terms. This was theorized by Bottoms and Tankebe (2012), who argued that procedural justice could be a constitutive element of legitimacy, rather than an external causal factor.

Similarly, it is possible that our results are still affected by third common causes, something that we cannot rule out entirely since our models primarily adjust for time-invariant between-participant heterogeneity. It is not difficult to think of time-varying factors possibly influencing some of our results. For example, it is possible that personal identity, which research has shown to take shape more intensely before reaching adulthood (Meus, 2011), could be explaining the diverging trajectories in perceptions of legitimacy across participants, as perhaps implied by the strong stability effects that we observed. It is harder, however, to see how third common cause bias can be explaining the lack of significance for the cross-lagged effects included in our models. This would involve the presence of an unaccounted time-varying factor positively associated with past perceptions of procedural justice while negatively associated with present perceptions of legitimacy, or vice versa. Still, even if specific factors can be difficult to pin down theoretically, the wide range of time-varying factors that could be acting as potential third common causes is long (e.g. moral disengagement, disenfranchisement, impulse control, etc.), and so it is prudent to leave open that possibility.

It is also important to highlight that we found the association of perceptions of procedural justice and legitimacy to be positive and significant at the between-participant level. This is a time-invariant relationship; participants who systematically report higher perceptions of procedural justice tend to report higher beliefs of legitimacy throughout our window of observation, and vice versa. We cannot determine the direction of that relationship if there is one, but we cannot rule out that it is the result of a potential effect of procedural justice on legitimacy that took place at a time point earlier than the start of the window of observation contemplated in this study. Such a hypothetical early effect would be consistent with Jackson and Pósch's (2019) 'temporal stickiness' hypothesis, which suggests that perceptions of procedural justice and their effect on legitimacy might be formed during early interactions with the authorities.

Limitations of the Pathways to Desistance. Lastly, there are important limitations with how procedural justice and legitimacy are measured in the dataset that we should not overlook. The original procedural justice and legitimacy indexes included in pathways to desistance do not reflect the latest theoretical developments on the field, are not internally consistent, appropriately aggregated, or invariant across time. As a result, we opted to create new measures using theoretically relevant items showing adequate internal and time-invariant consistency, and to conduct separate analyses for different dimensions of procedural justice referring to equality of treatment and voice. This more statistically principled approach has, however, limited the coverage of our study since we were not able to employ measures of procedural justice reliably tapping into other dimensions of the construct such as quality and respect of police interactions. In addition, it is also possible that our measure on equality of treatment may be tapping into elements of distributive justice. A similar criticism could be made to the measure of legitimacy used, which does not reflect some of the new dimensions considered in recent studies on the subject, such as felt obligation to obey, or normative alignment.

This inability to ‘move with the times’ and reflect the most recent theoretical developments is endemic to longitudinal studies that typically take place over many years at substantial expense. Here, the commitment to collect a set of consistent measures from one period to the next limits the capacity to update questions and incorporate new dimensions of the concepts under study. As such, the extent to which the Pathways to Desistance does not capture the latest theoretical developments in procedural justice theory is understandable, particularly when considered alongside the remarkable theoretical progress that the field has undergone over the last decade. To address this limitation and to assess the external validity of our findings, we encourage researchers with access to more recent longitudinal datasets on the subject to replicate the random intercepts cross-lagged model employed here.

Implications for Law Enforcement

Our findings, together with the experimental evidence in the literature, point at the ineffectiveness of procedural justice to foster police legitimacy across time. This, however, should not justify the rejection of procedural justice principles by the police or any other criminal justice authorities. Beyond legitimacy there is a wide range of research pointing at the positive impact of procedural justice on many other aspects of a well-functioning police force, and criminal justice system more broadly. These include voluntary compliance (Murphy et al., 2009), trust (Hough et al., 2010), or cooperation (Tyler et al., 2010), to name a few. Much of that literature is based on observational data, but there is also some experimental evidence supporting the expected positive effect of police procedural justice, see for example Murphy et al.'s (2014) reported positive effect on trust, or Paternoster et al. (1997) on reduced offending. It is also worth acknowledging the vast amount of experimental evidence documented in the broader psychological literature, where procedural justice is shown to foster compliance with a wide range of authorities (MacCoun, 2005). It is therefore most likely that the beneficial effect attributed to police procedural justice remains unaltered, only that this effect may not be mediated through legitimacy.

