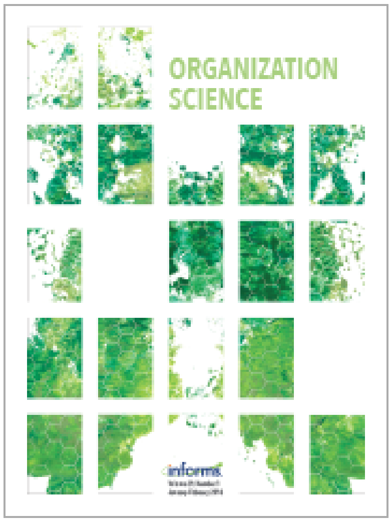

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Iterative Coordination and Innovation: Prioritizing Value over Novelty

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Abstract. An innovating organization faces the challenge of how to prioritize distinct goals of novelty and value, both of which underlie innovation. Popular practitioner frameworks like Agile management suggest that organizations can adopt an iterative approach of frequent meetings to prioritize between these goals, a practice we refer to as *iterative coordination*. Despite iterative coordination's widespread use in innovation management, its effects on novelty and value in innovation remain unknown. With the information technology firm Google, we embed a field experiment within a hackathon software development competition to identify the effect of iterative coordination on innovation. We find that iterative coordination causes firms to *implicitly* prioritize value in innovation: Although iteratively coordinating firms develop more valuable products, these products are simultaneously less novel. Furthermore, by tracking software code, we find that iteratively coordinating firms favor integration at the cost of knowledge-creating specialization. A follow-on laboratory study documents that increasing the frequency and opportunities to reprioritize goals in iterative coordination meetings reinforces value and integration, while reducing novelty and specialization. This article offers three key contributions: highlighting how processes to prioritize among multiple performance goals may implicitly favor certain outcomes; introducing a new empirical methodology of software code version tracking for measuring the innovation process; and leveraging the emergent phenomenon of hackathons to study new methods of organizing.

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1. Introduction

Organizations often face the challenge of simultaneously pursuing multiple performance goals (Cyert and March 1963, Gavetti et al. 2012). For instance, airlines simultaneously strive for safety and profitability (Gaba and Greve 2019), while manufacturing firms seek to concurrently decrease costs and increase revenues (Obloj and Sengul 2020). Often, progress made in pursuit of one goal may inadvertently undermine performance toward other goals (Hu and Bettis 2018). This challenge applies broadly to organizations—even where individuals do not have conflicting preferences per se (Ethiraj and Levinthal 2009)—because an organization's multiple goals do not perfectly correlate with one another (Simon 1964). To help manage the pursuit of multiple goals, organizations can prioritize their most important subset of goals first before addressing those of lesser importance (Ethiraj and Levinthal 2009, Unsworth et al. 2014). Nonetheless, how organizations manage the pursuit of multiple goals for which there is no clear ex ante prioritization available remains unclear in existing organizational research (Greve and Gaba 2020).

One situation where organizations must manage multiple simultaneous goals with no clear, established prioritization among them is the pursuit of innovation. Scholars across literatures conceptualize innovation as the simultaneous pursuit of novelty and value (Amabile 1983, Singh and Fleming 2010, Kaplan and Vakili 2015). Despite novelty and value being distinct dimensions of innovation performance, prior work simplifies innovation to a singular dimension by implicitly assuming that the two dimensions of novelty and value travel with one another (Oldham and Cummings 1996, Shalley and Perry-Smith 2001). Nonetheless, more recent literature suggests that novelty and value may diverge in practice (Berg 2014). For instance, a mobile application that translates words from an alien language to English may be novel, but may offer little value, whereas a simple mobile-payments application may be quite valuable to customers, albeit not entirely novel. Thus, we take the view that the process of innovation may be better conceptualized as the pursuit of the distinct goals of novelty and value. Yet, because an innovating organization must achieve both novelty and value, it is difficult to ex ante prioritize between the two.[1] For instance, for a firm striving to develop an innovative mobile application, does the firm develop an application of high value that would be of known interest to customers leading to its purchase, or does it instead focus on novelty to help differentiate their application in a crowded market? In short, it is unclear how an organization would prioritize between these two distinct goals. This challenge raises the question of which techniques an organization may use to prioritize among the underlying distinct goals of novelty and value when striving for innovation as an outcome.

Practitioners of popular management frameworks, such as Agile management, prescribe an iterative approach of frequent meetings to prioritize among multiple goals in innovation (Sutherland and Sutherland 2014, Rigby et al. 2016a, Bernstein et al. 2019), a practice we refer to as *iterative coordination*. In contrast to traditional innovation management practices, which emphasize extensive ex ante planning to manage multiple competing objectives, Agile broadly encompasses a set of management practices defined by an "iterative approach [that] makes it easier to keep projects aligned" (Relihan 2018, p. 1). As part of this iterative approach, Agile practitioners and gurus prescribe the use of iterative coordination in the form of frequent "stand-up" meetings for prioritizing among multiple goals in innovation (Strode et al. 2012, Sutherland and Sutherland 2014, Rigby et al. 2016a).[2] Helping drive its widespread use in practice for the management of innovation, practitioners often adopt iterative coordination with the expectation that it will help their firms ultimately produce more innovation (Rigby et al. 2016a, Birkinshaw 2018, Bernstein et al. 2019). Nonetheless, the scholarly literature lacks both theoretical grounding and empirical evidence for how iterative coordination actually impacts innovation.[3] Motivated by the managerial importance of this practice and the striking absence of rigorous empirical evidence grounded in organizational theory to validate it, we ask: How does iterative coordination to manage innovation affect the outcomes of novelty and value?

In this paper, we develop theory for how iterative coordination affects an organization's ability to deliver novelty and value when innovating. Although basic intuition would suggest that an organization could choose to pursue either novelty or value (or both), we argue that practicing iterative coordination drives processes that ultimately result in the prioritization of value over novelty in what is eventually delivered. Relative to a baseline of minimal coordination (Lifshitz-Assaf et al. 2020), as an organization uses additional meetings to discuss its goals, it creates additional interim deadlines (Gersick 1988, 1989, Waller et al. 2002). This leads individuals to focus on integrating their existing knowledge to create value, as opposed to specializing in their work to create novelty. Finally, to avoid failure in the pursuit of their originally stated goals, the organization endogenously shifts its goals with each meeting to achieve value over novelty. Thus, although iterative coordination may appear to be an impartial way to manage goals while in the pursuit of innovation, we posit that the practice *implicitly* shifts an organization to realize value at the cost of novelty. The prioritization of value over novelty is implicit, because iterative coordination does not feature any explicit cues to favor value in goals and outcomes.

To empirically measure the effects of iterative coordination on innovation, we partnered with Google LLC, a multinational information technology firm, to embed a field experiment within a public, one-day software application development competition, popularly known as a hackathon. We randomly assign firms at the hackathon to a treatment of iterative coordination. This exogenous variation mitigates traditional endogeneity concerns associated with archival data approaches (Chatterji et al. 2016). To collect precise data that tracks firms, we introduce a novel methodology leveraging the version-control systems used in software development. By documenting the progress of actual software code developed, we capture patterns of firm activities at a granular level over time (by minute) in a balanced panel data set. Our partner provided performance assessments of each organization's final software applications. In addition, we run a follow-up experiment in the laboratory, where we vary mechanisms for iterative coordination and further validate the internal consistency of our findings.[4]

We find that, although iterative coordination leads firms to develop products that are judged to be more valuable, these products are simultaneously less novel. Furthermore, by tracking minute-by-minute changes in the software source code, we find that iteratively coordinating firms favor knowledge integration at the cost of in-depth, specialized knowledge creation by their members. In the follow-on laboratory study, we find that increasing the meeting frequency and opportunities for goal reprioritization in the implementation of iterative coordination reinforces integration and value, while reducing specialization and novelty.

Our study offers three contributions to the organizations literature. First, our findings contribute to the literature on managing multiple performance goals in strategy (Ethiraj and Levinthal 2009, Hu and Bettis 2018, Gaba and Greve 2019, Obloj and Sengul 2020) by studying how iterative approaches may direct organizations to prioritize among distinct goals in innovation. In particular, we emphasize the idea that iterative coordination—and iterative management practices more generally—might carry with it an implicit and overlooked consequence for which dimension of innovation is ultimately prioritized. Second, we contribute novel methodology in the empirical study of organizations by introducing software code tracking as a new data-collection method for studying organizational innovation. This methodology allows researchers to track innovation process at a granular level in real time—for example, exact identification of occurrences of knowledge integration, etc.—bearing implications for the study of innovation across individuals, firms, and time. Finally, we contribute to an emergent literature on new forms of organizing (Levine and Prietula 2014, Puranam et al. 2014, Burton et al. 2017) by studying the management of innovation in hackathons, a setting that encapsulates the broader challenges facing organizations pursuing multiple, potentially conflicting objectives in innovation.

The structure of this manuscript reflects the exploratory orientation that we take toward our study of iterative coordination and innovation. Section 2 details the widespread managerial phenomenon and builds a general theory relating iterative coordination to potential mechanisms that might affect novelty and value in innovation. Our primary experiment in Section 3 then measures the general effects of iterative coordination on innovation in an externally valid field context. Based on the theoretical intuition that emerges from these empirical findings, Section 4 presents the follow-on laboratory experiment that validates specific mechanisms in a more controlled and precise environment. Finally, Section 5 details the contributions that emerge over the course of these theoretical development and experimental efforts.

2. Iterative Coordination and Innovation

2.1. Iterative Coordination Phenomenon in Practice

We propose a model of iterative coordination based on its implementation in practice. Iterative coordination is commonly practiced as a part of Agile management, a broad set of management practices defined by an iterative approach for prioritizing among multiple performance goals (Schwaber and Sutherland 2013, Rigby et al. 2016a).[5] Agile's key insight is the use of an iterative approach to prioritizing among multiple organizational goals (Rigby et al. 2016a, Relihan 2018). Seeking to rebut the extensive ex ante planning and linear approach of traditional manager-driven coordination methods, the designers of Agile methodology reasoned that more frequent iteration on organizational goals was necessary to help organizations embrace innovative outcomes (Cao and Ramesh 2007, Furr et al. 2016). To this end, existing methods for organizational coordination, such as traditional planning and role assignment by managers (Van de Ven et al. 1976, Okhuysen and Bechky 2009), were deemed antiquated and ineffective (Hoppmann et al. 2019, Lu et al. 2019). With new methods to rapidly iterate on organizational goals, Agile methodology could make organizations more flexible and adaptive to their environments, helping them to innovate toward product-market fit (Ries 2011) and market differentiation to achieve competitive advantage (Vesey 1991). Although Agile prescribes a variety of practices to help organizations iterate on multiple performance goals, the use of iterative coordination remains a unifying factor across these diverse implementations. To illustrate the distinction between our specific notion of iterative coordination and Agile more generally, Figure 1 summarizes the broader set of iterative practices most commonly associated with Agile, of which iterative coordination represents an important subset.

