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More Direction but Less Freedom? How Task Rules Affect Intrinsic Motivation

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Task rules restrict freedom by definition, but do they necessarily harm intrinsic motivation? We examine how task rules for an open-ended writing activity affect intrinsic motivation, or enjoyment, with one's sense of direction and psychological freedom as potential mechanisms. Results from three online experiments (Experiment 1, Experiments 3a and 3b; $N = 1{,}176$), conducted with both undergraduate student and adult (Amazon MTurk and Prolific) samples, suggest that task rules may indirectly increase enjoyment by enhancing direction (indirect effect: β's range [0.09, 0.17], p's $< .05$), yet at the same time, indirectly decrease enjoyment by reducing freedom (indirect effect: β's range [−0.31, −0.07], p's $< .05$). Results from a fourth online experiment (Experiment 2; student sample; $N = 121$) address a potential alternative explanation, finding that only the task rules, not mere examples, were sufficient to increase direction (rules present: $d = 0.55$, $p = .04$; examples: $d = 0.25$, $p = .48$) and reduce freedom (rules present: $d = 0.78$, $p < .001$; examples: $d = 0.22$, $p = .31$). Theoretical and empirical connections are made to self-determination theory and flow theory. Further research is needed to delineate situational and personal factors that may moderate these effects.

Keywords: rules, intrinsic motivation, sense of direction, psychological freedom, basic psychological need theory

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Imagine you are the instructor of an introductory psychology class, and you are about to assign a paper. Should you include specific directives—for the topic, the minimum number of references, the page limit, etc.—or leave things more open-ended? Alternatively, imagine that you are responsible for cooking dinner tonight for your family. Would you appreciate a directive to use certain ingredients? Or would you rather embrace full creative freedom? In everyday life, task rules for open-ended activities like writing or cooking might have a complicated relationship to how enjoyable one finds the activity. In the present research, we aim to examine such effects of task rules on enjoyment, with a focus on potential mechanisms of influence.

Task Rules and Intrinsic Motivation

Rules are widely present in daily life. As "prescribed guide[s] for conduct or action" (Merriam-Webster, n.d.-b), they define what is expected, (in)appropriate, (il)legal, required, and prohibited. Children from a young age are able to learn, create, and enforce rules and norms (Hardecker et al., 2017; Hardecker & Tomasello, 2017), which suggests the importance of rules as tools for everyday functioning as a member of society (Asch, 1952; Chung & Rimal, 2016; Cialdini & Trost, 1998).

One might be largely concerned with the functional purpose of rules. Indeed, many rules are functional, such as following traffic laws meant to enforce safety, avoiding talk of your ex on a first date to be polite, or having an office dress code to set clear expectations for a professional appearance. Not all rules are necessary, however, and some are purely arbitrary. For instance, the rules of basketball prohibit "traveling," yet one can imagine an alternative version of the game in which players are allowed to carry the ball around without dribbling. Rules that seem to serve an important purpose in one context may seem less important, even silly, in another.

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Data, materials, and code for the study analyses, as well as preregistrations for Experiment 1a (https://osf.io/28u5s) and Experiment 1b (https://osf.io/2vdbe), can be accessed via the following link: https://osf.io/p4dqt/?view_only=f637fd1717f2430d8e6586806389eebe.

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And some cultures tend to enforce more rules and norms than others (Gelfand et al., 2011).

Functional or not, because rules are so widely encountered, it is critical to understand how and why they affect *intrinsic motivation*—engaging in an activity because it is inherently interesting and enjoyable, rather than engaging in it for some other ends (Burkley & Burkley, 2018; Deci & Ryan, 2008; Keller & Bless, 2008; Kruglanski et al., 2018). For example, a student working hard on her term paper so she can get a high grade and keep up her GPA for graduate school applications is not intrinsically motivated. In contrast, a student who is fascinated by her paper topic and finds the hours working on it happily slipping by is driven by intrinsic motivation.

Just as there are variations in how to conceive the intrinsic motivation concept (for discussion, see Keller & Landhäußer, 2012; Rheinberg, 2008), there are several established ways to operationalize intrinsic motivation (for a review, see Guay et al., 2000). Some studies, for example, have participants engage in an activity, then later give them the opportunity to continue on in that activity, but only for as long as they would like to (e.g., Deci, 1971; Koestner et al., 1984). The amount of time spent re-engaged in the activity by free choice, then, indicates greater intrinsic motivation. In other studies (e.g., Brière et al., 2021; McAuley et al., 1989; Mouratidis et al., 2011), and in the present research, intrinsic motivation is captured by self-reported interest and enjoyment in an activity, behavior, or setting (e.g., at work).

We focus on intrinsic motivation as the primary outcome of interest because it has been linked to feeling more autonomous, greater task engagement and persistence, increased performance and creativity, and even greater psychological well-being (Cerasoli et al., 2014; Deci & Ryan, 2008; Koestner et al., 1984; Kruglanski et al., 2018; Rheinberg, 2008; Ryan & Deci, 2000; Sheldon et al., 2004; van Egmond et al., 2017; cf., Gerhart & Fang, 2015). Intrinsic motivation is also an important component of personal growth, in that a person who enjoys an activity is more likely to persist in it and seek greater challenges to remain engaged (Nakamura & Csikszentmihalyi, 2014; Woolley & Fishbach, 2017). In short, intrinsic enjoyment is an optimal experience with short- and long-term benefits.

The potential value and risk of introducing task rules for an activity should therefore take into account the full picture of how they might influence intrinsic motivation. Although one might conceive of rules as typically damaging intrinsic motivation, because they impose restrictions on behavioral freedom, there is another point of view. Rules might alternatively be conceived of as supporting intrinsic motivation if one calls to mind, for instance, rules that direct enjoyable sports and games. We delve into both perspectives in the present research to develop a more nuanced understanding of how task rules may affect intrinsic motivation.

Relatedly, we are interested in how task rules affect flow experiences. The concept of *flow* (Csikszentmihalyi, 1975, 1990) is used to describe an "optimal" experience in which high challenges are met with a high skill-level (i.e., *challenge–skill balance*), one has clear proximal goals, and feedback on one's progress is readily available (for a review, see Nakamura & Csikszentmihalyi, 2014). The subjective flow state manifests in the following cluster of related experiences: focused concentration on the activity, a merging of action and awareness, a loss of self-consciousness, a sense of control over one's actions, a distortion in the experience of time passing, and experiencing the activity as intrinsically rewarding (Nakamura & Csikszentmihalyi, 2014).

Put another way, our main outcome of interest, intrinsic enjoyment, is one indicator of flow, with those in a deeper flow state finding an activity more intrinsically rewarding and enjoyable as a consequence (Melnikoff et al., 2022). In fact, one dimension of the flow experience, as measured in the Flow State Scale (FSS; Jackson & Marsh, 1996), is called *autotelic experience* and represents the degree to which an activity is intrinsically rewarding. Thus, our interest in intrinsic motivation readily extends to flow as an outcome, and we contribute to the small but growing body of literature uniting these two areas (see, e.g., Abuhamdeh, 2021; Melnikoff et al., 2022; Rheinberg, 2008).

Thus far, we have referred to task rules as a broad construct. However, we recognize that task rules can be characterized by many different attributes, and these attributes might determine their impact on intrinsic motivation. For example, task rules might apply to different content within the task, like a rule requiring a painter to use a certain color of paint versus a rule stipulating a certain painting style. And, task rules may or may not be instrumental to a certain goal. Despite this complexity, we do not systematically vary the type of task rules across our studies. Instead, we focus on the proximal and distal effects of a single type of task rules, allowing us to meet our aim of identifying sense of direction and psychological freedom as two key mechanisms through which the presence of task rules may affect intrinsic motivation. In the General Discussion, we return to this issue of generalizing our findings to the broader construct of task rules as a whole.

Mediation via Psychological Freedom

Task rules by definition reduce objective freedom of action by constraining behavioral options in completing an activity (Asch, 1952). By constraining possible courses of action, task rules might also, logically, reduce a person's subjective, *psychological freedom*,[1] that is, the experience of one's actions falling in line with one's desires and intentions, as opposed to pressured or forced action (see Kruglanski & Cohen, 1974; Reeve et al., 2003; van der Kaap-Deeder et al., 2017). With one's psychological freedom reduced by the presence of rules, one might reasonably expect a decrease in overall intrinsic motivation as a result.

Existing research related to basic psychological need theory—a "mini-theory" within self-determination theory that delineates autonomy, competence, and relatedness as universal requirements for psychological growth, development, and well-being (Ryan & Deci, 2017; Vansteenkiste et al., 2020)—addresses this potentially harmful side of rules. For example, van der Kaap-Deeder et al. (2019) studied rules in a prison setting. Prisoners who reported that the correctional officers delivered rules in a more controlling manner were more aggressive and irritated with the officers. A study with children (Koestner et al., 1984) found that instructions for a painting activity that were delivered in a controlling, rule-like manner harmed intrinsic motivation compared to instructions delivered in a more autonomy-supportive, informational style. Additional

[1] This is akin to the concept of "volition" (Reeve et al., 2003; Reeve & Jang, 2006), but we prefer the term psychological freedom, because volition is also used to refer to goal striving (e.g., Achtziger & Gollwitzer, 2010; Oettingen et al., 2016).

research has demonstrated the importance of autonomy satisfaction and frustration as predictors of intrinsic motivation and overall well-being (Chen et al., 2015; Ryan & Deci, 2000, 2017). Altogether, these results suggest that task rules should reduce psychological freedom, which is one experiential quality of autonomy (Deci & Ryan, 1987; Reeve et al., 2003), and therefore impair intrinsic motivation, at least indirectly. These findings are in line with the Rogerian perspective that instilling freedom and choice in the educational environment is critical for children's motivation to learn (Rogers & Freiberg, 1994). Imposing constraints on freedom and choice should have the opposite effect. Hence, our first hypothesis is that task rules will indirectly decrease intrinsic motivation to the extent that they reduce psychological freedom.