Conclusion

Our findings call into question the validity of the commonly attributed effect of procedural justice on police legitimacy. Nagin and Telep (2017) identified two problems that could be affecting the main body of evidence on the subject that has so heavily relied on observational designs: third common causes and reverse causality. Though we did not detect evidence of the latter, we found that third common causes bias might be substantial. We suggest that: future interpretations of the cross-sectional evidence base pointing at a strong effect of procedural justice on legitimacy should consider the possibility that the strength of this relationship may be overestimated; and that future empirical studies consider the still much untapped potential afforded by longitudinal methods on this subject.

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Table 1. Descriptive statistics

Variable labels*	Wave 1			Wave 2			Wave 3			Wave 4			Wave 5			Wave 6			Wave 7		
	Mean	SD	N																		
Proc. just. (treatment)	0.00	0.41	1353	0.00	0.39	1261	0.00	0.50	1260	0.00	0.55	1228	0.00	0.54	1230	0.00	0.53	1233	0.00	0.60	1231
<i>Different treatment gender</i>	2.51	1.03	1341	2.58	1.00	1259	2.59	1.03	1259	2.60	1.00	1226	2.61	1.01	1228	2.64	0.98	1232	2.59	0.97	1230
<i>Different treatment age</i>	2.61	1.06	1348	2.56	1.01	1259	2.6	1.01	1260	2.56	0.97	1228	2.55	1.00	1230	2.61	0.96	1233	2.54	0.93	1231
<i>Different treatment race</i>	2.67	1.09	1352	2.76	1.06	1259	2.77	1.03	1256	2.79	1.02	1226	2.76	1.05	1230	2.81	1.00	1233	2.76	1.01	1228
<i>Diff. treat. neighbourhoods</i>	2.46	1.04	1353	2.53	1.02	1261	2.53	1.01	1260	2.59	1.00	1227	2.59	1.00	1230	2.66	1.00	1231	2.59	0.99	1228
Proc. just. (voice)	2.38	1.11	1135	2.34	1.01	979	2.25	0.99	928	2.26	0.99	846	2.30	0.99	769	2.22	0.93	695	2.24	1.01	704
Legitimacy	0.00	0.65	1353	0.00	0.70	1261	0.00	0.72	1260	0.00	0.71	1228	0.00	0.71	1230	0.00	0.74	1233	0.00	0.76	1232
<i>I have respect for the police</i>	2.01	1.07	1352	2.00	1.02	1261	2.10	1.03	1260	2.12	1.00	1227	2.12	0.99	1229	2.22	0.99	1233	2.16	1.00	1231
<i>I feel proud of the police</i>	1.78	0.92	1352	1.80	0.90	1260	1.90	0.95	1260	1.97	0.93	1227	2.01	0.95	1229	2.06	0.94	1231	2.06	0.94	1230
<i>People should support</i>	2.16	1.04	1352	2.18	0.99	1260	2.26	0.98	1259	2.30	0.97	1226	2.35	0.99	1229	2.38	0.98	1231	2.38	0.98	1229
<i>Should hold suspect</i>	2.19	1.16	1351	2.08	1.09	1259	2.12	1.08	1259	2.10	1.06	1225	2.12	1.04	1230	2.10	1.03	1229	2.09	1.01	1228

Note. Variables in italics represent the items used to estimate latent variables of procedural justice and legitimacy.