Iterative coordination is implemented in the form of frequent meetings to coordinate individuals on their multiple goals, held once a day or even multiple times a day. In each of these meetings, iterative coordination allows an organization to prioritize among their multiple goals via discussion questions, generally three. In a common formulation—which we apply in our experimental studies—each meeting features discussion centered around the following three questions: (1) "What have you accomplished since the last meeting?"; (2) "What are your goals until the next meeting?"; and (3) "What are your goals for the end of the project (and have they changed)?"[6] Question 1 updates members of an organization about prior work toward pre-existing organizational goals. Question 2 defines goals for the organization until its next meeting. Question 3 prompts members of an organization to revisit their overall set of goals. Question 3 especially differentiates iterative coordination from a regular meeting by forcing the organization to revisit its priorities with respect to its shared goals. Through this third question, an organization can

Figure 1. Managerial Practices Associated with Agile

Agile Practice	Description
Interim Deadline Frequency	
Short Interations*	Breaking the development process down to phases, each with an interim deadline.
Frequent Releases	Setting nearly continuous deadline for delivery of working product features to the customer.
Prioritization Discussion Content	
Pre-Existing Goal	
Iteration/Sprint Review*	Reporting of completed work and its efficacy towards achieving pre-defined performance objectives.
Retrospective	Allowing opportunity to reflect on which general processes were ineffective towards a pre-existing goal and can be improved.
Interim Goal	
Iteration/Sprint Planning*	Determining which tasks must be completed to address short-term goals within the context of a development cycle, known as an iteration or sprint.
Team Estimation	Estimating, formally or informally, the effort or cost associated with a potential development goal.
Release Planning	Identifying which aspects of a product or service in question will be delivered to customers for feedback.
Long-Term Goal	
Product Roadmapping*	Defining long-term goal to overlay onto milestones for how the product or service will be developed (or delivered to the customer) over time.
Dedicated Customer/Product Owner	Designating organizational member to prioritize the interests of the customer in long-term development process.
Coordination Mode	
Scrum Standup Meeting*	Discussing orally the organizational priorities and tasks in a short meeting, often while standing up.
Kanban Board, Story Mapping	Depicting visually the goals and tasks at various stages of a product or service development process, often on a whiteboard with sticky notes.

Notes. Summary of most popular specific practices associated with or derived from the Agile management framework. Organizations considered to be Agile use at least one, but not necessarily all, of these specific practices. Only the most popular practices in 2020 are included here (VersionOne 2020).

Practices indicated by * are included in our study of iterative coordination. The other practices not marked are not a formal part of this research, but are included to illustrate distinction between iterative coordination and other ways that practitioners might implement Agile.

We provide an organizing framework that categorizes these practices by their specific purpose: setting *Interim Deadline Frequency*, specifying goal *Prioritization Discussion Content*, and facilitating communication with a *Coordination Mode*. *Prioritization Discussion Content* facilitates discussion on a pre-existing goal, an interim goal, and/or a long-term goal; these three types of content roughly map to the three discussion questions, respectively, that we vary in our experimental intervention.

revisit and potentially prioritize among multiple performance goals.

2.2. Applying Iterative Coordination to Manage Innovation

Increasingly, organizations use iterative coordination to manage the pursuit of innovation, ranging from the development of new software applications to creating new broadcast programming (Rigby et al. 2018). Despite its widespread use in the management of innovation, it remains unknown whether and how managing goals with iterative coordination affects the outcomes of novelty and value. We now turn to how the previously undertheorized phenomenon of iterative coordination may shape the prioritization of the distinct goals of novelty and value when innovating.

Although basic intuition would suggest that an organization could choose to pursue either novelty or value (or both) using iterative coordination, we argue that practicing iterative coordination drives processes that ultimately result in the *implicit* prioritization of value over novelty. The prioritization is implicit because it does not involve an ex ante choice to favor value over time. Prioritization occurs due to two mechanisms: additional interim deadlines and goal reprioritization.

The first mechanism—additional interim deadlines—promotes value at the cost of novelty. Under iterative coordination, organizations meet more frequently to discuss shared goals. This creates additional interim deadlines for work (Gersick 1988, 1989, Waller et al. 2002). Prior literature suggests that deadlines serve as an impetus to integrate an organization's existing knowledge (Okhuysen and Eisenhardt 2002). For example, an approaching deadline might inspire a mobile-application developer to combine the payment system she has been working on with the user interface for the rest of the application. In doing so, the organization realizes value from integration: The payment system would not contribute any value if not integrated with the rest of the application. This example highlights the primary purpose of integration in organizations, which is to realize value from the existing knowledge of its members (Grant 1996). For instance, the organization can integrate different individual perspectives to refine ideas and proposals that promise to deliver the most value (Girotra et al. 2010, Keum and See 2017).[7]

Absent iterative coordination to prompt the revisiting of shared goals, organizations integrate less knowledge from their members. As individuals create new knowledge, they must share it in a way that is accessible to other members in an organization (Nonaka 1994, Spender and Grant 1996). Accordingly, when lacking the impetus to share knowledge through an intervention such as iterative coordination, organizations have fewer opportunities to integrate their knowledge in a way that creates value for the organization (Okhuysen and Bechky 2009).

Meanwhile, iterative coordination's implicit focus on integration drives out individual specialization needed to generate new knowledge, which may limit the emergence of novelty (Cyert and March 1963). Individual specialization develops and expands the bounds of an organization's knowledge, helping organizations identify novelty (March and Simon 1958, March 1991, Kaplan and Vakili 2015). When individuals specialize in their knowledge-creation efforts, the organization can more efficiently identify and develop ideas that represent truly novel breakthroughs (Csikszentmihalyi 1996, Taylor and Greve 2006). But as individuals focus on integrating their existing knowledge under iterative coordination, they leave less time for specialization. Given limited resources and attention, individuals may integrate or specialize, but they cannot pursue both processes simultaneously (Knudsen and Srikanth 2014). For example, the time it takes to communicate findings and integrate knowledge directly takes away from the time an individual has to specialize in their own work and develop new knowledge (Knudsen and Srikanth 2014, Levinthal and Workiewicz 2018).[8] In light of this well-documented trade-off between integration and specialization in organizations (Lawrence and Lorsch 1967), we would expect specialization and, ultimately, novelty to suffer as a result of iterative coordination.

Although the pressure of additional deadlines may favor value as opposed to novelty in output, how can organizations avoid failing to meet originally stated goals of both high novelty and high value? To this end, a second mechanism of goal reprioritization becomes relevant. Hu and Bettis (2018, p. 886) propose that an iterative approach to goal setting "may sometimes, perhaps often, involve lowering expectations for one or more goal levels, especially under time pressure." Here, iterative coordination allows an organization to reprioritize among simultaneous goals of high novelty and value. With additional deadlines driving an organization toward the goal of value, we predict that a treatment of iterative coordination will lead an organization to implicitly reprioritize the balance between value and novelty in its goals, leading to the ultimate realization of value over novelty in its final outcome. We predict this to be true, regardless of an organization's starting point in terms of its balance between value and novelty, as empirical work demonstrates that it is much more difficult to reintroduce novelty than value during the innovation process (Berg 2014).

Using this theoretical viewpoint as a starting point for our exploratory inquiry, we now describe the empirical findings from a series of experimental studies that cumulatively guide the development of additional theory on iterative coordination's effects on innovation.[9]

3. Primary Study: Software Development Field Experiment

To study the effect of iterative coordination in innovation, we design and deploy a field experiment. Given iterative coordination's roots in the software industry (Sutherland and Sutherland 2014, Rigby et al. 2016b), we focus on the context of managing software development. To maintain managerial relevance, we sought an externally valid experimental context to demonstrate the impact of iterative coordination as a managerially implementable practice. We begin by presenting background on the externally valid empirical context—a software development competition, known generally as a hackathon—followed by the exposition of our procedure to administer iterative coordination as experimental treatment.

3.1. Experimental Setting

We partnered with Google LLC (Google), a multinational information technology firm, to embed a field experiment within a one-day software application ("app") development competition, or hackathon, hosted on the campus of a university in the northeastern United States.[10] The experiment described below was approved by the university's institutional review board.

3.1.1. Hackathons: Background and External Validity.

Over the last decade, hackathons emerged to play a pivotal role in software development culture and practice (Broussard 2015, Leckart 2015, Pan Fang et al. 2021). Hackathons commonly entail sets of software developers who compete in a contest to develop and present working software by the end of a time frame of a day or two (Leckart 2012). Although some hackathons focus on particular themes or interest areas, they generally operate as open-ended design contests that embrace ambitious innovation goals. In spite of the short time frame allotted, what motivates participants at a hackathon are clear articulations of their project goals (Lifshitz-Assaf et al. 2020).

The competition aspect of the hackathon typifies the dynamic and entrepreneurial settings in the software industry, where firms implement iterative coordination in practice (Ott et al. 2017). Each set of participants mirrors the composition of an archetypal software start-up firm in terms of skills and size. In fact, many successful start-up firms began as hackathon projects: The popular messaging app GroupMe was conceived at the 2010 TechCrunch Disrupt hackathon and acquired a year later by Skype for about $80 million (Arrington 2010, Ante 2011). Given these contextual factors and theory on what defines a firm, we refer to competing teams at a hackathon as firms.[11]

The participating firms compete against each other in a "market," where customer choice is represented by the evaluation of event judges. These judges evaluate the output of each firm at the end of the competition, rewarding selected firms with prizes based on a number of preselected criteria. Hackathons across contexts favor novelty and value in ideas and solution approaches, even as specific judging criteria may differ. This hackathon environment is well suited to study iterative coordination: Much like the features of software markets in which iterative coordination is adopted as a management practice, the hackathon environment prioritizes innovation.

Hackathon sponsors commonly provide mentors to participating firms throughout the competition. Given their nonevaluative, authority-free support role at hackathons, mentors are ideal facilitators of our iterative coordination treatment.

3.1.2. Competition Specifications. In terms set by Google, competing firms developed a software application that provided an innovative solution to some social objective—for example, a sustainability app to track personal carbon footprint or an app for non-governmental organization (NGO) fieldworkers to collect data. Each firm defined its organizational goal as a description of a novel and valuable technical product that they wished to create by the end of the competition.[12] Consistent with standard hackathon practice, firms chose the specific problem they wished to work on, provided it was in service of the general theme of the event. Prizes totaling $2,000 US dollars (USD) in monetary value were provided by Google to top-performing firms.[13]

In collaboration with Google, we recruited firms consisting of software engineers to compete in the hackathon.[14] Competing firms were composed of upper-level undergraduate computer science majors from local universities and professional and freelance software developers. Individual participants qualified based on their prior collaborative software development experience, assessed through a submitted portfolio of past projects. Participants registered together in firms of two to four members in a pre-event survey designed with our cosponsor; the pre-event survey data also served as a source of control variables and for screening potential participants on the technical skills necessary to be productive during the hackathon.