Mediation via Sense of Direction

Although task rules have the potential to reduce psychological freedom and, thus, to reduce intrinsic motivation (at least, indirectly), there may be circumstances in which task rules provide an indirect or even overall *benefit* to intrinsic motivation. Consider, for instance, a creative struggling with a "blank canvas" problem, such as writer's block. For such a person, the addition of task rules might provide structure or inspiration that, despite constraining behavioral avenues, unlocks intrinsic motivation. In the absence of such task rules, Schwartz's (2000, 2004) *paradox of choice* idea posits that having a vast number of behavioral avenues on the table can result in burdensome indecision. Certain choice contexts can undermine mental resources (Choi & Fishbach, 2011) and even lead individuals to delegate the choice to others (Steffel et al., 2016; Steffel & Williams, 2018). As Veenhoven (2000, p. 260) puts it, "Choice involves mostly uncertainty and responsibility. Often people shy away from this." Therefore, Schwartz (2000, p. 81) argues that the study of optimal functioning should attend to "which constraints on self-determination are the crucial ones." In this article, we investigate whether task rules could, in addition to their potential harm, be "crucial" in supporting intrinsic motivation to the extent that they strengthen a person's sense of direction.

Accordingly, one possible function of rules is to provide a *sense of direction*, which we define as the subjective feeling of having a clearly defined goal or sub-goal suited to the task at hand. Direction is a fundamental motivational construct, whether it be an organism's direction of physical movement (Hebb, 1955) or a human's more abstract aim (Kleinginna & Kleinginna, 1981) toward some end state. A person who has a direction and is aware of this—that is, has a strong sense of direction—knows what she is trying to accomplish. We propose that rules that enhance one's sense of direction should, in turn, increase one's intrinsic motivation.

The argument for task rules as capable of increasing one's sense of direction is similar to the prior argument for their detrimental effect on psychological freedom: by constraining freedom of action and limiting the scope of possibilities, task rules may facilitate solidifying the path forward. Consider, for example, how much easier it would be for a hairstylist to come up with a plan of attack for a new hairdo if the client specified wanting a shoulder-length cut with layering and a honey blonde color versus if she offered zero stipulations for her desired look. This notion is especially relevant in the context of open-ended activities—those "permitting or designed to permit spontaneous and unguided responses" (Merriam-Webster, n.d.-a)—because having a more abstract, less defined end-goal allows for many viable means of task completion, and choosing among these, in the absence of task constraints, may be difficult (Kruglanski et al., 2002, 2011; Schwartz, 2004).

With a greater sense of direction should come an increase in intrinsic motivation. This follows from a vector definition perspective of motivation (e.g., Hebb, 1955; Kleinginna & Kleinginna, 1981), in which motivation is conceptualized as a function of both energy and behavioral direction (Ferguson, 1976, via Kleinginna & Kleinginna, 1981)—ergo, more direction begets more motivation. Thus, our second hypothesis is that task rules will indirectly increase intrinsic motivation to the extent that they increase one's sense of direction.

Returning to our connections with flow theory, when task rules enhance a sense of direction for an activity, they may also facilitate a flow state, characterized by intrinsic motivation (Keller & Landhäußer, 2012; Nakamura & Csikszentmihalyi, 2014), by providing clear proximal goals. *Goal clarity*—variously described as a "condition" (Nakamura & Csikszentmihalyi, 2014), "antecedent" (Keller & Landhäußer, 2012), or "dimension" (Jackson & Marsh, 1996) of flow—is present when the task structure is well-defined and well-understood by the actor. For example, a trained surgeon has goal clarity in performing an open appendectomy, because she knows what steps need to be taken to attain the desired outcome. Were a layperson to attempt such a procedure, however, she might know that the superordinate goal is to remove the appendix but have no clue as to the more proximal aims, like separating the abdominal muscles. Note that "goal clarity" in this example could easily be replaced with "a strong sense of direction": we consider these concepts as highly similar, if not interchangeable, such that a person high in goal clarity is also high in direction. In terms of our theoretical model, then, task rules for an open-ended activity may enhance direction and goal clarity, thereby facilitating intrinsic motivation and flow.

The Present Research: Aims and Overview

We tested whether and how task rules influence intrinsic motivation using a story-writing paradigm in four experiments (Experiment 1, Experiment 2, and Experiments 3a and 3b). Specifically, we aimed to test the hypotheses that task rules both indirectly (a) decrease intrinsic motivation to the extent that they reduce psychological freedom and (b) increase intrinsic motivation to the extent that they increase one's sense of direction. In addition to the theoretical arguments detailed above, we found preliminary empirical support for these hypotheses with a Pilot Experiment reported in the online supplemental material. As these mechanism-focused hypotheses delineate opposite-direction indirect effects of task rules on intrinsic motivation, the overall effect of the rules on intrinsic motivation was not of primary interest.

We decided to test these hypotheses in the context of writing a short story because it is an individual, open-ended activity to which rules can be easily applied in an online survey-based experimental setting. Moreover, we presumed that there would be natural variability in participants' sense of direction and psychological freedom for the activity, allowing the task rules to take effect, as opposed to butting up against ceiling/floor effects. By using a single activity across the experiments, however, we must emphasize that the generalizability of our findings to different activity contexts is unproven.

Task rules were operationalized as required elements to include in one's short story. Intrinsic motivation was operationalized as self-

reported enjoyment of the writing activity (Deci et al., 1994; McAuley et al., 1989). We also used self-report survey items to measure participants' sense of direction and psychological freedom for the writing activity, prior to the manipulation (in Experiment 3) and immediately after the manipulation (in all studies).

In Experiment 1, we manipulated the presence of task rules and tested whether the rules indirectly increased enjoyment by increasing direction, yet also indirectly decreased enjoyment by reducing freedom, with a parallel mediation model. In Experiment 2, we included an additional control condition to address an alternative explanation of the observed effects of the task rules on direction and freedom. Specifically, this control condition presented participants with story elements framed as examples that they could include, rather than rules for what they must include. In Experiments 3a and 3b, we included baseline measures of sense of direction, psychological freedom, and (in Experiment 3b only) enjoyment. With these additional measures, we accounted for a potential source of bias in the mediation effects. Secondarily, we examined potential connections to flow theory (Csikszentmihalyi, 1975, 1990) in Experiment 3b with measures of goal clarity and challenge–skill balance.

Experiment 1: Proof of Concept for Opposing Forces

In Experiment 1, we establish proof of concept that the influence of task rules on intrinsic motivation may be one of opposing forces, that is, of an indirect benefit via enhanced direction with a simultaneous, indirect detriment via restricted psychological freedom. Based on the empirical Pilot Experiment findings (see the online supplemental materials) and additional theorizing, as outlined in the Introduction, we conducted Experiment 1 to test these two main hypotheses.

Method

Participants

For detailed demographic information, refer to Table S1 in the online supplemental material. Participants in Experiment 1 were adults (N for analysis = 434) recruited from MTurk, participating in exchange for financial compensation in October 2019 (M_{age} = 37.97; 55.5% men; 70.0% White).

Sample Size. We determined our desired sample size of 430 based on recommendations by Fritz and MacKinnon (2007) and the size of the smaller of the two indirect effects estimated in the Pilot Experiment (see Table S3, Multiple Mediation model, in the online supplemental material). Accordingly, we posted 450 study participation slots on Amazon TurkPrime (now called Cloud Research) and ended up with a final sample size of 434 participants (Absent: $n = 224$, Present: $n = 210$) following exclusions (see the online supplemental material for details).

Design and Procedure

All procedures were approved by the university's Institutional Review Board. Upon signing up, participants could open the online survey link and provide informed consent. At this point—unbeknownst to participants—participants were randomly assigned to one of two task rules conditions: Absent ($n = 224$) or Present ($n = 210$). In the first part of the study, all participants were informed that they would be asked "to write a story," and reported baseline measures of expectations and incentive value. Next came the experimental manipulation, in which participants were given further, condition-dependent instructions for the writing activity. Immediately after the manipulation, participants reported their sense of direction and psychological freedom, as well as follow-up measures of expectations and incentive value.

Then, participants had 3–5 min[2] to "write a short story in the text box below." On this page, we also included a reminder of the manipulation-specific instructions. Additionally, participants were told, "Don't worry about spelling or grammar," to encourage them to remain engaged in writing rather than self-editing.

After writing, participants completed two subscales from the Intrinsic Motivation Inventory: enjoyment (primary dependent measure of interest) and perceived competence. Finally, participants filled out auxiliary measures and demographic questionnaire items before the survey concluded with open-ended questions probing for suspicion, a full debriefing, and compensation with $1.00 (participation duration: median = 8.44 min; range = 4.52, 27.75).

Task Rules Manipulation

Participants in both task rules conditions were told, "Soon, you will have 5 min to write a short story." The instructions thereafter were condition-specific. Participants in the Present condition read:

> Other than the time limit, there are 2 required elements for the story: You must include (1) a required object and (2) a required line of dialogue in your story in some way.
>
> 1. Required object: an egg
> 2. Required line: "I meant to tell you a few days ago."
>
> We will display these instructions for reference when you are writing.

In this manner, rules were imposed on the story-writing activity with firm, restrictive language (e.g., "You must…"). These task rules were adapted from the 48 Hour Film Project (Required Elements @ 48 Hour Film Project), a filmmaking competition that gives teams required elements to include in their film submissions.

Participants in the Absent condition read a similar set of instructions that explicitly did not require any elements for the story: "Other than the time limit, there are no required elements for the story: You may write a story about whatever you wish with no constraints."

Measures

Manipulation Check. We included a single-item manipulation check immediately after the manipulation: "Is the statement below true or false? There are elements I am required to include in my short story" (0 = *False*, 1 = *True*).

Sense of Direction and Psychological Freedom. After the manipulation, participants were shown a matrix of survey questions "about the upcoming story-writing activity," including three about their psychological freedom (e.g., "I have a great deal of freedom"; $\alpha = 0.81$) and three about their sense of direction (e.g., "I have a

[2] We clarified for participants that they would be able to continue on to the next page after 3 min had passed and that the page would automatically advance after 5 min.

strong sense of direction"; $\alpha = 0.67$). These randomly ordered items were assessed on a seven-point scale (1 = *strongly disagree*, 7 = *strongly agree*), and we averaged them after reverse-scoring negatively worded items to create composite scores as preregistered, for descriptive and exploratory purposes.