Table 2. Measurement models

	Factor loading	SE
<i>Procedural Justice</i>		
Police treat males and females differently	1.00	0.00
Police treat differently depending on age	0.98	0.03
Police treat differently depending on race/ethnic group	1.36	0.04
Police treat differently by neighborhoods	1.26	0.03
<i>Legitimacy</i>		
I have a great deal of respect for the police	1.00	0.00
I feel proud of the police	1.05	0.02
People should support police	1.03	0.02
Police should hold suspect until they have evidence	0.53	0.02

Table 3. Contemporaneous and cross-lagged pairwise correlations between procedural justice and the two measures of procedural justice (p-values within brackets)

	Legitimacy 1	Legitimacy 2	Legitimacy 3	Legitimacy 4	Legitimacy 5	Legitimacy 6	Legitimacy 7
Treatment 1	0.21 (<0.001)	0.17 (<0.001)					
Treatment 2	0.19 (<0.001)	0.26 (<0.001)	0.16 (<0.001)				
Treatment 3		0.22 (<0.001)	0.27 (<0.001)	0.21 (<0.001)			
Treatment 4			0.18 (<0.001)	0.25 (<0.001)	0.18 (<0.001)		
Treatment 5				0.20 (<0.001)	0.22 (<0.001)	0.18 (<0.001)	
Treatment 6					0.15 (<0.001)	0.24 (<0.001)	0.21 (<0.001)
Treatment 7						0.17 (<0.001)	0.23 (<0.001)
Voice 1	0.19 (<0.001)	0.14 (<0.001)					
Voice 2	0.09 (0.005)	0.15 (<0.001)	0.10 (0.003)				
Voice 3		0.17 (<0.001)	0.23 (<0.001)	0.18 (<0.001)			
Voice 4			0.24 (<0.001)	0.25 (<0.001)	0.25 (<0.001)		
Voice 5				0.09 (0.01)	0.18 (<0.001)	0.15 (<0.001)	
Voice 6					0.23 (<0.001)	0.27 (<0.001)	0.28 (<0.001)
Voice 7						0.25 (<0.001)	0.28 (<0.001)