Prior to the start of the competition, firms were randomly assigned into treatment and control conditions. In all, 38 firms competed in the hackathon, consisting of 112 participants (62 students and 50 professionals).[15]

Although firms had flexibility to define the nature of their applications, they were required to meet a few basic requirements for the competition. First, they were required to use a fixed development toolkit provided by Google. This finite software toolkit limited the product attributes firms could consider and use to build their applications (Levinthal 1997, Fleming 2001). By holding available technological inputs constant across treatment and control conditions, we strengthen our ability to interpret the causal effect of our intervention. Second, to collect the detailed data over time on development processes, all firms were required to record their work over the course of the competition with the open-source version-control software Git. By tracking the emergence of software code, Git allows for the detailed measurement of software development activities over time.[16] Finally, Google communicated the need for solutions that would be both novel and valuable to customers—thereby articulating the multiple goals of innovation.[17] Each of these requirements was clearly communicated to all hackathon participants in an opening presentation prior to the official start of the competition.

Within the scope of these requirements, at the start of the competition, firms set a fairly diverse set of goals for themselves to pursue. For example, one firm wanted to build a virtual reality application that would use facial recognition technology to help senior citizens with dementia or Alzheimer's disease identify friends and family. Another firm wanted to build a mobile application with a proprietary algorithm to match refugees with support communities and resources. Yet another firm wanted to create an application that would use artificial intelligence to automatically categorize unstructured data inputs from fieldworkers of small NGOs to make the data practically usable later.

3.2. Experimental Procedure

3.2.1. Experimental Treatment. Leveraging the natural features of the hackathon format, Google engineers who served as mentors to each firm facilitated iterative coordination. At the start of the treatment period, all firms, in both the treatment and control conditions, were approached every two hours by their randomly assigned mentor, who was instructed to offer a null greeting in reference to an item on the schedule (e.g., "How was lunch?"). Each mentor appeared before an equal number of treatment and control firms. After engaging in friendly small talk, mentors visiting control firms concluded their interaction and did not facilitate any discussion on firm goals.[18] In contrast, mentors visiting treatment firms would facilitate a short iterative coordination meeting asking treatment firms to discuss three questions: (1) "What have you accomplished since your last check-in?"; (2) "What are your goals until the next check-in two hours from now?"; and (3) "What are your goals for the end of the day (and have they changed)?"[19] Per instruction, mentors did not provide any quality judgments to firms during these iterative coordination meetings; rather, they simply served as facilitators for group discussion.[20] To ensure that the treatment closely reflected practice, we devised the three aforementioned questions after observing iterative coordination meetings used at Google; we further verified the external validity of these questions in interviews with other engineers from Google and peer firms that practice iterative coordination.[21]

We built in a pretreatment period of 2.5 hours, in which no firms were treated. The inclusion of this pretreatment period allows us to include firm fixed effects and run a generalized difference-in-differences regression model. As we shall address in our firm-minute analysis of the firm processes in Section 3.4, the firm fixed effects control for time-invariant quality differences between firms, bolstering causal identification in case there was any further unobserved time-invariant heterogeneity between firms not addressed by randomization of the treatment. After this pretreatment period, the periodic iterative coordination meetings occurred every two hours until the close of the competition for treatment firms, but not for the control firms. Treatment firms experienced three iterative coordination meetings over the course of the competition.

3.2.2. Other Experimental Design Considerations.

To ensure causal identification from the field experiment, we made several explicit efforts to: limit participant perception of mentor authority; prevent participant awareness of heterogeneous treatment; and ensure proper administration of the iterative coordination treatment by mentors.

We minimized the perception of mentors as authority figures in three ways. First, it was clearly communicated to the participants that mentors absolutely did not serve as or communicate with the judges in the competition. Second, the mentors were demographically similar (e.g., age, professional background, etc.) to the participants, minimizing perceptions of authority enforced by differences in social status (Lincoln and Miller 1979, Ashforth and Mael 1989). Third, the mentors did not provide any unsolicited normative guidance to participants.

Participants remained unaware that treatment firms and control firms experienced different mentor interactions through a number of design decisions made in conjunction with our partner, Google. The undesirable consequences of such awareness range from spillover effects from treatment to control (Duflo and Saez 2003) to Hawthorne effects, where firms act differently due to their awareness of being observed for study (Levitt and List 2011). First, we physically separated the workspaces of treatment and control to minimize the chance of across-condition interaction that might lead to awareness of different attention from mentors. Second, to reinforce perception of parity, the assigned mentors visited firms every two hours in both treatment and control, as previously noted. Although a pure counterfactual control to our iterative coordination treatment could conceivably involve no interaction with mentors every two hours in control, excluding control firms from any mentor visits would risk increasing awareness of differential mentor attention. In addition, keeping the mentor visit events uniform for both treatment and control firms has the desired effect of keeping constant the time for cycles of software development work. Discussions with Google made it evident that any mentor interaction could break up software development cycles in a way that may otherwise not occur. To isolate the causal effect of iterative coordination questions—without the potential confounding factor of different cyclicality—we ensured that both treatment and control would be visited by mentors on the same two-hour cycle. Online Appendix A1 further details the experimental design—including detailed floor plans of the physical space and mentor scripts—as well as several precautions taken to ensure that Google mentors properly administered the treatment of iterative coordination.

We now discuss the data, statistical methods, and results of the field experiment looking at the effect of iterative coordination: first, a section on innovation outcomes—that is, value and novelty; and second, a section on potential processes—that is, integration and specialization, which may lead to innovation.[22]

3.3. Organizational Outcomes

We begin our analysis of iterative coordination on innovation by analyzing its effects on outcomes of *Value* and *Novelty*, two key dimensions of innovation (Amabile 1983, Kaplan and Vakili 2015).

3.3.1. Data. Our data set to study performance outcomes consists of a cross-section of firm project evaluation by expert judges after the end of the hackathon competition, combined with a set of covariates to serve as control variables collected through a pre-event survey.

Each firm was visited by a third-party panel of three judges to evaluate their projects at the end of the competition. Given that novelty and value in innovation are socially constructed by perceptions (i.e., novelty is established relative to existing products), expert judge evaluations are the appropriate method for measuring innovation (Amabile and Pratt 2016). These judges were not involved with and were unfamiliar with our study design. Each judge had several years of work experience in the software industry and had both participation and judging experience in other hackathons prior to our event. The judges tested and interacted with the applications that the firms developed.

As part of the formal registration process for the hackathon, participants were asked to complete a short registration survey that was designed with our cosponsor. We used these survey data to generate firm control variables and assess the efficacy of experimental randomization.

3.3.2. Variables. To measure outcomes of innovation, we use two measures capturing different dimensions of innovation for the applications developed by firms. Our first outcome measure is *Value*, which measures the extent to which an application caters to its existing target customer base. Our second dependent variable, *Novelty*, captures whether an application solves customer problems with a new approach within the scope of Google's furnished app-development toolkit. Judges scored each firm's final project along the two aforementioned outcome categories based on a Likert scale of one to five, summarized in Table 1. The use of these specific criteria to evaluate software applications had been validated by our cosponsor, Google, from experience hosting prior hackathons. Furthermore, *Value* and *Novelty* capture independent components of innovation (Amabile 1983, Singh and Fleming 2010, Kaplan and Vakili 2015), as discussed earlier. We also conducted an independent validation of these measures, detailed in Online Appendix A2.

To control for observable time-invariant, across-firm heterogeneity that might remain despite the randomization process, we include several firm characteristics drawn from the pre-event survey as independent variables. *Current Student* is the firm mean of student status (with students taking a value of one; and zero otherwise). *Graduate Degree* is the firm mean of educational experience (with master's and doctoral degrees taking a value of one; and zero otherwise). *GitHub* is the firm mean of prior history using GitHub. *Google Development* is the firm mean years of experience with the development toolkits provided by Google. *Software Development* is the firm mean years of professional software development experience. *Prior Hackathons* is the firm mean of hackathons attended prior to the event. *Firm Size* is a count variable of the number of members in the firm.

Table 2 presents the summary statistics and correlations of these control variables. In addition, we use these same variables in *t*-tests of differences across treatment and control firms as a check of the efficacy of the randomization, also shown in Table 2. We do not find any evidence of systematic bias in our randomization.[23]

3.3.3. Estimation Model. To compare end-of-competition firm outcomes, we run cross-sectional ordinary least-squares (OLS) models with dependent variables for the evaluation categories regressed on an indicator variable for treatment, with firm control variables drawn from the pre-event survey listed in Table 2. In addition, we include the dummy indicator *No Evaluation* to control for whether a firm officially submitted an application for judge evaluation, which commenced a half-hour after the competition officially closed. Regardless of participation in judge evaluation, all firms nonetheless stayed to the end of the competition, and their project code was observable to us throughout the competition.

3.3.4. Results. Table 3 presents the effects of iterative coordination on final outcomes. Model 3-1 demonstrates that iteratively coordinating firms scored, on average, 0.614 points higher (0.341 standard deviations higher) on *Value* than informally coordinating firms ($p = 0.009$). Removing firms that did not participate in judge evaluation from our sample, Model 3-3 shows that iteratively coordinating firms scored an average of 0.846 points higher (0.471 standard deviations higher) on *Value* ($p = 0.007$). Supporting our

Table 1. Primary Field Study: Variable Definitions and Summary Statistics of Firm Outcomes from Judge Evaluation

Variable	Definition	Mean	SD
Value	How much does your project appeal to the intended market? (Likert scale 1–5)	2.553	1.796
Novelty	Does the project help solve the problem in a new and ambitious way? (Likert scale 1–5)	2.316	1.726

Note. Judges were asked to score each firm's final submission on a 1–5 Likert scale according to the criteria provided by our corporate cosponsor Google.

Table 2. Primary Field Study: Firm Characteristics and Correlations

	Sample				Pairwise correlation						
Variable	Full	Treatment	Control	Difference	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(1) *Current Student*	0.539	0.495	0.579	0.0838	1						
	(0.386)	(0.402)	(0.377)	(0.126)							
(2) *Graduate Degree*	0.360	0.375	0.346	−0.0292	−0.253	1					
	(0.344)	(0.334)	(0.361)	(0.113)							
(3) *GitHub*	0.901	0.954	0.854	−0.0995	0.227	−0.557	1				
	(0.192)	(0.138)	(0.223)	(0.0609)							
(4) *Google Development*	0.461	0.463	0.458	−0.00463	0.496	0.155	−0.049	1			
	(0.323)	(0.363)	(0.292)	(0.106)							
(5) *Software Development*	3.695	3.838	3.567	−0.271	−0.411	0.359	−0.124	−0.006	1		
	(3.839)	(3.661)	(4.083)	(1.264)							
(6) *Prior Hackathons*	1.825	1.662	1.971	0.309	0.104	−0.079	−0.033	0.238	0.349	1	
	(1.186)	(0.943)	(1.378)	(0.387)							
(7) *Firm Size*	2.947	2.722	3.150	0.428	0.132	−0.12	0.009	0.242	−0.361	−0.214	1
	(0.837)	(0.826)	(0.813)	(0.266)							

Notes. Means and standard deviations in parentheses for firm-level observations of the full sample ($N = 38$), treatment condition, and control condition. The Difference column shows a t-test of difference in means between the treatment and control condition, with standard errors in parentheses. The numbered columns to the right display pairwise correlations.

findings of a positive association between iterative coordination and *Value* are Models 3-2 and 3-4, which include the full set of firm controls to control for observable heterogeneity not addressed by the experimental randomization.