Enjoyment. We adapted the seven-item interest/enjoyment subscale of the Intrinsic Motivation Inventory (e.g., Deci et al., 1994; McAuley et al., 1989) to refer to our story-writing activity. These items, presented in a randomized order, were assessed on a seven-point scale (1 = *not at all true*, 4 = *somewhat true*, 7 = *very true*; e.g., "While I was working on the story I was thinking about how much I enjoyed it"). We computed an enjoyment composite by reverse-scoring the negatively worded item then averaging ($\alpha = 0.94$).

Auxiliary Measures. We included the following measures for exploratory purposes: expectations and incentive value, perceived challenge, perceived competence, and story length. Full methodological details and supplementary analyses involving these variables are reported in the online supplemental material.

Transparency and Openness

We report how we determined the sample size, all data exclusions, all manipulations, and all measures, although some measurement details are included in the online supplemental material. We follow JARS (Kazak, 2018). Data, analysis code, and research materials are available (see Mutter et al., 2021; https://osf.io/p4dqt/?view_only=f637fd1717f2430d8e6586806389eebe). A summary of the measures included in each experiment is reported in Table S2 in the online supplemental material. Data were analyzed using IBM SPSS Statistics, Versions 27-28, except for structural equation modeling analyses, for which we used R, Version 4.0.4 (R Core Team, 2021) and the package *lavaan*, Version 0.6-8 (Rosseel, 2012). The design of and analyses for this study were preregistered on the Open Science Framework (https://osf.io/2vdbe).

Results and Discussion

Descriptive statistics for the key variables are reported in Table 1. Correlations among key outcomes are reported in Table 2.[3]

Manipulation Check

To test whether we successfully manipulated the presence of task rules, or "required elements," for the story, we conducted a planned binary logistic regression with the manipulation check response as the outcome and condition (dummy-coded: 0 = *Absent*, 1 = *Present*) as the sole predictor. The intercept estimate was negative and statistically significant, indicating that participants in the Absent condition were less likely to respond True ($n = 42$) than False (correct answer; $n = 182$), as expected, $b = -1.47$, $SE = 0.17$, Wald $\chi^2(1) = 73.37$, $p < .001$, $OR = 0.23$. Also as expected, the likelihood of responding True was significantly increased for participants in the Present (n False = 5, n True [correct answer] = 205) versus Absent condition, $b = 5.18$, $SE = 0.48$, Wald $\chi^2(1) = 114.58$, $p < .001$, $OR = 177.67$, 95% CI_{OR} [68.82, 458.69]. Thus, participants in the Present, but not the Absent, condition were aware that rules had been imposed on the writing activity.

Multiple Mediation Model

To determine whether the task rules condition had parallel but opposing indirect effects on enjoyment through sense of direction and freedom, we conducted a planned multiple mediation analysis. We used a structural equation modeling approach (for a comparison of this and an ordinary least squares regression approach, see Ledgerwood & Shrout, 2011) and defined psychological freedom, sense of direction, and enjoyment as latent variables, each specified by their respective scale items.

The final model, estimating the effect of task rules (0 = *Absent*, 1 = *Present*) on enjoyment, is depicted in Figure 1. This final model deviates from the pre-planned specifications in two respects, for better model fit and parsimony, respectively: First, based on modification indices (Pilot Experiment: 19.50; Experiment 1: 81.29) and an examination of the item content, we decided to slightly adjust the measurement model by allowing a residual covariance between Items 2 and 6 of the enjoyment scale.[4] These items both contain the word "interesting," so the reason for this additional relation between them is clear. Second, after noting that the direct effect of condition on enjoyment, adjusting for direction and freedom, was not statistically significant in either the Pilot Experiment (see the online supplemental material) or Experiment 1, we removed this path from the final model. In support of retaining a more parsimonious model, likelihood ratio tests comparing the fit of the models with and without the direct effect indicated that the addition of the direct effect did not significantly improve model fit; Pilot Experiment: $\chi^2(1) = 0.47$, $p = .49$; Experiment 1: $\chi^2(1) = 1.05$, $p = .31$. Importantly, neither of these post hoc changes meaningfully altered the coefficient estimates reported in Table 3.[5]

We used bootstrapped confidence intervals (CIs; n draws = 5,000) to determine statistical significance because they do not rely on the assumption of normally distributed indirect effects (MacKinnon, 2008; Shrout & Bolger, 2002). For Experiment 1, we had planned to use percentile bootstrapped CIs, but for Experiment 3b, we planned to use bias-corrected bootstrapped CIs. Given that the significance testing conclusions from both types of CI were the same in Experiment 1, we report bias-corrected bootstrapped CIs across all experiments. Fit statistics for this model are reported in Table 4, and coefficient estimates are reported in Table 3.

We can examine the indirect effects for the evidence of mediation processes (Rucker et al., 2011). As hypothesized, we observed a positive and significant indirect effect of the task rules condition on enjoyment via sense of direction. Additionally, as expected, we observed a negative and significant indirect effect of condition on enjoyment via psychological freedom. Thus, as expected, task rules indirectly enhanced enjoyment of a story-writing activity by

[3] As these correlations, and those reported in Table 5, are presented for descriptive purposes only, we did not apply a statistical correction for multiple tests. Hence, caution should be used in interpreting their statistical significance.

[4] In the final models across all experiments, the covariance between these two enjoyment items was positive and significant. This was also the case for the covariance between these items in the baseline measure for Experiment 3b.

[5] One minor exception: In the planned Experiment 1 model, the total effect was statistically significant, with a percentile bootstrapped 95% CI ranging from −0.66 to −0.006.

Table 1
Means (and Standard Deviations) by Task Rules Condition Across All Experiments

	Experiment 1		Experiment 2			Experiment 3a		Experiment 3b	
Measure	Abs	Pre	Abs	Pre	Exa	Abs	Pre	Abs	Pre
Baseline									
Sense of direction						4.21 (1.48)	4.30 (1.42)	3.98 (1.54)	3.95 (1.42)
Psychological freedom						5.22 (1.27)	5.24 (1.15)	5.01 (1.44)	5.30 (1.25)
Enjoyment								4.27 (1.59)	4.57 (1.50)
Post-manipulation									
Sense of direction	4.23 (1.46)	4.90 (1.19)	3.48[a] (1.42)	4.22[b] (1.27)	3.82[a,b] (1.30)	3.86 (1.62)	4.46 (1.31)	3.52 (1.39)	4.01 (1.41)
Psychological freedom	5.55 (1.21)	4.43 (1.49)	5.50[a] (1.07)	4.62[b] (1.07)	5.25[a] (1.23)	5.40 (1.26)	4.41 (1.41)	5.42 (1.26)	4.90 (1.32)
Autonomy satisfaction			4.81 (1.22)	4.13 (1.32)	4.81 (0.93)				
Autonomy frustration			3.29 (1.64)	3.54 (1.46)	3.53 (1.42)				
Goal clarity								3.57 (1.40)	4.15 (1.45)
Post-writing									
Enjoyment	4.74 (1.54)	4.40 (1.59)	4.63[a] (1.60)	4.37[a] (1.43)	4.41[a] (1.49)	4.80 (1.46)	4.68 (1.57)	4.50 (1.52)	4.77 (1.45)
Challenge–skill balance								3.88 (1.39)	4.08 (1.32)

Note. Abs = absent condition; Exa = examples condition; Pre = present condition.
[a,b] ANOVA follow-up comparisons for Experiment 2, as reported in the Results, are represented in the table with superscript letters. Group means that share the same letter do not differ significantly ($p \geq .05$).

giving participants a stronger sense of direction, but simultaneously indirectly diminished enjoyment by making them feel less free. Consistent with the "opposing forces" notion, we did not observe conclusive evidence for a total, overall effect of the task rules on enjoyment.

Thus, with Experiment 1, we established initial evidence for our theoretical model, in which task rules influence intrinsic motivation via two opposing indirect pathways. Sense of direction and psychological freedom are supported as psychological mechanisms for the effect of task rules. By recognizing these mechanisms, and their oppositional nature, we can better understand the psychological underpinnings of a "null effect" of task rules on intrinsic motivation—that it should not be taken at face value but rather as a potential result of competing indirect effects of rules via direction and freedom.

A limitation of our experimental design was that the two conditions differed in two respects: whether they listed possible elements to include in the story (e.g., an egg) and whether there was anything required of the participants when writing. As task rules ought to limit the objective freedom of one's possible behavioral avenues, it is important that we can attribute any effects of condition to the fact that the elements were required, not just presented. We conducted Experiment 2 to address this potential limitation.

Table 2
Correlations Among Main Outcome Variables, Experiments 1 and 2

Outcome variable	1	2	3	4	5
1. Sense of direction	—	.18			.37
2. Psychological freedom	.24	—			.41
3. Autonomy satisfaction	.38	.50	—		
4. Autonomy frustration	−.22	−.61	−.26	—	
5. Enjoyment	.32	.39	.42	−.35	—

Note. Correlations above the diagonal are from Experiment 1; those below the diagonal are from Experiment 2. All correlations are statistically significant, p's $\leq .015$.

A second limitation of Experiment 1 is that the study design does not allow us to make a firm causal inference for either the relationship between direction and enjoyment or between freedom and enjoyment. In Experiments 3a and 3b, we discuss this issue further and amend the study design to strengthen these claims, in accordance with our theoretical mediation model.