Table 4. Results from the random intercepts cross-lagged panel models

	Model 1: Treatment				Model 2: Voice			
	<i>Coef.</i>	<i>95% CI</i>	<i>SE</i>	<i>P value</i>	<i>Coef.</i>	<i>95% CI</i>	<i>SE</i>	<i>P value</i>
<i>Procedural justice (w2)</i>								
Procedural justice (w1)	0.08	(-0.04, 0.20)	0.06	0.18	-0.006	(-0.08, 0.06)	0.04	0.87
Legitimacy (w1)	0.02	(-0.05, 0.09)	0.04	0.60	0.00	(-0.15, 0.15)	0.08	0.99
<i>Procedural justice (w3)</i>								
Procedural justice (w2)	0.07	(-0.08, 0.21)	0.07	0.38	0.03	(-0.05, 0.11)	0.04	0.44
Legitimacy (w2)	0.06	(-0.03, 0.15)	0.05	0.19	0.06	(-0.11, 0.23)	0.09	0.47
<i>Procedural justice (w4)</i>								
Procedural justice (w3)	0.17	(0.05, 0.30)	0.06	0.005	0.15	(0.06, 0.23)	0.05	0.001
Legitimacy (w3)	-0.03	(-0.12, 0.07)	0.05	0.60	0.14	(-0.05, 0.33)	0.09	0.14
<i>Procedural justice (w5)</i>								
Procedural justice (w4)	0.17	(0.06, 0.28)	0.06	0.003	0.09	(-0.005, 0.19)	0.05	0.06
Legitimacy (w4)	0.06	(-0.04, 0.15)	0.05	0.26	-0.11	(-0.31, 0.09)	0.10	0.27
<i>Procedural justice (w6)</i>								
Procedural justice (w5)	0.22	(0.13, 0.31)	0.05	<0.001	0.08	(-0.02, 0.17)	0.05	0.12
Legitimacy (w5)	-0.04	(-0.13, 0.31)	0.04	0.32	0.18	(0.01, 0.35)	0.09	0.04
<i>Procedural justice (w7)</i>								
Procedural justice (w6)	0.37	(0.29, 0.45)	0.04	<0.001	0.14	(0.03, 0.26)	0.06	0.02
Legitimacy (w6)	0.02	(-0.06, 0.09)	0.04	0.64	0.14	(-0.04, 0.31)	0.09	0.12
<i>Legitimacy (w2)</i>								
Procedural justice (w1)	0.03	(-0.10, 0.16)	0.07	0.65	0.02	(-0.02, 0.05)	0.02	0.42
Legitimacy (w1)	0.20	(0.11, 0.29)	0.05	<0.001	0.21	(0.12, 0.29)	0.05	<0.001
<i>Legitimacy (w3)</i>								
Procedural justice (w2)	-0.19	(-0.35, -0.04)	0.08	0.02	-0.03	(-0.07, 0.02)	0.02	0.28
Legitimacy (w2)	0.29	(0.18, 0.39)	0.05	<0.001	0.26	(0.16, 0.37)	0.05	<0.001
<i>Legitimacy (w4)</i>								
Procedural justice (w3)	0.03	(-0.11, 0.16)	0.07	0.68	-0.004	(-0.06, 0.05)	0.03	0.87
Legitimacy (w3)	0.29	(0.17, 0.41)	0.06	<0.001	0.29	(0.18, 0.40)	0.06	<0.001
<i>Legitimacy (w5)</i>								
Procedural justice (w4)	-0.001	(-0.12, 0.12)	0.06	0.99	0.06	(0.003, 0.11)	0.03	0.04
Legitimacy (w4)	0.27	(0.16, 0.39)	0.06	<0.001	0.26	(0.15, 0.38)	0.06	<0.001

<i>Legitimacy (w6)</i>								
Procedural justice (w5)	-0.008	(-0.11, 0.09)	0.05	0.87	0.02	(-0.04, 0.08)	0.03	0.48
Legitimacy (w5)	0.40	(0.30, 0.50)	0.05	<0.001	0.40	(0.30, 0.50)	0.05	<0.001
<i>Legitimacy (w7)</i>								
Procedural justice (w6)	0.009	(-0.08, 0.10)	0.05	0.86	0.10	(0.04, 0.16)	0.03	0.002
Legitimacy (w6)	0.43	(0.34, 0.52)	0.05	<0.001	0.41	(0.32, 0.50)	0.05	<0.001
<hr/>								
<i>Contemporaneous effects</i>								
Proc. just. (w1) – Legit. (w1)	0.04	(0.02, 0.06)	0.01	0.001	0.09	(0.04, 0.13)	0.02	<0.001
Proc. just. (w2) – Legit. (w2)	0.04	(0.02, 0.06)	0.01	<0.001	0.04	(-0.002, 0.08)	0.02	0.06
Proc. just. (w3) – Legit. (w3)	0.04	(0.03, 0.06)	0.01	<0.001	0.05	(0.02, 0.09)	0.02	0.006
Proc. just. (w4) – Legit. (w4)	0.04	(0.02, 0.06)	0.01	<0.001	0.05	(0.008, 0.08)	0.02	0.02
Proc. just. (w5) – Legit. (w5)	0.03	(0.005, 0.05)	0.01	0.01	0.06	(0.02, 0.10)	0.02	0.005
Proc. just. (w6) – Legit. (w6)	0.04	(0.02, 0.06)	0.01	<0.001	0.06	(0.02, 0.10)	0.02	0.003
Proc. just. (w7) – Legit. (w7)	0.04	(0.02, 0.06)	0.009	<0.001	0.07	(0.03, 0.11)	0.02	0.001
<hr/>								
<i>Random effects</i>								
Var. random int. proc. just.	0.15	(0.13, 0.17)	0.01	<0.001	0.21	(0.18, 0.25)	0.02	<0.001
Var. random int. legitimacy	0.32	(0.28, 0.36)	0.02	<0.001	0.31	(0.28, 0.35)	0.02	<0.001
Covariance proc. just.-legit.	0.09	(0.07, 0.11)	0.009	<0.001	0.11	(0.09, 0.14)	0.01	<0.001
<hr/>								
<i>Goodness of fit</i>								
RMSEA/CFI/TFI	0.02/0.98/0.97				0.02/0.96/0.99			
<hr/>								
<i>Sample size</i>								
Participant	1354				1354			
<hr/>								