In contrast, Model 3-5 indicates that iteratively coordinating firms scored approximately a half-point less than control firms (0.278 standard deviations lower) on *Novelty* ($p = 0.082$), with a similar negative association in Model 3-7 ($p = 0.079$). Models 3-6 ($p =$

Table 3. Primary Field Study: Regression Analysis of Firm Outcomes from Judge Evaluation

	Value				*Novelty*			
Variable	(3-1)	(3-2)	(3-3)	(3-4)	(3-5)	(3-6)	(3-7)	(3-8)
Treatment Condition	0.614**	0.546*	0.846**	0.661*	−0.499†	−0.692*	−0.687†	−0.960*
	(0.222)	(0.208)	(0.290)	(0.302)	(0.278)	(0.332)	(0.375)	(0.370)
Current Student		0.725*		0.991		0.145		0.638
		(0.353)		(0.650)		(0.390)		(0.793)
Graduate Degree		0.430		0.365		−0.458		−0.877
		(0.384)		(0.590)		(0.520)		(0.697)
GitHub		0.120		0.106		0.893		0.907
		(0.798)		(0.965)		(0.996)		(1.106)
Google Development		−0.207		−0.310		0.898†		1.006
		(0.410)		(0.813)		(0.498)		(0.695)
Software Development		−0.050†		−0.039		0.007		−0.003
		(0.028)		(0.041)		(0.040)		(0.048)
Prior Hackathons		−0.144		−0.182		−0.096		−0.046
		(0.087)		(0.107)		(0.124)		(0.162)
Firm Size		−0.113		−0.096		−0.222		−0.421†
		(0.102)		(0.149)		(0.202)		(0.223)
No Evaluation	−3.497***	−3.702***			−3.336***	−3.331***		
	(0.189)	(0.195)			(0.202)	(0.256)		
Constant	3.274***	3.590***	3.154***	3.456***	3.518***	3.284***	3.615***	3.782**
	(0.219)	(0.883)	(0.249)	(1.170)	(0.214)	(1.151)	(0.241)	(1.434)
R^2	0.874	0.922	0.261	0.574	0.774	0.824	0.117	0.454
Sample	Full sample		Evaluation only		Full sample		Evaluation only	
Observations	38	38	27	27	38	38	27	27

Notes. Ordinary least squares estimation of cross-sectional data at the firm level. Robust standard errors are shown in parentheses. In estimates involving the full sample, *No Evaluation* takes a value of one for firms that decided not to undergo the judging process.
$^{\dagger}p < 0.10$; $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$.

0.046) and 3-8 ($p = 0.018$) demonstrate the robustness of this result when including the full set of firm controls.

3.3.5. Supplemental Analyses. We conduct a number of additional tests to verify the robustness of these findings to alternative specifications and explanations. We confirm the direction and statistical significance of the main effects across these analyses to rule out alternative explanations. First, we conduct a post hoc analysis of the effect of heterogeneity in the *Value* and *Novelty* of the articulated starting goal of each firm at the start of the hackathon. This post hoc analysis provides suggestive evidence that there is a boundary condition to the effect of the iterative coordination experimental intervention. Firms that start with a goal that is already high in *Value* and/or low in *Novelty* are still impacted by the intervention, but to a lesser degree. We also use these data to show that the mean *Value* and *Novelty* of starting goals are comparable across the treatment and control conditions, supporting the efficacy of the experimental randomization. Relatedly, this post hoc analysis provides suggestive evidence that goals do shift due to iterative coordination based on the difference between a firm's starting goal and what it ultimately delivers at the end. Second, we find the same patterns in an ordered logit analysis mirroring Table 3. Third, we rule out the alternative story that there may be differences in productivity across firms by looking for possible differences in project completion by the end of the competition. Fourth, we do not find that the intervention has any effect on selection by firms into evaluation. Fifth, we confirm that the same pattern holds when we allow for firm characteristics as moderators. The online appendix presents the full results of these robustness checks in detail.

In measuring the effect of iterative coordination on innovation outcomes, we find the results are mixed: Although iteratively coordinating firms develop products that are more valuable, these products are simultaneously less novel. Despite the competitive context of the Google hackathon demanding novel solutions, our results demonstrate that firms treated by iterative coordination questions produce less-novel output.

3.4. Organizational Process

Given the results outlined in the previous section, we ask: What processes might iterative coordination be influencing that associate with more valuable, yet less novel, output? In Section 2, we presented a theory for iterative coordination that associates greater *Value* and reduced *Novelty* with the processes of integration and specialization, respectively. We now dive into firm software code to unpack iterative coordination's influence on processes of integration and specialization, both of which have long been studied by organizations scholars for their relationship to innovation (Lawrence and Lorsch 1967, Grant 1996, Levinthal and Workiewicz 2018).

3.4.1. Data: Software Development. To study the effect of iterative coordination on the innovation process, we analyze a balanced firm-minute panel with dependent variables measuring integration and specialization in the software development process based on our minute-by-minute tracking of the firms' updates of their software code through Git.[24] With each update timestamped to the minute, our novel empirical strategy achieves a precise level of granularity.

Our dependent variables measure specific actions in the software development process consistent with integration or specialization, as summarized in Table 4.[25]

3.4.2. Dependent Variables. To measure organizational integration, *Code Integration Action* consists of the stock count of actions taken by the firm to integrate software code within the firm's shared code base. This measure captures two types of convergent development efforts in facilitating an integrated codebase. First, individual software developers, who may specialize and write some code independently, must combine their individual code with the firm's shared code base to integrate it with the overall project. Second, developers may combine code that is already in the shared codebase, thereby further integrating aspects of the project. The required version-control

Table 4. Primary Field Study: Variable Definitions and Summary Statistics of Firm Process from Software Code

Variable (Interp.)	Definition	Mean	SD	Min	Max
Code Integration Action (Integration)	Count of actions taken by the firm to integrate software code into and within the firm's shared code base.	1.954	3.316	0	20
Advanced API Specialization (Specialization)	Count of uses of nonrequired specialized and advanced application programming interface procedures, protocols, and tools.	0.700	1.474	0	7

Notes. Dependent variables for firm process defined with their conceptual interpretation (Interpret.). Observations are at the firm-minute level, with 20,520 firm-minute observations across 38 firms.

software enables and tracks these integrative activities, which are a standard part of software workflow management.

To measure specialization, *Advanced API Specialization* measures a firm's use of nonrequired specialized and advanced application programing interface (API) procedures, protocols, and tools in their codebase. Although firms were required to use a broader toolkit provided by our sponsor, Google, there were several advanced API tools available to the developers for free that were encouraged, but not required, in the competition. These tools allow firms to use a number of advanced cloud-based features: analyze data using artificial intelligence capabilities, conduct natural language processing, leverage remote graphics processing units for machine learning and 3D visualization, and connect with Internet-of-things devices, among other functionality. We measure the use of these tools by identifying the number of API calls or requests to these tools appearing in a firm's codebase. These tools require in-depth specialized knowledge to use, beyond the common knowledge developers would have coming into the competition. Moreover, these tools were only free in the context of our competition; they were available as paid enterprise software outside of the competition, making it likely that developers would not have used them regularly prior to the competition. An optional tutorial on these advanced features was available to all hackathon participants.[26] Because *Advanced API Specialization* reflects advanced technical development beyond the expected standards, we use it to measure specialization.

3.4.3. Estimation Model. We use these two dependent variables in a firm-minute panel to estimate the following differences-in-differences model:

$$Y_{it} = \beta(Treatment_i \times Post_t) + \alpha_i + \delta_t + \epsilon_{it}.$$

Y_{it} represents the dependent variables of *Code Integration Action* and *Advanced API Specialization*. $Treatment_i$ is an indicator variable taking a value of one for firms treated by iterative coordination, and $Post_t$ is an indicator variable equaling one after the completion of the first of three mentor check-ins. Our coefficient of interest β estimates the effect of iterative coordination meetings on Y_{it}.[27] The intentional inclusion of a pretreatment period in the experiment allows us to include firm fixed effect α_i to control for time-invariant unobserved confounding factors (e.g., the complexity of the firm's chosen problem, Google toolkit know-how, etc.).[28] δ_t is a minute fixed effect to control for potential shocks across all firms during the hackathon (e.g., the beginning of lunch service at the event). We cluster robust standard errors at the firm level.

3.4.4. Results. Table 5 reports the results of regression analyses that test the effects of iterative coordination on integration and specialization. Model 5-1 reveals that treatment is positively associated ($p = 0.024$) with *Code Integration Action*. That is, after iterative coordination meetings commence, iteratively coordinating firms conduct, on average, 2.074 more code integrations than control firms.

On the other hand, Model 5-2 displays a negative relationship ($p = 0.009$) between iterative coordination and *Advanced API Specialization* use. Specifically, iteratively coordinating firms conduct 1.124 fewer highly specialized uses of Google's advanced application development toolkit in the postperiod.

Figure 2 visualizes the estimated effect of the iterative coordination treatment over time.

3.4.5. Supplemental Analyses. We devise a number of additional tests to assess the robustness of these findings. First, we devise two additional measures of integration and specialization based on the underlying structure of the file hierarchies in the software code. These two measures are based on branching factors, a standard performance measure in the computer science literature (Knuth and Moore 1975, Baudet 1978, Muja and Lowe 2009). We find statistically significant results consistent with those reported in Table 5. We carry these measures through the other robustness checks.

Second, to ensure the robustness of our results relative to estimates of standard errors, which may be underestimated due to serial correlation in long time-series panels, we follow Bertrand et al. (2004) and run our main analysis with observations collapsed to a preperiod and a postperiod. We find statistically significant effects consistent with our findings in Table 5.