Experiment 2: Addressing the "Mere Presence" Alternative Explanation

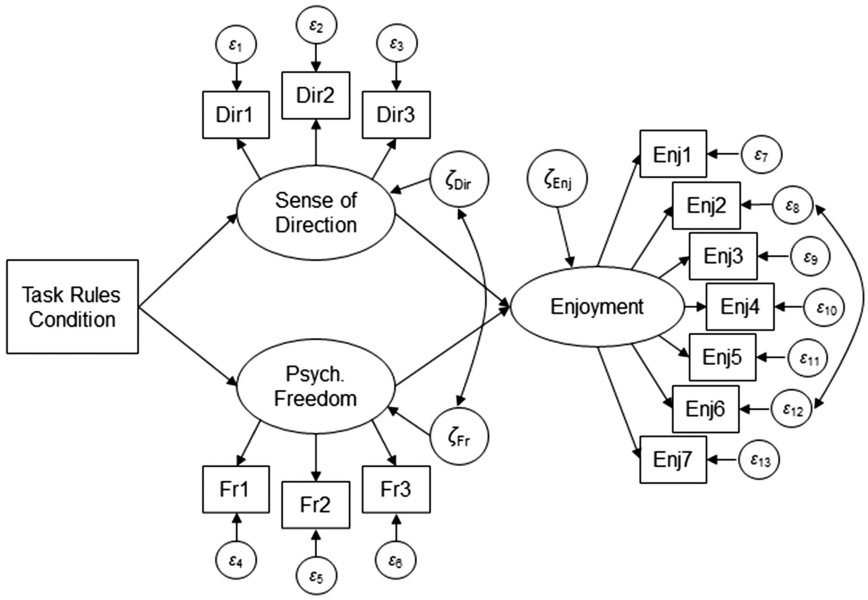
In Experiment 2, we primarily sought to address the alternative explanation that the effects of task rules might be attributable to the mere presence of the story elements (the object and line of dialogue), rather than their being required. To do so, we included an additional control condition, in which participants were given the same story elements as in the Present condition but were explicitly free to include or not include them in the story. Rather than rules or requirements, that is, the story elements were presented merely as examples.

If the key ingredient of the task rules manipulation used in Experiment 1 was the mere presence of the elements, then this Examples condition should increase sense of direction and decrease psychological freedom compared to the Absent task rules control condition. If, as we expect, however, the enforcement implied by the task rules (Asch, 1952) is responsible for producing their proximal effects on direction and freedom, then the Examples condition should be insufficient to affect direction and freedom relative to the Absent control.

As an extension of these ideas, one might also expect that the mere Examples could increase participants' sense of direction, by inspiring ideas for the story, but without having any effect on freedom, because participants can choose to follow the examples or not. If this were the case, we might observe an overall positive effect of the Examples condition on enjoyment, relative to the Absent condition. We test this as an unplanned, auxiliary aim due to our primary interest in the effects on direction and freedom.

As a secondary aim, we validated our measure of psychological freedom by comparing it to a measure of autonomy. Given the

Figure 1
Final Multiple Mediation Model, Experiment 1



Note. Latent variables are enclosed in ovals and observed variables in rectangles. Straight, single-sided arrows represent paths, and curved, double-sided arrows represent covariances. εs represent residuals for observed indicator variables after accounting for the latent factor. ζs represent disturbances (errors) for latent variables. Task rules condition is dummy-coded (0 = *Absent*, 1 = *Present*). The residuals for two enjoyment items (Enj2, Enj6) were allowed to covary based on modification indices. Dir = sense of direction; Fr = psychological freedom; Enj = enjoyment.

theoretical link between psychological freedom and autonomy (Ryan & Deci, 2006), we wanted to confirm this relationship empirically and, in doing so, ground our theoretical model in the context of self-determination theory.

Method

Participants

Participants for Experiment 2 (N for analysis = 121) were undergraduate students from a large mid-Atlantic university, recruited to participate in a brief online study for partial course credit in the fall of 2019 (M_{age} = 19.14; 69.4% women; 37.2% Asian). For more detailed demographic info, refer to Table S1 in the online supplemental material.

Sample Size. We posted 180 participation slots in October 2019, and 178 of these were filled by the semester deadline, which was our stopping rule for data collection. Following exclusions (see the online supplemental material), we obtained a final sample size for analysis of 121 participants (Absent: n = 42, Present: n = 36, Examples: n = 43). We did not conduct a formal a priori power analysis.[6] However, per a post hoc sensitivity analysis conducted in G*Power, our post-exclusions sample size was sufficient to detect an omnibus ANOVA effect size, f, of 0.286 or greater, with 80% power and an α of .05. Therefore, we were adequately powered to detect a medium sized effect (Cohen, 2013).

Design and Procedure. The Institutional Review Board-approved study procedure was the same as that of Experiment 1, except for one additional task rules condition, additional measures related to psychological freedom, and an additional measure of intrinsic motivation. Following informed consent (or assent plus parental permission), participants were randomly assigned to one of three task rules conditions: Absent, Present, or Examples. After baseline measures of expectations and incentive value, participants completed the task rules manipulation and manipulation check. Then, they reported on their sense of direction and psychological freedom as in Experiment 1. They also reported on their autonomy satisfaction and autonomy frustration regarding the story-writing activity.

Following this, participants had 3–5 min to write a short story according to their condition-specific instructions. Next, participants completed measures of intrinsic motivation: self-reported enjoyment and perceived competence as in Experiment 1, plus an exploratory behavioral measure of intrinsic motivation (presentation order randomized). Finally, participants filled out a few checks and demographic items before a full debriefing and compensation with .5 participation credits (participation duration: median = 10.53 min; range = 4.87, 336.82).

[6] With the simpler analysis needed to test our primary Experiment 2 hypotheses (i.e., one-way ANOVA), we relied on the heuristic that having at least 30 participants per cell would be sufficient (Wilson VanVoorhis & Morgan, 2007). Anticipating data exclusions as in prior experiments, we deliberately oversampled by posting 180 slots.

Table 3
Multiple Mediation Model Estimates

	Experiment 1			Experiment 3a			Experiment 3b		
Effect/Path	*b*	CI_b	β	*b*	CI_b	β	*b*	CI_b	β
Regression estimates									
Enj ~ Dir	0.46	[0.32, 0.61]	0.38	0.49	[0.39, 0.60]	0.49	0.22	[0.09, 0.35]	0.21
~Free	0.49	[0.35, 0.64]	0.39	0.37	[0.26, 0.50]	0.34	0.20	[0.05, 0.36]	0.17
~b_Enj							0.49	[0.37, 0.62]	0.44
Dir ~ Cond	0.63	[0.32, 0.96]	0.46	0.55	[0.28, 0.83]	0.35	0.58	[0.28, 0.89]	0.40
~b_Dir				0.75	[0.63, 0.87]	0.66	0.45	[0.33, 0.58]	0.45
Free ~ Cond	−1.07	[−1.37, −0.76]	−0.80	−1.15	[−1.45, −0.87]	−0.81	−0.51	[−0.82, −0.21]	−0.42
~b_Free				0.65	[0.49, 0.80]	0.49	0.28	[0.14, 0.42]	0.28
Covariances									
Dir, Free	0.74	[0.51, 0.99]	0.45	0.42	[0.20, 0.64]	0.32	0.58	[0.38, 0.80]	0.41
b_Dir, b_Free				0.84	[0.66, 1.07]	0.56	1.08	[0.84, 1.35]	0.60
b_Dir, b_Enj							1.24	[0.98, 1.54]	0.65
b_Free, b_Enj							0.93	[0.70, 1.20]	0.57
Indirect effects									
Via Dir	0.29	[0.14, 0.46]	0.17	0.27	[0.14, 0.43]	0.17	0.13	[0.04, 0.27]	0.09
Via Free	−0.52	[−0.73, −0.35]	−0.31	−0.43	[−0.59, −0.29]	−0.27	−0.10	[−0.22, −0.03]	−0.07
Total effect	−0.23	[−0.52, 0.03]	−0.14	−0.16	[−0.38, 0.06]	−0.10	0.02	[−0.14, 0.21]	0.02
R^2 estimates									
Dir	.052			.465			.245		
Free	.158			.398			.122		
Enj	.386			.456			.374		

Note. *b* = unstandardized estimate; b_ = baseline; CI_b = bootstrapped 95% confidence interval around the *b* estimate; Cond = task rules condition; Dir = sense of direction; Enj = enjoyment; Free = psychological freedom; β = partially standardized estimate, with only endogenous variables standardized.

Task Rules Manipulation

The instructions for the Present and Absent conditions were exactly the same as in Experiment 1. Participants in the Examples condition were given instructions similar to the Absent condition in that they were free to write about whatever they wished, but similar to the Present condition in that they were shown two elements that, unlike the Present condition, they were not required to include:

> Soon, you will have 5 min to write a short story. Other than the time limit, there are no required elements for the story: You may write a story about whatever you wish with no constraints.
>
> Here are 2 example elements you could include in the story if you so choose:
>
> 1. An egg
> 2. The following line: "I meant to tell you a few days ago."

Table 4
Multiple Mediation Model Fit Statistics

Statistic	Experiment 1	Experiment 3a	Experiment 3b	Experiment 3b FTM
χ^2 (*df*)[a]	459.47 (72)	617.66 (162)	533.11 (312)	768.90 (449)
CFI	0.92	0.92	0.97	0.96
TLI	0.90	0.90	0.97	0.96
RMSEA	0.11	0.09	0.05	0.05
SRMR	0.08	0.07	0.05	0.07

Note. CFI = comparative fit index; FTM = flow theory-based model; RMSEA = root-mean-square error of approximation; SRMR = standardized root mean square residual; TLI = Tucker Lewis index.
[a] χ^2 values across all experiments were statistically significant, $p < .001$.

Measures

In Experiment 2, we included the same single-item manipulation check, sense of direction measure (α = .77), and enjoyment measure (α = .96) as in Experiment 1.

Psychological Freedom and Autonomy. In addition to the three psychological freedom items used in Experiment 1, we included three new freedom items (e.g., "I am constrained") on the same measurement scale. The purpose of the new items was to explore whether the task rules simply made participants feel less free (i.e., lower in the "positive" presence of freedom) versus making them feel actively restrained (i.e., higher in a "negative" feeling of restriction; see the online supplemental material). For the main analyses reported here, we created a composite score for freedom by averaging across all six items after reverse-scoring the negatively worded ones (α = 0.89).

Moreover, we included four items on the same scale to measure autonomy satisfaction and frustration regarding the upcoming activity (satisfaction: "I feel a sense of choice and freedom in the things I will do," "I feel that my decisions will reflect what I really want"; frustration: "Most of the things I will do feel like 'I have to,'" "I feel forced to do many things I wouldn't choose to do" Chen et al., 2015). In the basic psychological need theory approach to autonomy, satisfaction and frustration are distinct experiences (Chen et al., 2015). So, we averaged the two autonomy satisfaction items, $r(119) = .43$, $p < .001$, and the two autonomy frustration items, $r(119) = .53$, $p < .001$, for separate subscales.

Auxiliary Measures. We included the following measures for exploratory or diagnostic purposes (for full methodological details and supplementary analyses, see the online supplemental material): baseline expectations and incentive value, perceived challenge, and a free choice measure of intrinsic motivation.