Note. All coefficients are standardized.

Figure 1. Graphical representation of the random intercepts cross-lagged panel model

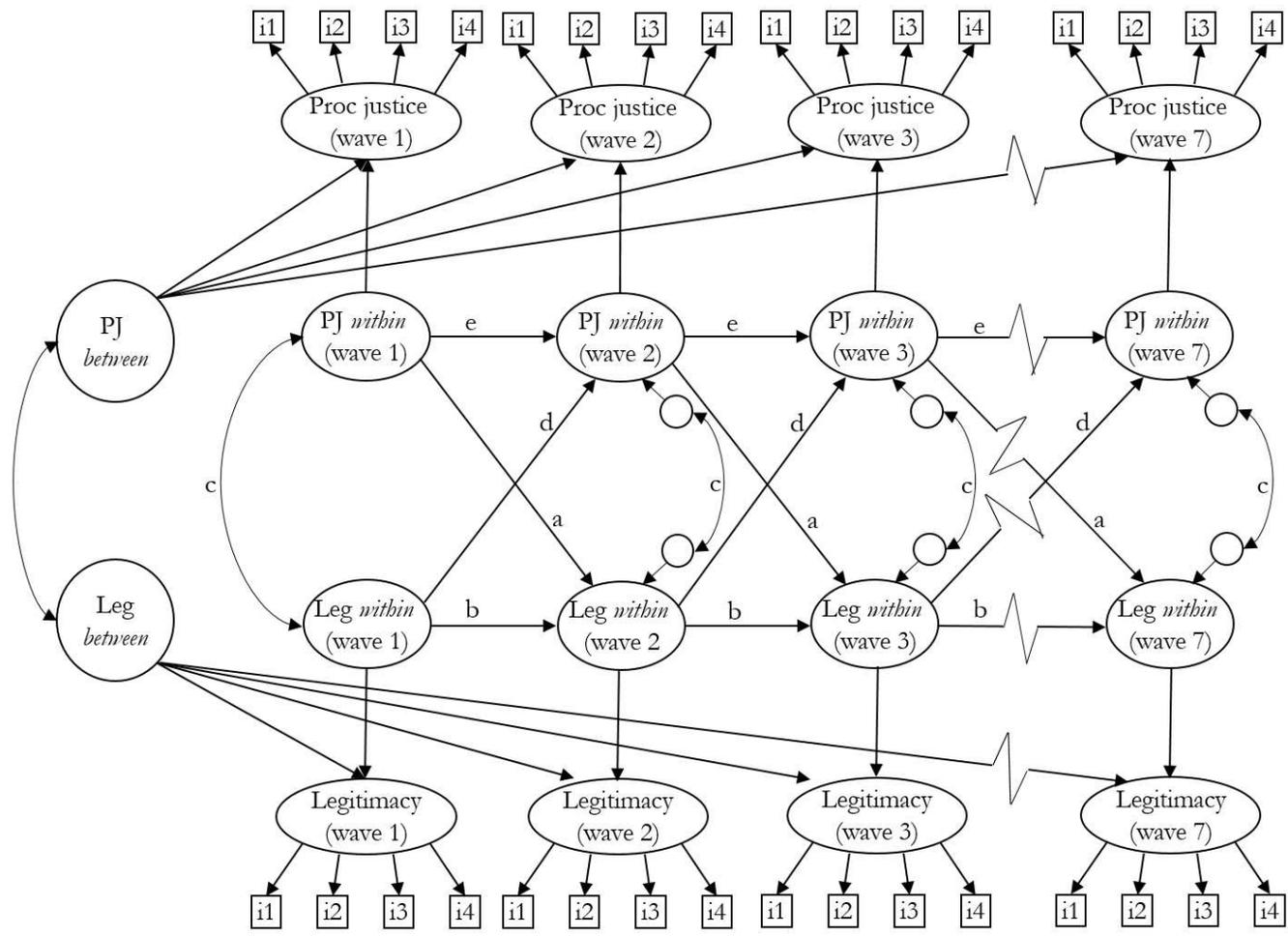
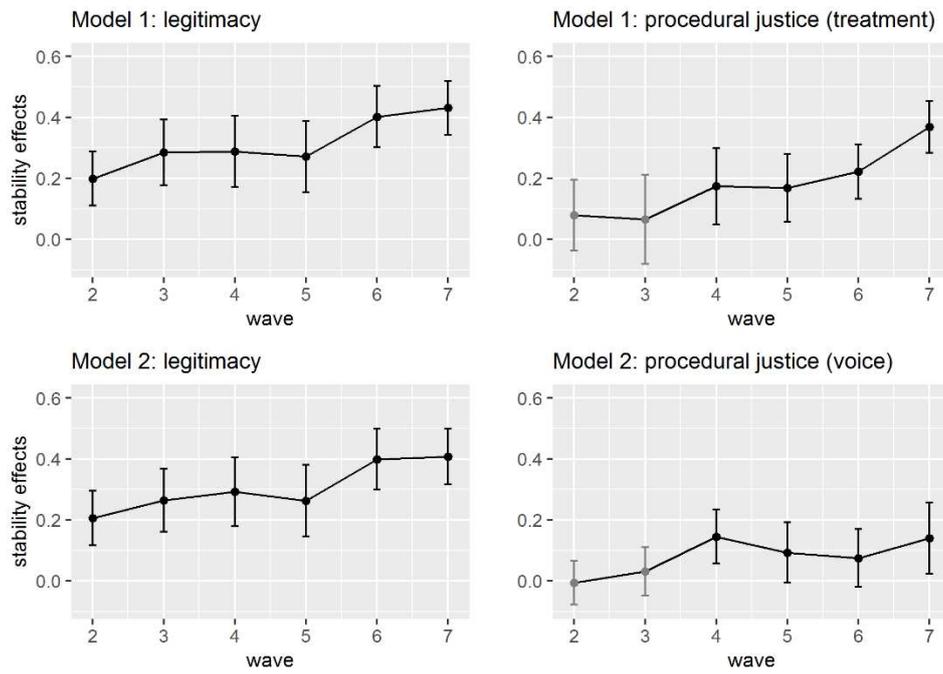


Figure 2: Stability effects from the random intercepts cross-lagged panel models



Note. Black dots represent statistically significant stability effects, grey dots represent non-significant effects.

Appendix A. Questions Used to Measure Procedural Justice and Legitimacy in the Pathways to Desistance