Third, given the cumulative nature of our treatment—three administered iterative coordination meetings—we

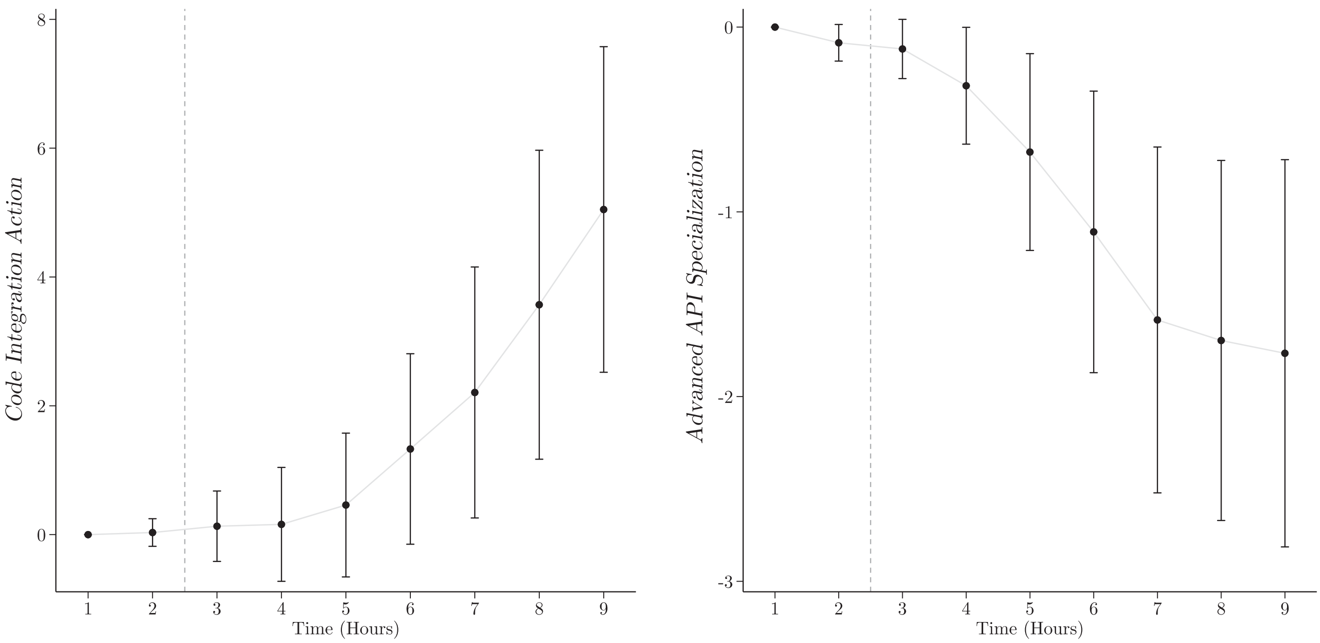
Table 5. Primary Field Study: Regression Analysis of Firm Process from Software Code

Variable	(5-1) *Code Integration Action*	(5-2) *Advanced API Specialization*
Treatment × *Post*	2.074*	−1.124**
	(0.878)	(0.408)
Firm FE	Yes	Yes
Time FE	Yes	Yes
R^2	0.456	0.335
Adjusted R^2	0.441	0.317
Firms	38	38
Observations	20,520	20,520
Level	Firm-minute	Firm-minute

Notes. Ordinary least squares estimation of firm-minute level data. Robust standard errors clustered at the firm level are shown in parentheses. FE, fixed effects.

*$p < 0.05$; **$p < 0.01$.

Figure 2. Primary Field Study: Effect on Process over Time



Notes. These two graphs depict the effect of the iterative coordination treatment over time. Each point estimate represents the difference in the level of the dependent variable between the treatment and control groups. The dotted grey line indicates the start of the treatment period. These graphs were constructed based on OLS regression estimates using an indicator variable for each hour of the experiment interacted with the treatment variable. The indicator variable for the first hour was necessarily omitted for estimation tractability, but shown here as the baseline term, set at a value of zero—that is, equating the treatment and control groups. Firm and time (minute) fixed effects included. The 95% confidence intervals shown derive from robust estimates of standard errors clustered at the firm level.

assess iterative coordination's cumulative influence on integration and specialization after each iterative coordination meeting. A concern for the viability and interpretability of our results would arise if, for instance, iterative coordination's effects on integration and specialization were observed early in the post-period and were not sustained through the rest of the total observation period. As we would expect, our estimates of the effect of iterative coordination over time identify larger (further from zero) point estimates with greater statistical significance in later periods.

Fourth, we consider the extent to which observed differences between iteratively coordinating firms and firms in control may be driven due to differences in underlying productivity. If, for instance, iterative coordination hindered productivity, this alternative mechanism may instead explain iterative coordination's negative relationship with specialization. Nonetheless, we find that iterative coordination bears no significant effect, positive or negative, on a firm's raw productivity, mitigating this alternative explanation.

Fifth, we find no statistically significant difference in the amount of time that treatment and control firms spent in their mentor interactions or afterward to regroup and get back to work. The time spent for these interventions is very short compared with the overall length of the competition.

Sixth, we assess the results relative to potential moderating firm characteristics. The full results of these six categories of additional analyses are provided in the online appendix.

3.5. Discussion of Primary Study

We now holistically consider how the empirical findings of the primary field experiment fit together. We first show that iterative coordination affects the innovative output of the firms we study, where it associates positively with value and negatively with novelty, as judged in the final products of each firm. We then turn to the granular software code data to understand what processes iterative coordination might impact. Consistent with our theory, we find that iteratively coordinating firms take more development actions oriented toward integration, as measured by changes in *Code Integration Action*. Meanwhile, they invest less in advanced, novel uses of Google APIs, suggesting decreased specialization. To formally test an empirical connection between specialization and novelty, we perform a post hoc mediation analysis, reported in the online appendix.[29] This analysis suggests that there is reason to believe that integration and specialization are mediators that link iterative coordination to value and novelty, respectively. Nonetheless, we recommend caution in broadly interpreting this particular post hoc finding: We do not exogenously vary mediators of *Code Integration Action* and *Advanced API Specialization*, which would represent the ideal empirical design for a causal mediation analysis. Overall, the processes of integration at the cost of specialization help illustrate goal reprioritization toward value over novelty—as firms shift their focus toward value over novelty objectives, the drive for individuals to specialize and create new knowledge to identify novel outcomes becomes less salient.

Although the results thus far explore iterative coordination's influence on innovation outcomes and related processes of integration and specialization, they leave open questions regarding mechanism. For instance, what provides the impetus for greater integration and value in iterative coordination? We previously theorized in Section 2 that (1) the addition of interim deadlines and (2) opportunities for reprioritizing among goals would provide an impetus for integration and the shift toward value over novelty in outcomes. Indeed, our post hoc analysis of starting goals suggests that iterative coordination generally pushes firms away from a starting goal of novelty and toward value, as realized in their final output. Importantly, this empirical finding is consistent with our theoretical perspective that allowing for reprioritization of goals leads to value over novelty in output.

To more directly unpack the influence of the aforementioned mechanisms of interim deadlines and opportunities for goal reprioritization, we conduct a follow-on laboratory experiment that allows for data to be collected to study these two mechanisms, beyond what was possible in our field setting.

4. Follow-On Study: Product Development Laboratory Experiment

To complement our primary field experiment, we run a second experiment in the laboratory to study the influence of iterative coordination's two mechanisms on innovation outcomes: interim deadlines and the opportunity to update and reprioritize goals. In addition to helping unpack these two mechanisms for iterative coordination, a follow-on laboratory experiment yields a number of desirable features. First, it follows the best practice of prior work to combine field data with laboratory experiments (Stoop et al. 2012). Although our primary field experiment provides the benefit of external validity and applicability, it represents a less-controlled experimental environment. The laboratory environment provides that control and precision. Second, we collect a broader set of data, not possible in the field, to build alternative measures that confirm our findings in the field and allow us to measure alternative mechanisms that may take place and to rule them out. In particular, we address the extent to which the interventions, in the form of formal meetings, account for time that participants would otherwise be working and whether and how the intervention affects the degree of coordination that would otherwise take place in between meetings.

4.1. Experimental Design

In the experiment, teams design a new dormitory or apartment product concept for a manufacturer. The teams compete with other teams for a cash prize based on their proposed product. We implement this task based on prior work by Girotra et al. (2010).[30] We randomly assign teams to one of three experimental conditions described later, where we vary the implementation of the iterative coordination treatment. We preregistered this laboratory experiment online with the Open Science Framework.[31]

4.1.1. Participant Sample. We recruited 210 participants, drawn from the general population, to participate in this study in a behavioral research laboratory at a university in the northeastern United States. To arrive at this sample size, we use the effect sizes estimated in the primary field experiment in an a priori power analysis. Given the nature of our task, we imposed a requirement for our study that all participants must have an education level of at least some college education and a high-school diploma or equivalent. Online Appendix A4 details the recruitment procedure and the a priori power analysis.

We randomly assign participants into 70 teams of three individuals and randomly assign each of these teams to one of three conditions.[32] Verifying the validity of this randomization, we find no statistically significant difference in individual characteristics across the three conditions.[33]

4.1.2. General Procedure. After entering the laboratory and completing a pre-experiment survey, participants receive instructions on their product development task. We randomly assign participants into teams of three and separately escort each team to their own private room to begin the experiment. Each team works in their own private room for 60 minutes. We provide teams with sketch paper to make preliminary individual drawings; each individual could write on the sketch paper with a unique-color pen assigned to the individual. Each room contains a whiteboard for the team to illustrate its final product submission.

After the 60-minute experiment, participants first complete a post-experiment survey. Participants then vote on which team's final product concept, among those in their session, is their favorite; they are barred from voting for their own product. Within a session, we assign all teams to the same experimental condition, so no team is unfairly advantaged for the prize. In addition to $25 USD in compensation to participate in the study, the team that receives the most votes from peers in the session wins a prize of $10 USD per person.

4.1.3. Conditions. We design three experimental conditions, seeking to vary iterative coordination by the frequency of its meetings and the extent to which its discussion questions allow for a reprioritization of organizational goals. With these two dimensions of variation in mind, we sought an experimental design that would allow us to simultaneously explore both dimensions, while minimizing the number of experimental conditions to maximize the number of teams per condition for statistical power; team-level experiments are especially expensive, given that they require, in our case, three times the number of recruited participants for the equivalent power of an individual-level study. Given this constraint, this follow-on study has no pure control condition, as in the primary study.

To implement the experimental treatments, a member of the research team acts as a team mentor who visits each team intermittently to administer the iterative coordination meeting intervention(s).[34] The general structure and content of the mentor-participant interaction parallel what was used in the hackathon in the primary study, except for the questions discussed and the frequency of meetings.

In condition 1, the baseline condition, we subject teams to only one intervention, asking only question 1 of an iterative coordination treatment: After 20 minutes from the start of the experiment, the mentor asks one question to each team, "What have you accomplished since the last check-in?", which updates all the members of the team on progress toward a pre-existing shared goal. In condition 2, teams only experience one intervention at 20 minutes into the experiment, as in condition 1, but we vary the question composition to include the opportunity to reprioritize shared goals; the mentor also asks, "What have you accomplished since your last check-in?" and "What are your goals for the end of the day?" We do not ask them, "What are your goals until the next check-in?" because there is no next check-in, and it would be redundant with the question on the goals for the end of the day. In condition 3, teams experience two interventions, one at 20 minutes and one at 40 minutes, and they discuss all iterative coordination questions mirroring those posed in the hackathon field experiment. Table 6 summarizes the questions asked in each mentor/team interaction.

The comparison between conditions 1 and 2 captures the mechanism of prioritizing between organizational goals: question 1 only entails describing work achieved in pursuit of a pre-existing goal. In contrast, whereas conditions 2 and 3 hold constant the mechanism of prioritizing between organizational goals, what differs is the frequency of meetings and, thus, interim deadlines: Condition 3 experiences an additional iterative coordination meeting at 40 minutes. In essence, one could view these conditions as representing low (1), medium (2), and high (3) levels of thoroughness or intensity in the implementation of iterative coordination.