Transparency and Openness

We report how we determined our sample size, all data exclusions, all manipulations, and all measures in Experiment 2, although some measurement details are included in the online supplemental material. We follow JARS (Kazak, 2018). Data, analysis code, and research materials are available (see Mutter et al., 2021; https://osf.io/p4dqt/?view_only=f637fd1717f2430d8e6586806389eebe). Data were analyzed using IBM SPSS Statistics, Versions 27-28. This study was not preregistered.

Results and Discussion

Descriptive statistics for key variables are reported in Table 1. Correlations among key outcomes are reported in Table 2.

Manipulation Check

We attempted to analyze the manipulation check with a binary logistic regression predicting the likelihood of a True (vs. False) response depending on condition, as we did for Experiment 1. However, the results were uninterpretable due to one of the cells (Present condition, False response) being completely empty. Alternatively, we note that the cell counts were consistent with predictions, with far fewer True than False responses in both the Examples—n False (correct answer) = 37, n True = 6—and the Absent conditions—n False (correct answer) = 39, n True = 3—but entirely True responses in the Present condition—n False = 0, n True (correct answer) = 36. An omnibus effect of condition on the manipulation check was supported by a significant chi-square test of association, $\chi^2(2) = 86.97$, $p < .001$. These findings suggest that participants were able to distinguish not only between rules and the complete absence of rules, but also between rules and mere suggestions.

Effect of Task Rules Manipulation on Direction, Freedom, and Enjoyment

To determine whether mere examples were sufficient to increase direction and reduce freedom, relative to the Absent task rules condition, we conducted separate one-way ANOVAs predicting direction and freedom with experimental condition as the sole between-subjects factor (Absent vs. Present vs. Examples). We then followed up with pairwise comparisons, using Fisher's LSD after a significant omnibus effect and Tukey's HSD after a nonsignificant omnibus effect (Cohen, 2013).

The omnibus effect of condition on direction was marginally significant, $F(2, 118) = 2.96$, $MSE = 1.79$, $p = .056$, $\eta^2 = 0.05$. Descriptively, those in the Present condition reported a greater sense of direction for the activity than both those in the Absent condition and those in the Examples condition (see Table 1). Per pairwise comparisons (Tukey's HSD), the Present group was higher in direction than the Absent group ($d = 0.55$, $p = .04$), as in Experiment 1, but the Examples group did not differ conclusively from either the Absent group ($d = 0.25$, $p = .48$) or the Present group ($d = 0.30$, $p = .38$). Thus, only the story elements that were required, not those merely present as examples, were sufficient to significantly increase participants' sense of direction in this experiment.

The omnibus effect of condition on the psychological freedom composite was statistically significant, $F(2, 118) = 6.18$, $MSE = 1.28$, $p = .003$, $\eta^2 = 0.10$. Descriptively, those in the Present condition reported feeling less free regarding the activity than either those in the Absent condition or those in the Examples condition (see Table 1). Per pairwise comparisons (Fisher's LSD), the Present group was lower in freedom than the Absent group ($d = 0.78$, $p < .001$), as in Experiment 1, and was also lower in freedom than the Examples group ($d = 0.56$, $p = .02$). In contrast, there was no conclusive evidence that the Examples group differed from the Absent group ($d = 0.22$, $p = .31$). Thus, only the required story elements, not the examples, significantly reduced participants' psychological freedom in this experiment.

For exploratory purposes, we applied the same analytical approach to an unplanned test of the effect of task rules condition on enjoyment. The omnibus effect of condition on enjoyment was nonsignificant, $F(2, 118) = 0.34$, $MSE = 2.29$, $p = .72$, $\eta^2 = 0.006$ (for group means, see Table 1), and none of the pairwise comparisons (Tukey's HSD) were statistically significant (Present vs. Absent: $d = 0.17$, $p = .74$; Present vs. Examples: $d = 0.02$, $p = .995$; Examples vs. Absent: $d = 0.15$, $p = .78$).

Hence, we once again observed a pattern in which the presence of task rules increased participants' sense of direction and decreased their psychological freedom. The mere presence of example elements, however, influenced these variables in the same direction as the rules, but to a lesser magnitude that was not conclusive given the level of power in Experiment 2. In keeping with this pattern of findings, we did not detect a difference in enjoyment of the writing activity when participants were given examples for elements they could choose to include. This implies that the large, indirect benefit of task rules via an increased sense of direction is reliant on their being required as rules, rather than simply present as examples. However, this same requirement aspect is what considerably restricts people's freedom. The tradeoff of reduced freedom for a large increase in direction, it seems, cannot be avoided simply by framing task rules as mere examples.

Comparing Psychological Freedom and Autonomy

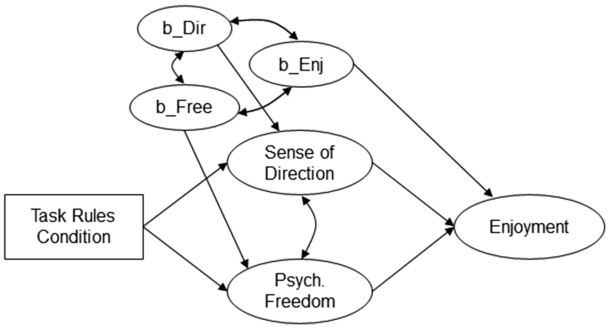
To better interpret our results through the lens of basic psychological need theory, we examined the relation between our psychological freedom measure and measures of autonomy satisfaction and frustration. We observed strong correlations such that individuals reporting greater freedom reported higher autonomy satisfaction and lower autonomy frustration (Table 2), though not so strong as to imply complete conceptual overlap (for more detailed analyses, refer to the online supplemental material).

Experiment-Set 3: Strengthening Internal Validity and Flow Theory Connections

The results of Experiments 1 and 2 suggest that task rules can have competing indirect effects on intrinsic motivation by increasing one's sense of direction and reducing one's psychological freedom, in line with our two main hypotheses. However, our model (see Figure 1 or 2) assumes that changes in task

Figure 2

Baseline-Adjusted Multiple Mediation Model, Experiment 3b



Note. Latent variables are enclosed in ovals and the observed variable, task rules condition (0 = *Absent*, 1 = *Present*), in a rectangle. Straight, single-sided arrows represent paths, and curved, double-sided arrows represent covariances. Indicator variables, residuals, and disturbances are not depicted. b_Dir = baseline sense of direction; b_Enj = baseline enjoyment; b_Free = baseline psychological freedom.

rules precede and cause changes in direction and freedom, which precede and cause changes in enjoyment (assumptions of temporal precedence and causal inference; MacKinnon, 2008, pp. 64, 67). The first half of this assumption is met because of random assignment to task rules condition. Accordingly, paths in the model that emanate from condition can be interpreted causally.

Direction and freedom, though, were measured, not manipulated. The fact that we measured the mediating variables and enjoyment sequentially—mediators before writing, and enjoyment after writing—is consistent with our theoretical model and the assumption of measurement timing (MacKinnon, 2008, p. 65). Still, we are unable to make a strong claim with the Experiment 1 design that the estimates emanating from direction and freedom capture a valid cause-and-effect relationship, as opposed to a spurious source of covariation (Shrout, 2011).

Accordingly, the primary aim of Experiments 3a and 3b is to strengthen the argument for the internal validity of our multiple mediation model by adjusting for baseline variation in sense of direction, psychological freedom, and (in Experiment 3b only) story-writing enjoyment.[7] We chose this approach over alternative strategies for testing causality assumptions in mediation models (see Preacher, 2015; Stone-Romero & Rosopa, 2011) because of our model's complexity: we have two theorized mediators that covary positively yet facilitate opposing indirect effects on enjoyment and therefore do not produce a reliable total effect.

Given this complexity, we adopted a baseline-adjustment strategy. Per this approach, we eliminate potential sources of bias in estimating the effects of direction and freedom on enjoyment by incorporating baseline measures of the outcomes as predictors in the model (Shrout, 2011). When adjusting for baseline sense of direction and psychological freedom (Experiment 3a), we can estimate how well a person's level of direction or freedom approaching the start of writing, above and beyond their baseline level, predicts their later level of enjoyment. When adjusting for baseline enjoyment in addition and allowing the three baseline measures to covary (Experiment 3b; Figure 2), trait-like tendencies for direction and enjoyment, and for freedom and enjoyment, to correlate are estimated and accounted for, thus removing this source of bias from the estimates and strengthening our causal interpretation.

Our secondary aim for Experiment-Set 3, which we addressed with Experiment 3b specifically, was to draw connections between our theoretical model and flow theory. We interpreted the enjoyment measure as one indicator of the flow experience, based on the conceptual overlap between intrinsic enjoyment and the flow concept of autotelic experience, and because of similarity between the relevant questionnaire items (Intrinsic Motivation Inventory item: "I enjoyed writing the story very much"; FSS item: "I really enjoyed the experience").

In Experiment 3b, we also included a second indicator of flow: the *challenge–skill balance* subscale of the FSS. Challenge–skill balance is most often considered an antecedent of flow (e.g., Nakamura & Csikszentmihalyi, 2014), and Rheinberg (2008) cautions against the use of challenge–skill balance as a sole indicator of the flow experience. Nonetheless, we can infer that a person who retrospectively reports a greater match between an activity's challenge level and their own skill level has experienced greater flow. In support of this view of challenge–skill balance as a flow *indicator*, Jackson and Marsh (1996, p. 29) found, in the physical activity domain, that challenge–skill balance was very strongly related to the higher-order, global flow latent factor (correlation of .88). Hence, we use this measure, along with enjoyment, to assess how the task rules influence participants' experience of flow in the writing activity.

In Experiment 3b, we also included an adapted version of the clear goals subscale of the FSS (Jackson & Marsh, 1996), concurrent with the post-manipulation sense of direction measure, to test whether it is valid to equate the two concepts. As Keller and Landhäußer (2012) argue, clear task instructions can provide goal clarity. The task rules given to Present participants may make the demands of the writing activity clearer, manifesting in greater direction and goal clarity.