Table A1. Questions used to measure procedural justice

Question number	Question wording	Range of answer options	Number of categories	Reverse coded
1	'During your last contact with the police when you were accused of a crime, how much of your story did the police let you tell?'	All of it (1) – None of it (4)	4	✓
2	'The police treat me the same way they treat most people my age'	Strongly disagree (1) – Strongly agree (5)	5	
3	'Over the last couple of years, the police have been treating me the same way they always treated me in the past'	Strongly disagree (1) – Strongly agree (5)	5	
4	'During my last encounter with the police, they treated me in the way that I expected they would treat me'	Strongly disagree (1) – Strongly agree (5)	5	
5	'During my last encounter with the police, they treated me in the way that I thought I should be treated'	Strongly disagree (1) – Strongly agree (5)	5	
6	'Even after the police make a decision about arresting me, there is nothing I can do to appeal it'	Strongly disagree (1) – Strongly agree (5)	5	✓
7	'Even after the police make a decision about arresting me, someone in higher authority can listen to my case, and even in some cases, change the decision'	Strongly disagree (1) – Strongly agree (5)	5	
8	'Police considered the evidence/viewpoints in this incident fairly'	Strongly disagree (1) – Strongly agree (5)	5	
9	'Police overlooked evidence/viewpoints in this incident'	Strongly disagree (1) – Strongly agree (5)	5	✓
10	'Police were honest in the way they handled their case'	Strongly disagree (1) – Strongly agree (5)	5	

11	'Police used evidence that was fair and neutral'	Strongly disagree (1) – Strongly agree (5)	5	
12	'Police made up their mind prior to receiving any information about the case'	Strongly disagree (1) – Strongly agree (5)	5	✓
13	'Think back to the last time the police accused you of doing something wrong. Did the police treat you with respect and dignity or did they disrespect you?'	Respect/Dignity (1) – Disrespect (3)	3	✓
14	'Think back to the last time the police accused you of doing something wrong. Did the police show concern for your rights?'	Showed a lot of concern (1) – Showed no concern (4)	4	✓
15	'Of the people you know who have had a contact with the police (in terms of crime accusation), how much of their story did the police let them tell?'	All of it (1) – None of it (4)	4	✓
16	'Police treat males and females differently'	All of it (1) – None of it (4)	4	✓
17	'Police treat people differently depending how old they are'	All of it (1) – None of it (4)	4	✓
18	'Police treat people differently depending on their race/ethnic group'	All of it (1) – None of it (4)	4	✓
19	'Police treat people differently depending on the neighborhoods they are from'	All of it (1) – None of it (4)	4	✓

Table A2. Questions used to measure legitimacy

Question number	Question wording	Range of answer options	Number of categories	Reverse coded
1	'I have a great deal of respect for the police'	Strongly disagree (1) – Strongly agree (4)	4	
2	'Overall, the police are honest'	Strongly disagree (1) – Strongly agree (4)	4	
3	'I feel proud of the police'	Strongly disagree (1) – Strongly agree (4)	4	
4	'I feel people should support the police'	Strongly disagree (1) – Strongly agree (4)	4	
5	'The police should be allowed to hold a person suspected of a serious crime until they get enough evidence to charge them'	Strongly disagree (1) – Strongly agree (4)	4	
6	'The police should be allowed to stop people on the street and require them to identify themselves'	Strongly disagree (1) – Strongly agree (4)	4	
7	'The courts generally guarantee everyone a fair hearing (trial)'	Strongly disagree (1) – Strongly agree (4)	4	
8	'The basic rights of citizens are protected in the courts'	Strongly disagree (1) – Strongly agree (4)	4	
9	'Many people convicted of crimes in the courts are actually innocent'	Strongly disagree (1) – Strongly agree (4)	4	✓
10	'Overall, judges in the courts here are honest'	Strongly disagree (1) – Strongly agree (4)	4	
11	'Court decisions here are almost always fair'	Strongly disagree (1) – Strongly agree (4)	4	

Appendix B. Results Based on Mean Scores of Procedural Justice and Legitimacy

Table B1. Results from the random intercepts cross-lagged panel model based on the mean scores of procedural justice and legitimacy