4.2. Data and Measures

Table 7 details the source, construction, and interpretation of our empirical measures, organized by the construct they intend to measure.[35]

We document the final product of each team, which was a product drawing on a separate whiteboard. These final products were rated by two independent raters on the dimensions of *Value* and *Novelty*, scored on the validated criteria and Likert scale (1–5) as in the primary study. Given high levels of interrater agreement, 0.86 and 0.80, respectively, an average of

Table 6. Follow-On Laboratory Study: Experimental Conditions

Time elapsed	Condition 1 (Baseline)	Condition 2 (Question Composition)	Condition 3 (Number of Interventions)
20 minutes	What have you accomplished since the last check-in?	What have you accomplished since the last check-in? What are your goals for the end of the day?	What have you accomplished since the last check-in? What are your goals until the next check-in? What are your goals for the end of the day?
40 minutes			What have you accomplished since your last check-in? What are your goals for the end of the day? (And have they changed?)

Notes. This table summarizes the experimental intervention design in each of the three conditions. Different sets of questions at different times were posed by the mentor in each intervention.

Table 7. Follow-On Laboratory Study: Variable Definitions and Sources

Variable	Definition	Source
Outcomes		
Value	Usefulness of the final product (Likert 1–5). Average of two independent rater assessments.	Final output
Novelty	Novelty of the final product (Likert 1–5). Average of two independent rater assessments.	Final output
Process		
Time to Integrate	Time in seconds into the experiment until the team began working on the final product on the board based on draft individual sketches.	Video recording
Individual Sketches	Count of pages of draft sketches generated by individuals. Reflects total productivity of individual specialized work.	Individual sketches

Note. Measures drawn from each team's *Final Output* design, *Video Recording* of their working session, and their *Individual Sketches*.

the two ratings for each dimension of product outcomes was used in analysis.

We collect and code the work output of each individual over the course of the experiment and each team at the end of the competition. We separately track the preliminary design work done by each individual over the course of the experiment in the form of sketches on regular pieces of paper. We use these sketches to measure individual specialization, *Individual Sketches*, which is the count of the pages of draft sketches generated by individuals. *Individual Sketches* serves as a proxy for the total productivity of individuals on a team in their specialized work.

We record the video and audio of each team throughout the course of the competition. As a measure of integration, *Time to Integrate* assesses how quickly teams begin the process of integrating their individual work into the final product. *Time to Integrate* measures the time elapsed in seconds from the start of the experiment to when a team first writes on the whiteboard, where they are required to report their final project submission. Writing on the whiteboard is a more integrative team activity, in contrast to individually drawn preliminary sketches on paper, which reflect a more individually specialized activity. To build this measure, a research assistant watches the video and takes down the time stamp of the first moment when a dry-erase marker touches the whiteboard; there is no ambiguity in the coding process. Because teams use the whiteboard space for the final product, the action of writing on the whiteboard reflects integration of team knowledge into the final product. *Time to Integration* serves as a salient and behavioral (i.e., non-survey-based) indicator of integration that we can observe in the laboratory.[36] We also use the audio to generate text transcripts for several supplemental analyses.

4.3. Results

In Table 8, we report the summary statistics and cross-sectional analysis of the results of this laboratory study that compares differences in the means of the measures between conditions 1 and 2 and between conditions 2 and 3.[37] The direction and statistical significance of these findings are preserved when we instead use an OLS regression model that contains indicators for conditions 2 and 3, where the relationships of interest would be the coefficient on the condition 2 indicator and the *t*-test of differences in the coefficients on the indicators for conditions 2 and 3.

4.3.1. Outcomes. We demonstrate that both the ability to prioritize among innovation goals and the frequency of meetings have a statistically meaningful effect on the outcomes, consistent with two suggested mechanisms of iterative coordination. With respect to *Value*, we find that including an opportunity to reprioritize among shared goals (comparing conditions 2 and 1) generates greater *Value*, 0.473 points higher on a five-point Likert scale (p = 0.006), representing a 0.786 standard deviation increase. The second mechanism of additional interim deadlines imposed by an additional meeting (comparing conditions 3 and 2) generates greater *Value* than in condition 2 by 0.295 points on a five-point Likert scale (p = 0.041), a 0.490 standard deviation increase. With respect to *Novelty*, we find an effect in the opposite direction. The addition of questions to reprioritize among goals (comparing conditions 1 and 2) generates lower *Novelty* by 0.342 points on a five-point Likert scale (p = 0.088), a 0.494 standard deviation decrease. Increasing the frequency of meetings in condition 3 generates a lower degree of *Novelty* than in condition 2 with only one meeting, 0.397 points on a five-point Likert scale (p = 0.023), or a 0.574 standard deviation decrease.

4.3.2. Process. With respect to the process of integration, measured by *Time to Integrate*, we find that including an opportunity to reprioritize goals in condition 2 (versus condition 1) and the higher frequency of meetings in condition 3 (versus condition 2) lead to

Table 8. Follow-On Laboratory Study: Summary Statistics and Cross-Sectional Analysis

	Sample				Difference in means	
Variable	Full	Condition 1	Condition 2	Condition 3	1 vs. 2	2 vs. 3
Outcomes						
Value	3.386	2.971	3.444	3.739	0.473**	0.295*
	(0.602)	(0.594)	(0.535)	(0.414)	(0.165)	(0.140)
Novelty	3.190	3.551	3.208	2.812	−0.342†	−0.397*
	(0.692)	(0.729)	(0.612)	(0.540)	(0.196)	(0.169)
Process						
Time to Integrate	1579.6	1969.3	1572.8	1196.9	−396.4*	−375.9*
	(659.5)	(496.6)	(771.8)	(427.4)	(190.2)	(183.1)
Individual Sketches	4.171	5.304	4.208	3.000	−1.096*	−1.208**
	(1.579)	(1.329)	(1.560)	(0.853)	(0.424)	(0.369)

Notes. The first three columns contain the mean and in parentheses the standard deviation of teams in each condition. The last two columns compare conditions 1 vs. 2 and conditions 2 vs. 3, respectively, based on a *t*-test of the difference in means; the values reflect the difference in means and, in parentheses, the standard error.
†$p < 0.10$; *$p < 0.05$; **$p < 0.01$.

faster *Time to Integrate* ($p = 0.043$ and $p = 0.046$, respectively), suggesting that the separate components of iterative coordination do lead to integration. The effect of the additional goal question (condition 2 versus condition 1) amounts to 396 seconds (6.6 minutes) faster *Time to Integration*, representing a 0.60 standard deviation reduction in how long it takes a team to use the whiteboard to integrate ideas, amounting to 11% of the total time (one hour) the team had available for the task. The effect of the additional meeting (condition 3 versus condition 2) amounts to 376 seconds (6.3 minutes) faster *Time to Integration*, a 0.57 standard deviation reduction amounting to 10% of the total available time.

To evaluate specialization, we then use our data on the sketches generated by the individuals on each team as an indication of intermediate individual-level specialization activity. We find that both the goal-reprioritization question in condition 2 (versus condition 1) and the additional meeting in condition 3 (versus condition 2) reduce the total count of *Individual Sketches* generated by the members of each team ($p = 0.013$ and $p = 0.002$, respectively). This result implies that the opportunity to reprioritize shared goals or increasing the frequency of meetings reduces the capacity of the team to be productive in preliminary individual specialization activity. The effect of the additional goal question (condition 2 versus condition 1) amounts to 1.1 fewer pages of sketches, a 0.69 standard deviation reduction in sketches or lost sketch productivity of about 0.61 person-hour. The effect of the additional intervention (condition 3 versus condition 2) amounts to 1.2 fewer pages of sketches, a 0.76 standard deviation reduction in sketches or lost sketch productivity of about 0.86 person-hour.[38]

4.3.3. Additional Findings. We collect data for a number of supplemental analyses to shed additional light on the effects of different implementations of iterative coordination. First, we assess the impact on completeness to assess (and then rule out) the possibility that there is a general productivity effect—for example, a world where iterative coordination positively (or negatively) impacts both value and novelty just because organizations get more (or less) work done on a general basis.

Second, we use the survey measures of coordination and specialization as further validation, from the participant perspective, of the processes that our experimental manipulation of iterative coordination engenders: increasing coordination for integration and reducing specialization. We confirm—via a post-experiment survey—that more thorough implementations of iterative coordination positively associate with self-reported measures of coordination and negatively associate with self-reported measures of specialization.

Third, we assess the amount of time iterative coordination takes because the time cost of iterative coordination is an important boundary condition for organizations deciding whether to adopt it. It is ex ante plausible that the time taken by the meetings would be meaningfully large and would hurt productivity on a general basis. We use the video recordings to code both the duration of iterative coordination meetings and the time it takes teams to get back to work after a meeting. We find that the meetings and time afterward take up a small amount of time (3.6% of total available time), and adding a second meeting (condition 3 versus condition 2) takes up less than double the time of the first meeting alone.

Fourth, we want to evaluate the communication taking place between and out of the iterative coordination meetings to get a sense of whether iterative coordination was increasing (complementary with) or decreasing (substituting for) the normal levels of communication that would otherwise take place if the organization did not practice iterative coordination. We measure the oral communication that takes place in between iterative coordination meetings using the video recordings. We find that adding the additional question (condition 2 versus condition 1) and adding the additional meeting (condition 3 versus condition 2) lead to a statistically significant increase in the frequency of communication between meetings, but not in the total amount of words being spoken.

Fifth, we analyze the oral communications of each team to build several text-based measures (Reypens and Levine 2018) to understand whether there is an effect on the salience of the final deadline (at the end of the 60-minute period) and its implications for integration and affect—that is, negative emotion or anxiety. We find that adding the additional question (condition 2 versus condition 1) and adding the additional meeting (condition 3 versus condition 2) lead to a statistically significant increase in the use of words associated with time, as well as a shift toward words proposing new activity distinct from prior activity. When combined with the other evidence, we interpret this to be a shift toward more integration. However, we do not find any evidence that a more thorough implementation of iterative coordination leads to more anxiety, negative emotion, or swearing, as observable in oral communication. The online appendix reports the data-collection methods and results for these analyses.

Together, these results suggest robustness to a number of alternate mechanisms. For instance, the time cost of (additional) meetings cannot account for the entire observed decrease in specialization among iteratively coordinating organizations. Although an additional meeting mechanically reduces raw time for work, the incremental time cost of a meeting decreases—that is, a subsequent additional meeting takes less time than a previous meeting. Similarly, although additional meetings increase raw latency for teams to resume their work, this latency diminishes with increasing frequency of meetings.

4.4. Discussion of Follow-on Study

The laboratory experiment shows that more thorough implementations of iterative coordination have a positive association with value and a negative association with novelty. These findings align with the findings of the primary field study, which only compares iterative coordination against a baseline control condition with no intervention. Furthermore, more thorough implementations of iterative coordination associate with integrative activity—reflected in this study as the quicker integration of sketch material into the final product—while being negatively associated with individually specialized activity—the pages of individual draft sketches produced.