With the additional goal clarity, individuals given task rules should slip more easily into a flow state. Therefore, we also explored a flow theory-based mediation model, in which task rules indirectly enhance the flow experience—as indicated by greater enjoyment and challenge–skill balance—by increasing goal clarity,[8] but may simultaneously present an indirect flow deterrent by restricting psychological freedom.

Method

Participants

We recruited participants for these online experiments through Prolific, in October and November 2020 (Experiment 3a; M_{age} = 30.82; 48.1% women; 66.6% White), and through the university participant pool, across the fall 2020 and spring 2021 semesters

[7] The baseline measure of enjoyment was unintentionally omitted from Experiment 3a.

[8] In some flow research (e.g., Jackson & Marsh, 1996), goal clarity is treated as an indicator (outcome) of flow; however, in line with other conceptualizations (e.g., Nakamura & Csikszentmihalyi, 2014), we treat goal clarity as an antecedent to flow.

(Experiment 3b; $M_{age} = 19.15$; 61.4% female; 35.1% Asian). For more detailed demographic info, refer to Table S1.

Sample Size. As for Experiment 1, we again referred to Fritz and MacKinnon (2007, Table 5) to determine our target sample size, except that we now planned to use bias-corrected bootstrapped CIs, lowering the target value to 368 for 80% power to detect the indirect effects. We rounded up slightly for a post-exclusions target *N* of 375 for both Experiment 3a and 3b. Following planned exclusions (see the online supplemental material), we obtained a final Experiment 3a sample size for analysis of 374 participants (Absent: $n = 193$, Present: $n = 181$) and a final Experiment 3b sample size for analysis of 368 participants (Absent: $n = 187$, Present: $n = 181$).

Design and Procedure

The Institutional Review Board-approved procedure for Experiments 3a and 3b was identical to that of Experiment 1, but with sense of direction and psychological freedom measured both prior to and after the manipulation. In Experiment 3a, these baseline measures were taken early on in the survey, whereas in Experiment 3b, the baseline measures were taken in a separate, prior survey that also contained a baseline measure of intrinsic motivation, demographic measures, and a battery of unrelated questionnaires. Experiment 3b contained additional items to explore the direction construct, goal interference, and connections to flow theory, as well as a few measures at the end for an unrelated survey. Median survey completion times were 8.88 min (range = 4.68, 58.07) for Experiment 3a and 16.99 min (range = 6.12, 400.40) for the Experiment 3b main survey. Participants were compensated with $1.32 (Experiment 3a) or 0.5 participation credits (Experiment 3b).

Task Rules Manipulation

The Present and Absent task rules conditions consisted of the same instructions as in Experiment 1. However, for methodological diversity, the task rules themselves were changed to the following: an envelope and the line, "Somebody has to take out the garbage" (Experiment 3a); cardboard and the line, "I have a funny feeling about this" (Experiment 3b).

Measures

In Experiment 3a and 3b, we included the same sense of direction ($\alpha_{3a} = 0.83$, $\alpha_{3b} = 0.82$), psychological freedom ($\alpha_{3a} = 0.81$, $\alpha_{3b} = 0.81$), and enjoyment ($\alpha_{3a} = 0.95$, $\alpha_{3b} = 0.94$) outcome measures as in Experiment 1. Given the conclusive manipulation check results from Experiments 1 and 2, we did not include this item in Experiment 3.

Baseline Sense of Direction and Psychological Freedom. The baseline measures of sense of direction and psychological freedom comprised the same post-manipulation items, as described in Experiment 1. However, the instructions were adapted to suit the pre-manipulation context, prompting participants to "imagine that you are asked to write a short story" and respond accordingly. Despite the fact that participants needed to make predictions about an imagined future to complete these measures, we consider them—and the baseline enjoyment measure described next—as baseline measures, because they indicate how much of these variables a person would experience when writing a short story, outside of the context of our task rules manipulation. Composite scores were created as for the post-manipulation equivalent measures (sense of direction: $\alpha_{3a} = 0.82$, $\alpha_{3b} = 0.81$; psychological freedom: $\alpha_{3a} = 0.78$, $\alpha_{3b} = 0.81$). Although these measures are incorporated in the primary analysis, we also report exploratory analyses in the online supplemental material of whether individual differences in baseline direction and freedom moderate the effects of task rules.

Baseline Enjoyment. The baseline measure of enjoyment included in Experiment 3b was similarly adapted from the original interest/enjoyment scale (Deci et al., 1994; McAuley et al., 1989), with a general instruction to "Imagine that you are asked to write a short story. For each of the following statements, please indicate how true it is for *how you expect to feel while writing a short story*." For instance, the item, "I enjoyed writing the story very much," became, "I will enjoy it very much." A composite score was created as for the corresponding post-manipulation measure ($\alpha = 0.96$).

Goal Clarity. In Experiment 3b, we also wanted to compare our sense of direction construct to the goal clarity construct from flow theory, so we included the four-item clear goals subscale of the FSS (Jackson & Marsh, 1996), adapted into present tense regarding the upcoming story-writing activity (e.g., "I know clearly what I want to do" vs. original item, "I knew clearly what I wanted to do"; $\alpha = 0.89$). The original FSS is assessed on a five-point scale of agreement, but we assessed goal clarity on the same 7-point scale of agreement as direction and freedom.

Challenge–Skill Balance. In Experiment 3b, to continue our integration with the flow literature, we included the four-item challenge–skill balance subscale of the FSS (Jackson & Marsh, 1996), adapted slightly to suit the activity context (e.g., "My abilities matched the high challenge of the story activity"; $\alpha = 0.86$). These items were presented on the same 7-point scale as the enjoyment measure (1 = *not at all true*, 4 = *somewhat true*, 7 = *very true*).

Auxiliary Measures. We included the following measures for exploratory or diagnostic purposes (for full methodological details and supplementary analyses, see the online supplemental material): expectations and incentive value (Experiments 3a and 3b), prior story-writing experience (Experiment 3b), perceived challenge (Experiment 3b), content-specific direction (Experiment 3b), goal interference (Experiment 3b), perceived competence (Experiments 3a and 3b), and whether participants finished their story (Experiments3a and 3b). Experiment 3b also included a few measures as part of pilot testing for a separate research project (for details, see the online supplemental material and research materials on the OSF).[9]

Transparency and Openness

We report how we determined our sample sizes, all data exclusions, all manipulations, and all measures in Experiments 3a and 3b, although some measurement details are included in the online supplemental material. We follow JARS (Kazak, 2018). Data, analysis code, and research materials are

[9] Please note that we are at an increased risk of experiment-wise Type I error in running and reporting numerous auxiliary analyses. Hence, supplementary results involving these auxiliary measures should be interpreted as informative for future research rather than as conclusive tests.

available (see Mutter et al., 2021; https://osf.io/p4dqt/?view_only=f637fd1717f2430d8e6586806389eebe). Data were analyzed using IBM SPSS Statistics, Versions 27-28, except for structural equation modeling analyses, for which we used R, Version 4.0.4 (R Core Team, 2021) and the package *lavaan*, Version 0.6-8 (Rosseel, 2012). These studies were not preregistered.

Results and Discussion

Descriptive statistics for key variables are reported in Table 1. Correlations among key outcomes are reported in Table 5.

Baseline-Adjusted Multiple Mediation Model

For our primary analysis, we estimated the same multiple mediation model as in Experiment 1 but with the following changes: baseline measures of sense of direction, psychological freedom, and—in Experiment 3b only—enjoyment were included as latent variable predictors in the regression equation predicting their corresponding outcome measure, and these baseline variables were allowed to covary (see Figure 2). As (exploratory) Experiment 1 results pointed to including a residual covariance between the 2nd and 6th enjoyment items, we included this in the Experiment 3a and 3b models too. These are complete mediation models (again, based on Experiment 1 results), but we also tested whether the addition of a direct effect of condition on enjoyment significantly improved model fit with likelihood ratio tests. This was not the case for Experiment 3a, $\chi^2(1) = 0.02$, $p = .90$, but the test was marginally significant for Experiment 3b, $\chi^2(1) = 3.17$, $p = .08$. So, we report estimates for the Experiment 3b model including a direct effect in Tables S4 and S5 in the online supplemental material. None of the estimates are meaningfully different from those reported in Table 3.

As planned, we used bias-corrected bootstrapped CIs (n draws = 5,000) to determine statistical significance. Fit statistics are reported in Table 4, and coefficient estimates are reported in Table 3. The Experiment 3a model used all 374 observations. The Experiment 3b model used 340 of 368 total observations, because of missing data on the baseline measures.

Findings were consistent across both Experiments 3a and 3b. Each baseline measure was positively associated with its respective outcome measure, and these baseline measures co-varied positively with one another. Adjusting for the baseline measures, both indirect effects remained significant and in the predicted direction. The total effect of condition on enjoyment was nonsignificant. Thus, even accounting for baseline direction, freedom, and enjoyment—and their interrelations—we found evidence that task rules can indirectly increase intrinsic motivation via sense of direction but also indirectly decrease intrinsic motivation via (reduced) psychological freedom. With the Experiment 3b findings in particular, we have support for the internal validity of the causal mediation model.

Table 5
Correlations Among Main Outcome Variables, Experiment 3

Outcome variable	1	2	3	4	5
1. Sense of direction	—	.22		.51	
2. Psychological freedom	.31	—		.40	
3. Goal clarity	.85	.27	—		
4. Enjoyment	.39	.25	.43	—	
5. Challenge–skill balance	.36	.18	.36	.51	—

Note. Correlations above the diagonal are from Experiment 3a; those below the diagonal are from Experiment 3b. All correlations are statistically significant, p's $\leq .001$.

Flow Theory Integration

We first compared our sense of direction measure to one of the flow theory concept of goal clarity, and then, we constructed an exploratory mediation model informed by flow theory.

Comparing Direction and Goal Clarity. In Experiment 3b, sense of direction at baseline was similarly predictive of post-manipulation sense of direction, $r(342) = .41$, $p < .001$, and goal clarity, $r(342) = .36$, $p < .001$. The latter two measures, direction and clarity, were very strongly related to one another and were similarly related to enjoyment (see Table 5), consistent with the notion that these are alternative indicators of the same underlying construct.