	<i>Coef.</i>	<i>95% CI</i>	<i>SE</i>	<i>P value</i>
<i>Procedural justice (w2)</i>				
Procedural justice (w1)	0.05	(-0.03, 0.13)	0.04	0.20
Legitimacy (w1)	0.06	(-0.02, 0.14)	0.04	0.15
<i>Procedural justice (w3)</i>				
Procedural justice (w2)	0.10	(0.03, 0.17)	0.04	0.006
Legitimacy (w2)	0.08	(0.004, 0.16)	0.04	0.04
<i>Procedural justice (w4)</i>				
Procedural justice (w3)	0.14	(0.07, 0.20)	0.04	<0.001
Legitimacy (w3)	-0.14	(-0.23, -0.05)	0.05	0.002
<i>Procedural justice (w5)</i>				
Procedural justice (w4)	0.23	(0.16, 0.31)	0.04	<0.001
Legitimacy (w4)	-0.02	(-0.11, 0.07)	0.05	0.63
<i>Procedural justice (w6)</i>				
Procedural justice (w5)	0.24	(0.17, 0.30)	0.03	<0.001
Legitimacy (w5)	-0.04	(-0.12, 0.04)	0.04	0.28
<i>Procedural justice (w7)</i>				
Procedural justice (w6)	0.28	(0.22, 0.34)	0.03	<0.001
Legitimacy (w6)	0.08	(0.002, 0.16)	0.04	0.05
<i>Legitimacy (w2)</i>				
Procedural justice (w1)	0.08	(0.005, 0.15)	0.04	0.04
Legitimacy (w1)	0.14	(0.07, 0.22)	0.04	<0.001
<i>Legitimacy (w3)</i>				
Procedural justice (w2)	-0.08	(-0.14, -0.02)	0.03	0.01
Legitimacy (w2)	0.18	(0.11, 0.25)	0.04	<0.001
<i>Legitimacy (w4)</i>				
Procedural justice (w3)	0.05	(-0.007, 0.11)	0.03	0.09
Legitimacy (w3)	0.16	(0.08, 0.24)	0.04	<0.001
<i>Legitimacy (w5)</i>				
Procedural justice (w4)	-0.01	(-0.08, 0.05)	0.03	0.68
Legitimacy (w4)	0.19	(0.11, 0.27)	0.04	<0.001
<i>Legitimacy (w6)</i>				
Procedural justice (w5)	0.02	(-0.04, 0.07)	0.03	0.53
Legitimacy (w5)	0.31	(0.24, 0.38)	0.04	<0.001
<i>Legitimacy (w7)</i>				
Procedural justice (w6)	-0.004	(-0.06, 0.05)	0.03	0.89
Legitimacy (w6)	0.33	(0.26, 0.40)	0.04	<0.001

<i>Contemporaneous effects</i>				
Proc. just. (w1) – Legit. (w1)	0.07	(0.06, 0.09)	0.007	<0.001
Proc. just. (w2) – Legit. (w2)	0.04	(0.03, 0.06)	0.006	<0.001
Proc. just. (w3) – Legit. (w3)	0.03	(0.02, 0.04)	0.006	<0.001
Proc. just. (w4) – Legit. (w4)	0.03	(0.01, 0.04)	0.006	<0.001
Proc. just. (w5) – Legit. (w5)	0.03	(0.02, 0.04)	0.007	<0.001
Proc. just. (w6) – Legit. (w6)	0.04	(0.02, 0.05)	0.006	<0.001
Proc. just. (w7) – Legit. (w7)	0.04	(0.03, 0.06)	0.006	<0.001
<i>Random effects</i>				
Var. random int. proc. just.	0.10	(0.09, 0.11)	0.006	<0.001
Var. random int. legitimacy	0.18	(0.16, 0.19)	0.008	<0.001
Cov. proc. justice-legit	0.09	(0.08, 0.10)	0.006	<0.001
<i>Goodness of fit</i>				
RMSEA/CFI/TFI	0.04/0.98/0.97			
<i>Sample size</i>				
Participant	1354			

Note. All coefficients are standardized.