When testing two mechanisms of iterative coordination—namely, the frequency of interim deadlines and goal-reprioritization questions—we find that augmenting these aspects of iterative coordination amplifies its effects. That is, the addition of discussion questions (creating opportunities for reprioritizing goals) and the additional meetings (imposing additional interim deadlines) yield stronger positive effects on value and stronger negative effects on novelty. These two mechanisms similarly amplify the positive effect on integration and the negative effect on specialization associated with iterative coordination in general. In particular, additional deadlines lead to a shift in activity toward integrating knowledge. This shift is triggered by an attention to time, a focus on the future deadline, and a recognition of discrepancy between the current state and the future desired state. Interestingly, additional interim deadlines from iterative coordination do not increase anxiety among participants. Here, we posit that the lack of anxiety may relate to an ability to reprioritize goals using iterative coordination. Prior work demonstrates how unfilled goals make individuals anxious (Masicampo and Baumeister 2011). By reprioritizing goals, individuals treated with iterative coordination may create new plans for unfilled goals, helping reduce their anxiety (Masicampo and Baumeister 2011). Overall, for business practice, the mechanisms of frequency of interim deadlines and goal-reprioritization questions allow mangers and organizations to tweak iterative coordination to generate the degree of value and novelty they desire.

In interpreting the mechanisms underlying the questions, in Section 2, we particularly focus on question 3's influence on helping the organization revisit its overall priorities and potentially adjust them in the future (by asking, for instance, if goals have changed). This idea of adjustment in prioritization over time is central to Agile practice more generally (Sutherland and Sutherland 2014, Rigby et al. 2016b). Theoretically, it permits satisficing behavior where relaxing originally stated objectives allows for more attainable standards on objectives—that may fail to be met due to ongoing underperformance (Simon 1947, Hu and Bettis 2018)—such as the acceptable level of novelty in the final product. Future work could examine whether ex ante specifying the goals to be discussed in the third discussion question to explicitly include novelty may help preserve outcomes of novelty over time, or if it would instead lead to underperformance on both novelty and value objectives.

5. Discussion and Conclusion

How organizations pursue multiple performance goals for which there is no clear prioritization among them is an important question for organizations research. In this paper, we study the effects of the widespread, yet poorly understood, practice of iterative coordination, which allows organizations to prioritize among novelty and value in innovation. We embed a field experiment within a software development competition in partnership with Google. We find that although iteratively coordinating firms develop products that are more valuable, these products are simultaneously less novel. By tracking the underlying software code, we find that iteratively coordinating firms integrate existing knowledge at the cost of specializing to create new knowledge. We then conduct a follow-on laboratory experiment to help verify underlying mechanisms for iterative coordination. We find that increasing the frequency of meetings and including an opportunity to reprioritize organizational goals amplify integration in iterative coordination, driving value at the cost of novelty in outcomes.

We now detail three primary contributions of this work: highlighting how iterative process to manage multiple performance goals may *implicitly* prioritize certain outcomes in innovation; introducing software code-tracking methodology for studying organizational innovation process; and addressing recent calls from the literature to study new methods of organizing, especially in emergent contexts such as hackathons.

5.1. Prioritizing Multiple Goals and Outcomes in Innovation

Our findings suggest that iterative coordination causes an organization to *implicitly* prioritize value over novelty in innovation. This prioritization is implicit: Even when an organization believes in preserving novelty in outcomes, the process of iterative coordination may drive it to ultimately favor value. With additional deadlines, iterative coordination drives integration at the cost of specialization—processes that we document in the software code from the primary field experiment and in the product development activities in the follow-on laboratory experiment. These processes, in turn, associate with outcomes of value over novelty. This finding stands out because iterative coordination itself does not explicitly define a priority for either novelty or value.

Although the literature on managing multiple performance goals examines the influence of frequent and repeated coordination on performance broadly defined (Gavetti et al. 2012, Knudsen and Srikanth 2014, Levinthal and Workiewicz 2018), coordination's influence on novelty and value in outcomes remains underexplored. To contextualize our findings with respect to prior work, we can consider the literature on goal setting in creativity, which predicts that frequent and repeated coordination on innovation goals yields both greater novelty and value in outcomes (Carson and Carson 1993, Byron and Khazanchi 2012). The assumption is that the process of revisiting and iterating in innovation-oriented goals reinforces individual motivation, an important antecedent to innovative outcomes (Cromwell et al. 2018). Especially for more dynamic organizational activity, such as new product development, a process for iterating on goals in innovation is argued to help keep organizational members aligned in their pursuit of innovation in the face of failures (Alexander and Van Knippenberg 2014). Because failures in the pursuit of innovation can have a demotivating effect, scholars have argued in favor of using frequent and repeated coordination to help promote novelty and value in end outcomes (e.g., Amabile and Pratt 2016).

Given the theorized benefits of goal-setting-driven motivation for producing novelty and value in end outcomes, we could also expect these benefits to manifest in other dimensions of innovation performance. However, our empirical evidence suggests no differences in general productivity from our coordination interventions in innovation projects, both in terms of aggregate code contributions in the primary field study and completeness of the final task in the follow-on laboratory study. Although we do not directly measure effects on motivation, it is theoretically unclear from prior work whether goal setting's motivational benefits should manifest themselves in terms of greater novelty *and* value—the two constituent parts of innovation—or whether the effect is channeled into just one of these two outcomes, such as value. In other words, it is not clear from prior literature whether we should expect motivation's effects to improve innovation across end outcomes as a whole or whether it more narrowly drives parts of innovation, such as value.

In contrast to the predictions of the literature on goal setting in creativity, our findings suggest that frequent and repeated coordination on innovation goals leads to improved value, but to *reduced* novelty. This insight derives in part from our conceptualization of the innovation process as the pursuit of the *distinct* goals of novelty and value. Prior work often conceptualizes creativity and innovation as a joint outcome of novelty and value (Oldham and Cummings 1996, Shalley and Perry-Smith 2001). Accordingly, the joint view inherently underemphasizes the costs associated with coordinating individuals on potentially competing goals of novelty and value. Knudsen and Srikanth (2014) describe coordination costs as those associated with communicating among multiple organizing members, which inherently takes time away from specialist work. Our analysis suggests that although greater coordination on innovation goals may help drive integration and the pursuit of value, it does so at the cost of specialization and the pursuit of novelty as a *distinct* goal. As a result, even if organizational members feel they can choose to pursue either novelty or value, we argue that iterative coordination and related management practices drive an iterative process that ultimately results in the implicit prioritization of value over novelty. This reprioritization toward value over novelty is especially salient in supplemental empirical findings from the primary study that distinguish originally stated objectives from what gets developed in final projects.[39]

Having established the importance of studying innovation as distinct goals of value and novelty, our findings also contribute to the behavioral theory of the firm (BTF) by explaining how satisficing occurs across multiple, potentially conflicting, goals in innovation. A long stream of research on the BTF long posited that decision makers respond to search across multiple performance objectives by satisficing (Simon 1955, Posen et al. 2018), or choosing the first alternative they expect to satisfy all objectives (Gavetti et al. 2012). Although this literature explores satisficing behavior with respect to a single objective (Posen et al. 2018), exactly how organizations satisfice across multiple objectives remains unclear, with Greve and Gaba (2020, p. 323) noting that "the theoretical treatment of adaptive behavior amid multiple goals and performance aspirations has only begun." This research has been limited in part due to the ambiguity of performance feedback when simultaneously pursuing multiple objectives. For example, it is unclear how organizations respond when an alternative is judged to be a success on some objectives, but a failure on others (Levinthal and Rerup 2021). Given mixed signals of success and failure across multiple objectives, on which objective does the organization focus? In early work on this domain, Gaba and Greve (2019, p. 647) suggest that "the goal perceived as more important for survival gets priority and triggers stronger reactions." However, in the pursuit of innovation, it is unclear whether value or novelty would or should be prioritized to ensure organizational survival. Does an organization embrace outcomes of high value that would be of known interest to customers, or does it instead focus on novelty to distinguish itself among customers? This fundamental ambiguity exists for both innovating organizations and the scholars that seek to study those organizations.

Our findings suggest that satisficing in innovation occurs specifically in favor of value and against novelty. What is particularly striking about our findings is that, regardless of an organization's starting point in the balance between novelty and value, the organization deprioritizes novelty. When interpreting feedback across multiple objectives, Levinthal and Rerup (2021) suggest that a process of self-enhancement may occur, where organizations prioritize the dimensions on which they are performing the strongest. Such a response follows from the intuition that the very first alternative that is deemed satisfactory is ultimately chosen (Gavetti et al. 2012). In other words, if an organization is already performing relatively better on novelty than value, why not focus on novelty? A promising area of future inquiry would be to understand exactly why organizations do not adopt a self-enhancement perspective to emphasize novelty under frequent and repeated goal coordination.

5.2. Methodological Contributions

As a second primary contribution, we are among the first researchers in this literature, to the best of our knowledge, to measure organizational innovation in real time using version-control software. Tracking software code development provides significant advantages toward studying innovation across individuals, firms, and time, which we detail below.

First, it allows for the measurement of productive and creative tasks *across individuals*. In particular, extant literature in strategy for how organizations generate innovation commonly brings up the concepts of specialization and integration as processes by which innovation might emerge (Ethiraj and Levinthal 2004, Levinthal and Workiewicz 2018). However, prior empirical work—for example, using patent data or other final outputs—often cannot directly measure how much individuals in the organization work together and integrate their ideas across individuals in real time. We can track in the software code development process when code is being integrated across individuals (i.e., *Code Integration Action*). There are plenty of opportunities for future studies to use code tracking of across-individual activity—for example, integration across managers and subordinates (e.g., Reitzig and Maciejovsky 2015 and Ghosh et al. 2020), division of labor in innovation production (e.g., Lawrence and Lorsch 1967, Knudsen and Srikanth 2014, and Ghosh 2020), and the extent to which ideas are combined with one another or discarded from consideration (e.g., Girotra et al. 2010).

Second, it allows for the measurement of innovative activity *across firms*. Our measure of *Advanced API Specialization* reflects whether firms put effort into learning and utilizing sophisticated developer tools offered by Google. Although we use this measure in a relatively limited way, this measure can be interpreted more broadly in the context of a rapidly growing literature on multisided platforms (Wen and Zhu 2019, Pan Fang et al. 2021). This set of developer tools can be thought of as a multisided platform, where our firms are the complementors to the Google platform (and consumers are the other side of the platform). Future studies can use this type of data to identify when and how firms draw on external platforms or tools for the development of innovation. For example, these data could be used to measure precisely the knowledge-driven factors leading to phenomena such as how entrepreneurs draw on knowledge from larger organizations, particularly as they depart from a larger organization to launch their own firm (Kacperczyk and Marx 2016).