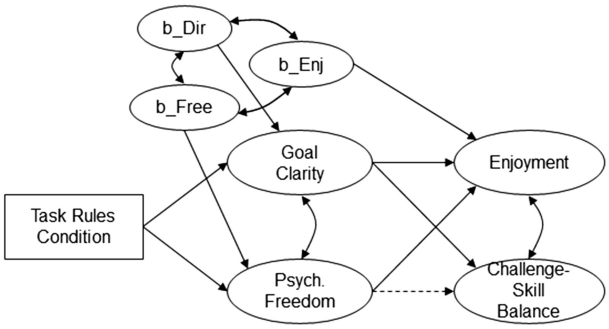
We also examined whether the effect of condition was the same whether the outcome variable was sense of direction or goal clarity with a mixed ANOVA, adjusting for mean-centered baseline direction. We observed a main effect of condition, $F(1, 341) = 21.48$, $MSE = 3.04$, $p < .001$, $\eta_p^2 = 0.06$, with higher average scores in the Present (vs. Absent) condition. There was also a main effect of measure, $F(1, 341) = 6.49$, $MSE = 0.30$, $p = .01$, $\eta_p^2 = 0.02$, with higher average scores on the goal clarity measure than the sense of direction measure. However, there was no evidence for a condition-by-measure interaction effect, $F(1, 341) = 0.39$, $p = .53$, $\eta_p^2 = 0.001$, indicating that the task rules increased participants' sense of direction and goal clarity alike. Altogether, these results support the construct validity of our sense of direction measure by demonstrating its close similarity to a flow theory-based measure of goal clarity. In short, participants with a stronger sense of direction were more clear about their goals for the writing activity.

Flow Theory-Based Mediation Model. To determine whether the task rules influenced participants' experience of flow in the story-writing activity, we estimated the same baseline-adjusted multiple mediation model as for the primary analysis, but with goal clarity in place of sense of direction, and with challenge–skill balance as an additional distal outcome alongside enjoyment (which was included as an indicator of autotelic experience, or intrinsic reward). Figure 3 depicts the model, Table 4 contains the fit statistics, and Table 6 reports the estimates (n bootstrap draws = 5,000). Like the primary Experiment 3b model, this model was estimated with 340 of the 368 total observations.

Every path estimated was statistically significant except for the effect of freedom on challenge–skill balance. In addition to replicating the indirect effects of the task rules condition on enjoyment via goal clarity (as a conceptual replication of the results with direction) and via psychological freedom, we also observed an indirect effect of condition on challenge–skill balance via goal clarity. Specifically, participants in the Present (vs. Absent) condition were more clear about the task goals and, in turn, reported a greater balance between the challenge of the activity and the skills they brought to it.

These results demonstrate how task rules may actually, in some circumstances, help individuals get into a flow state. To the extent that the rules improve goal clarity, individuals should slip into a deeper state of flow, as indicated by their feelings of intrinsic reward and by their experience of challenge–skill balance. However, the

Figure 3
Flow Theory-Based Model, Experiment 3b



Note. Latent variables are enclosed in ovals and the observed variable, task rules condition (0 = Absent, 1 = Present), in a rectangle. Straight, single-sided arrows represent paths, and curved, double-sided arrows represent covariances. The dashed path is not statistically significant. Indicator variables, residuals, and disturbances are not depicted. b_Dir = baseline sense of direction; b_Enj = baseline enjoyment; b_Free = baseline psychological freedom.

Table 6
Flow Theory-Based Mediation Model Estimates, Experiment 3b

Effect/Path	*b*	CI_b	β
Regression estimates			
Enj ~ GClar	0.27	[0.15, 0.37]	0.27
~Free	0.21	[0.07, 0.35]	0.18
~b_Enj	0.42	[0.31, 0.55]	0.39
CSB ~ GClar	0.24	[0.14, 0.35]	0.36
~Free	0.08	[−0.05, 0.21]	0.10
GClar ~ Cond	0.61	[0.31, 0.92]	0.41
~b_Dir	0.43	[0.32, 0.55]	0.43
Free ~ Cond	−0.51	[−0.83, −0.21]	−0.42
~b_Free	0.28	[0.15, 0.44]	0.29
Covariances			
GClar, Free	0.51	[0.30, 0.72]	0.35
b_Dir, b_Free	1.08	[0.84, 1.35]	0.59
b_Dir, b_Enj	1.24	[1.00, 1.55]	0.65
b_Free, b_Enj	0.93	[0.70, 1.21]	0.57
Enj, CSB	0.48	[0.32, 0.70]	0.46
Indirect effects			
On Enj via GClar	0.16	[0.07, 0.29]	0.11
On CSB via GClar	0.15	[0.07, 0.26]	0.15
Total via GClar	0.31	[0.15, 0.53]	0.26
On Enj via Free	−0.11	[−0.22, −0.04]	−0.08
On CSB via free	−0.04	[−0.12, 0.02]	−0.04
Total via free	−0.15	[−0.32, −0.04]	−0.11
Total on Enj	0.05	[−0.10, 0.22]	0.04
Total on CSB	0.11	[−0.02, 0.23]	0.11
Total effect	0.16	[−0.10, 0.43]	0.15
R^2 estimates			
GClar		.225	
Free		.127	
Enj		.366	
CSB		.161	

Note. *b* = unstandardized estimate; b_ = baseline; CI_b = bias-corrected bootstrapped 95% confidence interval around the *b* estimate; Cond = task rules condition; CSB = challenge–skill balance; Dir = sense of direction; Enj = enjoyment; Free = psychological freedom; GClar = goal clarity; β = partially standardized estimate, with only endogenous variables standardized.

potential for rules to infringe on psychological freedom should not be discounted: taking this opposing pathway into account, the total effects of the task rules condition on both enjoyment and challenge–skill balance were nonsignificant. In other words, as much as task rules may indirectly enhance flow via goal clarity, they may also indirectly interfere with flow via diminished psychological freedom, especially when it comes to the intrinsic reward component of the flow experience.

General Discussion

Together, these studies delineate how and why task rules and other external constraints on freedom might influence intrinsic motivation. Our research points to a sense of direction (or goal clarity) and a subjective psychological freedom (related to autonomy) as important mechanisms to consider. Across four experiments and in accordance with our hypotheses, we provide evidence that requiring individuals to include certain elements in a story-writing activity simultaneously strengthens their sense of direction and restricts their psychological freedom, in turn leading to greater and less enjoyment, respectively. Our data were consistent with this parallel mediation model even when baseline covariance between the mediators and enjoyment was accounted for in Experiment 3b, further strengthening the case for a causal process. Thus, task rules may be a double-edged sword, in part favoring and in part harming intrinsic motivation, as they move the levers of direction and psychological freedom.

Theoretical Contribution

We next consider how these findings contribute to our understanding of basic psychological need theory and flow theory.

Basic Psychological Need Theory

Under basic psychological need theory, any rule or constraint that diminishes or threatens the need for autonomy should damage intrinsic motivation (Koestner et al., 1984; Ryan & Deci, 2017). The present research supports yet complicates this idea. In line with the prior research (see also Rogers & Freiberg, 1994), we found that the presence of rules indeed diminished intrinsic motivation indirectly, by restricting participants' psychological freedom (which was highly related to their autonomy). Yet, even these controlling rules ("You must…") delivered without a reason had a net neutral effect on intrinsic motivation because they also provided participants with an increased sense of direction.

Future research might further explore connections to basic psychological need theory by testing whether the effect of direction on intrinsic motivation could be explained by perceived competence, another basic psychological need characterized by feeling capable and effective in one's actions (Ryan & Deci, 2017; Vansteenkiste et al., 2020). Specifically, we might expect that the increase in direction from the rules would instill participants with a greater sense of their own competence, which, in turn, increases their intrinsic motivation. Although our data are largely consistent with this notion (see the online supplemental material), these studies were not specifically designed to test a sequential mediation model. Nonetheless, this is a promising avenue for future research to identify circumstances that influence intrinsic motivation via two basic

psychological needs, but in opposite directions (i.e., less autonomy but more competence).

Our research also extends work on autonomy support and structure in working with children or other educational settings. Ginott (1959) argues for setting limits in child play therapy, with good reason and in a non-punitive manner. Reeve and Jang (2006) identify several specific instructional behaviors that are autonomy supportive, like providing rationales, and several others that detract from autonomy, like asking controlling questions. Jang et al. (2010) found that autonomy support and structure complemented one another when it came to fostering students' engagement in learning, rather than being at odds with one another. This body of research implies that it is not the mere presence of rules, structure, or other limitations that matters. Rather, it is the impact of these constraints on the recipient that matters, especially the impact on their autonomy.

Our findings complicate this picture by reinforcing the notion that freedom-limiting rules indirectly harm intrinsic motivation, but those same freedom-limiting rules can simultaneously provide helpful structure in the form of a stronger sense of direction. Although we did not directly test this, our research is consistent with the notion that some task rules, for some individuals at least, may outright increase enjoyment if they are both autonomy-supportive and direction-facilitating. As Asch points out (1952, p. 352), "Rules are constraints, but there is a clear difference between constraints that we adopt because they serve a purpose we endorse and those that we must follow because of coercion." Thus, rules that provide purpose (i.e., direction) and are delivered in a non-coercive, autonomy supportive manner may be optimal in supporting intrinsic motivation, at least when compared to rules delivered without autonomy support. Beyond the objective qualities of the task rules, this same reasoning could be applied to individuals' interpretation of the task rules: A person who perceives a set of rules as nonthreatening to their psychological freedom should stand to gain the most from the direction they provide.

However, even if task rules and other constraints can provide enjoyable direction does not mean that this externally induced direction is preferable to a sense of direction that comes from within. Finding one's own sense of direction—that is, self-direction or self-determination (Ryan & Deci, 2017)—in personally valued goals may provide a more robust source of motivation than task rules (Koestner et al., 2008). Mental contrasting (e.g., Oettingen, 2012) is a goal-setting and goal pursuit strategy that may help individuals find self-direction, by contemplating their most important wishes and what could stand in the way of attaining them.

Flow Theory

Our findings in Experiment 3b build upon the flow theory literature by further integrating it with research on intrinsic motivation, and by elaborating the connection between task rules and flow experiences. We provided evidence that our sense of direction construct is virtually indistinguishable from the flow theory-based goal clarity construct (see Jackson & Marsh, 1996; Keller & Landhäußer, 2012; Nakamura & Csikszentmihalyi, 2014) and that both were increased by the presence of task rules. Therefore, with rules that provided a greater sense of direction for what to write in their stories, participants were also clearer on what their specific goals for the activity were (see target vs. purpose goals distinction in Harackiewicz et al., 1998; see also, Earley et al., 1987).

This increased clarity from the rules, in turn, manifested in a greater flow experience, as indicated by challenge–skill balance and intrinsic reward. We reason that those with greater goal clarity experienced greater challenge–skill balance, specifically, because they were better able to gauge how they were doing during the activity and use that feedback to adjust for a more engaging balance between challenge and skill (Nakamura & Csikszentmihalyi, 2014). Alternatively, goal clarity may have facilitated participants' ability to form accurate challenge and skill evaluations (Keller & Landhäußer, 2012), with higher scale endorsement as a byproduct.

Thus, our findings shed light on why flow experiences often occur in the context of sports and games (Cowley et al., 2008; Jackson, 1996; Jackson & Csikszentmihalyi, 1999; see also, Ryan et al., 2006), which are inherently rule-based (Cowley et al., 2008; Suits, 1967): the presence of rules in such active leisure activities can be a flow antecedent because of this goal clarifying function. According to Keller and Landhäußer's (2012) revisited flow model, the presence of rules likely also enabled participants to garner more useful feedback on their performance, thinking, for example, that they are doing well because they have incorporated both required elements into their story, as opposed to the Absent condition participants who had no such basis for judging their story-writing performance in the moment. Notably, however, the presence of task rules might also be a flow deterrent, to the extent that they are psychologically restrictive.

There is also a rich literature on flow experiences in writing and other creative endeavors (Csikszentmihalyi, 2013; Perry, 2009). Our experiments, conducted within a creative writing paradigm, directly address how the introduction of rules into such an activity may affect the flow experience, for better and for worse, in line with prior work finding that task constraints can be both beneficial and harmful to creativity. For example, Mehta and Zhu (2016) found that making resource scarcity salient and thus activating a "constraint mindset" reduced functional fixedness in participants trying to come up with new product uses. A study from Medeiros et al. (2014) paints a complex picture of constraints and creativity, in which motivation via need for cognition, the nature, and the number of constraints imposed must be considered. Future research might examine creativity as an outcome, testing whether the same pathways of influence from the presence of task rules onto enjoyment and flow also operate for creativity.

Limitations and Future Directions

Scholars studying the consequences of default choice options have remarked that, lacking perfect insight into what factors drive or alter default effects, researchers make inadvertent decisions to include or exclude certain factors (Jachimowicz et al., 2019; Zlatev et al., 2017). This is undoubtedly the case in our study of how task rules affect intrinsic motivation. We used only one specific type of task rules—required elements to include—in a specific context—writing a short story—and therefore do not know the exact characteristics of the rules that were responsible for their influence on a sense of direction, psychological freedom, and task enjoyment.

These inadvertent decisions we made in operationalizing task rules are certainly a limitation of our research, but they also point to avenues of future research. One pressing question is whether the opposing indirect effects of task rules on intrinsic motivation via sense of direction and psychological freedom generalize to

task rules of a different nature and generalize to a different context. For example, one could vary the instrumentality of the rules—whether they are seen as facilitating, thwarting, or irrelevant to the broader activity goal—and test whether this alters their effects. Regarding contextual influences, for example, one might examine whether rules provide a greater direction-mediated benefit to enjoyment when applied to an initially daunting activity, compared to an easier, more familiar one (for exploratory analyses of baseline task enjoyment and relevant experience as potential moderators of the overall effect of rules on enjoyment, refer to the online supplemental material). One might also examine the importance of when rules are delivered—for example, in the earliest predecisional phase of goal pursuit, compared to the preactional or actional phase (see Gollwitzer & Keller, 2016).

Furthermore, we can better anticipate conditions that might produce an overall benefit or detriment of rules. For example, task rules that are unlikely to provide a boost in direction may diminish intrinsic motivation on the whole. And those that are supportive of psychological freedom (see Reeve & Jang, 2006; van der Kaap-Deeder et al., 2019) may strengthen intrinsic motivation. Even simply providing a reason for the rules might ameliorate the indirect decrease in enjoyment due to restricted freedom (Koestner et al., 1984). Relatedly, one might design an experiment to test whether rules are beneficial if their content is freely chosen. Participants could be divided into three different groups: one that has rules imposed but can select which rules apply to them (e.g., whether they want to have a required line of dialogue vs. a required object for their story); a second group that has rules without a choice—a yoked design may be fruitful here; and a final group with no rules. One might expect the first, chosen rules group to have greater direction than the no rules control group but just as much freedom and, consequently, greater overall intrinsic motivation.

So, not only are direction and freedom psychological mechanisms by which task rules may influence intrinsic motivation, but they are also qualities that might characterize a set of task rules to a greater or lesser extent. By attending to these qualities, one can better predict how a set of rules will affect intrinsic motivation. Even simply drawing attention to either of these qualities may make a difference to the overall effect of rules on intrinsic motivation. Future research could test an experiment in which the same rules are applied, but participants' attention is drawn to either their freedom-impinging or direction-boosting qualities. If such a manipulation effectively strengthens the effect of rules on freedom or direction, respectively, then we would expect the first group to experience an overall reduction, and the second group an overall increase, in intrinsic motivation.

In another test of which characteristics of the task rules are critical to their consequences observed here, one could manipulate whether the task rules are "banned" versus "required" elements. Research on approach versus avoidance motivation (Elliot & Church, 1997; see also, Higgins, 2012; Janoff-Bulman et al., 2009) suggests that this framing may be an important factor. Might "banned" elements still reduce individuals' psychological freedom without inspiring a stronger sense of direction? It would also be informative to know whether rules imposed on the process, or means, of completing the task versus rules imposed on the task outcome, or goal, have different effects (see Kruglanski et al., 2002; Pham & Taylor, 1999).

Similarly, future research is needed to identify critical characteristics of the activity itself. We chose a writing activity because it is highly open-ended, with infinitely many ways to achieve the same broad end of writing a short story, and no consequences for breaking the rules. Would we observe similar patterns of effects in a more structured activity, such as solving a puzzle, or a higher-stakes activity, such as painting a commissioned work of art? And would we expect the mediated effects of the rules, via both direction and freedom, to persist when revisiting a similar activity later on?

It is also possible that the source of the rules—who communicates and enforces them—affects how the rules influence intrinsic motivation. As significant others, like close friends or romantic partners, are more central to one's sense of self than strangers (Aron et al., 1991), rules imposed by significant others may be less damaging to autonomy. On the other hand, rules from a close other may backfire if they contradict an expectation that the relationship members will support each other's autonomy and not try to control them.

We are also intrigued by the prospect that this pattern of opposing influence might play out in the large-scale endeavor of building a life. Could a preponderance of rule-like constraints on a person's life path development—such as parental strictness, cultural and religious mandates, or limited resources—influence life satisfaction via purpose in life (Ryff, 1989) and autonomy satisfaction (Chen et al., 2015)? We would expect that constraints on identity development and major life decisions might indirectly improve life satisfaction to the extent that they instill a greater sense of purpose, but might at the same time indirectly detract from life satisfaction by stifling autonomy. Following from this, we reason that individuals who lack a sense of purpose might be more attracted to and satisfied with rigid, rule-bound circumstances that stand to shape their path forward and save them from the responsibility of choosing for themselves.

Finally, future research should examine the possibility of a direction-freedom tradeoff, with these variables, treated separately in the present research, considered on a joint continuum. Importantly, we observed that participants' sense of direction was positively related to their psychological freedom, in terms of zero-order correlations that were weak-to-moderate in magnitude, and in terms of covariances within the multiple mediation model. However, it is possible to conceive that in a naturalistic setting, having a stronger sense of direction might co-occur with reduced psychological freedom because the same narrowing of behavioral possibilities that produces the sense of direction ("I know what it is that I intend to do") precludes the freedom of those unselected possibilities ("I will not be doing anything other than what I intend"). This calls to mind the idea of an optimal balance, wherein a person has enough direction to spur them onward but not so much that they feel restricted in their granular decision-making. For example, having conversation topics in mind when catching up with a friend, like their recent engagement or new job, shapes and enlivens the experience, but few would enjoy a stricter conversation script, in which facets X, Y, and Z of the friend's new job must be discussed for exactly 6 min.

Conclusion

The present research extends existing knowledge of the psychological consequences of rules and has the potential to inform the decision-making of anyone in a position to impose constraints on others. We found consistently that the presence of task rules for a story-writing activity indirectly increased participants' intrinsic motivation by increasing their sense of direction, yet also indirectly decreased their intrinsic motivation by restricting their psychological freedom.

Returning to our opening example of assigning an introductory psychology course paper, in deciding whether to include certain requirements, like a specified topic or a page limit, you can consider how these requirements might affect students' sense of direction and psychological freedom for the assignment. You might settle on giving them a choice between two topics and suggesting 10 pages as a length guideline (not requirement). This way, students can be inspired by the topic(s) and have a sense of whether they are on the right track in terms of the scope of the paper, with feedback based on the page number guideline. However, they retain autonomy in choosing their preferred topic and are unhampered by a strict and somewhat arbitrary page limit. That said, additional research on the motivational dynamics of task rules is needed to further refine our expectations for how people will respond to this kind of everyday situational constraint.

Context

Prior research by Gabriele Oettingen and Peter Gollwitzer has examined motivational and self-regulatory processes, consequences, and strategies. Elizabeth Mutter, their advisee, developed a specific interest in the motivational consequences of task rules after noticing that her personal experience with rules as fun, during improvisational comedy short-form games (think, *Whose Line Is It Anyway?*), seemed to contradict the self-determination theory perspective of external constraints as detrimental. Barry Schwartz's 2004 book, *The Paradox of Choice: Why More Is Less* was also an early source of inspiration. After initial pilot studies with an overall null effect of task rules, the authors realized that a focus on process—*how*, not just whether, the rules influenced motivation—was necessary. The Pilot Experiment helped identify two promising mechanisms: sense of direction and psychological freedom. In our view, this article contributes to a more nuanced understanding of why task rules encountered in everyday life may be fun in some instances, yet detrimental in others.

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