Third, it allows for the measurement of individual innovative activity *across time*. Despite the common notion that innovation might arise in a flash of genius, scholars recognize that innovation is often an emergent process that occurs over time (Amabile and Pratt 2016, Hu and Bettis 2018). In our experimental setting, we can allow for our developers to innovate over the course of a day, but in most settings, this innovative process runs over the course of months or years. By longitudinally tracking software code across time, we can capture with greater granularity how innovation arises. In particular, we show that iterative coordination has an additive or cumulative effect of pushing firms more toward integration activity and away from individually specialized activity. Future work can use this methodology perhaps even on a much grander time scale to look at how other organizational characteristics, such as structure, or individual characteristics, like managerial cognition, change the patterns of the innovation process over time (Schilke et al. 2018).

5.3. New Methods of Organizing and Hackathons

Finally, we respond to recent calls from the literature to study new forms in organizing (Levine and Prietula 2014, Puranam et al. 2014, Burton et al. 2017), particularly in the context of innovation at hackathons. Over the last several years, hackathons have emerged as an important context for organizational innovation (Pe-Than et al. 2019, Pan Fang et al. 2021), with start-ups and incumbents alike leveraging the context to develop ideas and projects of high value and novelty. Lifshitz-Assaf et al. (2020, p. 4) note that because "hackathons adhere to non-hierarchical and open ways of organizing, no clear process, structure or roles [are] defined." In such an environment, organizations need new structures and practices to sustain high performance (Meyer and Zucker 1989). In the absence of traditional process, structure, or roles in hackathons, new organizing practices, like iterative coordination, may be necessary to manage innovation in the highly time-constrained setting of a hackathon.

Where prior work finds that coordination frameworks from contexts brought in from outside a hackathon may be detrimental to innovation (Lifshitz-Assaf et al. 2020), our findings suggest that the signature Agile practice of iterative coordination can be effectively employed to produce complete innovation projects at hackathons. In contrast to prior findings suggesting that rapid coordination efforts in a hackathon setting may lead to project failure (Lifshitz-Assaf et al. 2020), we find that the high degree of coordination engendered by iterative coordination does yield complete, functioning output by the end of a hackathon. It is important to reflect on why our findings differ. Whereas Lifshitz-Assaf et al. (2020) suggest that coordination frameworks may cause firms to bound themselves to ultimately unattainable goals of both high value and high novelty, our findings demonstrate that iterative coordination allows firms to reprioritize their goals over time, allowing critical adjustments to be made regarding the degree of novelty that is acceptable to them over the course of their projects.

Of course, it is important to note that iterative coordination does not necessarily yield products that are judged to be more complete than a baseline of minimal coordination; rather, iterative coordination yields successfully completed projects that bias toward value instead of novelty. This raises the question: What may motivate a firm to use iterative coordination to manage innovation projects at a hackathon? One factor may be to appropriately balance value against novelty in nascent projects. For instance, in corporate hackathons, projects that are judged to be too new and that are difficult to relate to a focal firm's business value objectives are often abandoned after a hackathon and are not pursued for further development (Nolte et al. 2018). In this sense, an idea or proposal that is judged to be too new while offering little value has little chance of being implemented by the firm (Ahuja and Morris Lampert 2001). Although traditional coordination methods may stifle projects at a hackathon, iterative coordination could allow firms to inject necessary value into ideas, increasing their likelihood of being implemented.

5.4. Limitations and Future Work

We conclude by noting limitations to the present study and opportunities for future work. First, we note that although many contexts in which iterative coordination is used deeply prioritize innovation (Rigby et al. 2016a), iterative coordination itself does not directly frame a need for novelty or value. Given our interest in evaluating iterative coordination as it is practiced, we intentionally do not frame any of the three iterative coordination questions with an additional requirement that the organizational goal (or its subsequent output) be novel or valuable. Varying the composition of iterative coordination questions to articulate an explicit need for novelty, value, or other outcomes is a promising area for future study. In addition, given our empirical focus on entrepreneurial innovative settings, our theoretical treatment of iterative coordination assumes its application in settings in which existing technical knowledge is comparatively limited; examining the applicability of our theory to settings in which technical knowledge is well established would be a worthy topic of future inquiry.

There are several potential organizational moderators to iterative coordination left to be explored. The distribution of the quantity or quality of individual specialization—whether individual contributions are evenly divided or concentrated in one individual—might affect the degree of integration that would occur when practicing iterative coordination. The size of the practicing organization may also moderate the effects of iterative coordination—for example, large organizations have a greater need for coordination (Aggarwal et al. 2020)—but at the same time, iterative coordination may not scale well to a large number of participants in a meeting. In addition, characteristics of the goals themselves, such as being too specific or too challenging, may potentially moderate iterative coordination's influence on innovation (Ordóñez et al. 2009). Given limitations on statistical power in the present study, we recommend the exploration of these moderators for future study.

Finally, across practical contexts, there may be heterogeneity in the effects of iterative coordination. For instance, the design of physical products and/or services, as opposed to software development, may have fundamentally different organizing needs. This may arise due to alternate environments of complexity and modularity inherent in the architecture of the offering (Ulrich 1995). Of course, differences in underlying complexity and modularity may call for different methods of coordination in innovation (Baldwin and Clark 2000, Ethiraj and Levinthal 2004).

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Endnotes

[1] Following convention in the literature (e.g., Amabile 1983 and Kaplan and Vakili 2015), we define an innovation as something that is both valuable and novel. Under this definition, innovation emerges as a product of the pursuit of both novelty and value. Novelty for its own sake—such as a mobile application that translates words from an alien language to English—offers little value that can be created and captured by the organization. An innovative idea must involve the potential for an organization to commercialize it, as has been long assumed in prior literature (e.g., Kaplan and Vakili 2015). On the other hand, a simple mobile-payments application that is valuable, but not novel, does not qualify as innovation per the definition we follow.

[2] A stand-up meeting allows organizational members to discuss organizational priorities and tasks in a short meeting, often while standing up. Figure 1 situates stand-up meetings within a family of related practices.

[3] Recent organizations literature loosely relating to the construct of iterative coordination includes Obloj and Sengul's (2020) study of managing multiple performance objectives in the context of the French manufacturing sector. Here, the authors find that frequent face-to-face meetings among executives help them manage their multiple goals, helping address trade-offs between their goals and iterate toward outcomes that are acceptable to all parties involved. Similarly, in analyzing results from their study of interdependent task environments in the automobile industry, Hu and Bettis (2018) argue in favor of using iterative design approaches to address unforeseen interdependencies across objectives. Neither study fully characterizes the construct of iterative coordination that we formally introduce in Section 2, and, more importantly, they offer no predictions for how frequent meetings to prioritize among multiple goals may shape innovation goals and outcomes.

[4] We report how we determined our sample size, all data exclusions (not applicable), all manipulations, and all measures in the study.

[5] The use of the name "Agile" to describe this family of practices officially dates to February 2001, when 17 software developers gathered in Snowbird, Utah, to develop the principles behind what would become the Agile Manifesto (Beck et al. 2001). This work inspired a family of alternative interpretations and derivative frameworks that sometimes are referred to as Agile, despite not exactly matching the original Manifesto. Our use of the term Agile refers to the common aspects of these varied interpretations, and not specifically to the Agile Manifesto.

[6] In Agile practice, projects are time-bound within "sprints," which break up software development cycles into smaller chunks of time. In question 3, the use of the word "project" refers to the end of an Agile sprint.

[7] The theory follows from Grant (1996): integration serves the purpose of realizing value for the firm. An alternative theoretical viewpoint suggests that the integration of existing knowledge across boundaries can also help generate novelty (Rosenkopf and Nerkar 2001). This alternative perspective focuses on how existing components or knowledge can be recombined in new ways (Fleming 2001). We intend for the theory we put forth to apply to settings in which existing knowledge is limited—and where this alternative viewpoint is less applicable—such as the entrepreneurial contexts in which practicing firms most commonly implement iterative coordination (Ott et al. 2017). For these entrepreneurial contexts, we take the perspective that novelty primarily emerges through the development of new knowledge, which must be continuously generated by specialists, rather than the integration of existing knowledge.

[8] This trade-off is especially salient in our empirical setting of software development, which involves multiple specialist coders who

attempt to create a novel and valuable software application. They must divide and allocate their time and attention to either integrating with each other's existing knowledge or to specializing on their own to create new knowledge.

[9] As we are interested in developing new theory based on the phenomenon of iterative coordination, we avoid a formal statement of hypotheses, which are more appropriate for empirical studies of mature bodies of theory (Edmondson and McManus 2007). Instead, we offer theoretical predictions to guide the interpretation of results from our primary study in Section 3.

[10] Google LLC is the largest subsidiary of Alphabet Inc.

[11] A firm, or, more broadly, an organization, is defined as a bounded system where more than one agent shares system-level goals and where each constituent agent is expected to make a contribution. In this conceptualization, "boundaries and goals jointly identify organizations uniquely" (Puranam et al. 2014, p. 164). Using this definition, Puranam et al. (2015, p. 381) note that "there is no basis (besides convention) on which one can say that a three-person firm is an organization but a four-person team is not; to the extent these are goal directed multi-agent systems, they are both organizations."

[12] Scholars have long recognized the potential limitations to the concept of an "organizational goal" (Cyert and March 1963, Simon 1964, Gaba and Greve 2019). For instance, although phrased as a singular objective, an organizational goal often encompasses multiple demands that must be simultaneously satisfied (Simon 1947, Gaba and Greve 2019). This is especially salient in our context of the development of new technology, where managing multiple performance objectives simultaneously is a defining characteristic (Hu and Bettis 2018).

[13] Academic research grants supported the operational expenses of the experiment, event, and venue.

[14] Online Appendix A1 documents the materials used to recruit participants for the hackathon.

[15] Online Appendix A1 presents a post hoc analysis of statistical power.

[16] The Git, specifically GitHub, interface allows us to see which member contributed to which portion of the project over time. As each member writes code, they submit it to the shared GitHub repository that represents the body of code for the overall project by the firm.

[17] Online Appendix A1 presents an example of Google materials communicating this guidance.

[18] Online Appendix A1 provides the scripts used by mentors.

[19] In the final check-in, two hours before the end of the competition, only the first and third questions were asked.

[20] We instructed mentors to provide only *technical* feedback if *directly requested*. Mentors in both the treatment and control conditions restricted themselves in this way and did not proactively speak to firms outside of the formal interventions. By limiting feedback in this way, we exclude any normative guidance on the value or novelty of the product being developed.

[21] Peer firms included Twitter, Inc., and Cisco Systems, Inc.

[22] As a necessary condition to conduct the primary field study, the researchers and their university executed a contract with the corporate partner on this study that specifies requirements for nondisclosure of individual data. Online Appendix A1 provides additional information about the terms of this contract. In addition, the research team is available to answer any questions that arise about the experimental design of the primary field study. The data from the follow-on laboratory study do not have this type of contractual restriction and are made publicly available.

[23] As an additional check on the efficacy of randomization, we obtain and code data on the starting goals of the firms participating in the hackathon. By starting goal, we mean the initial description of a novel and valuable technical product that they wished to create by the end of the competition. We find no statistically significant difference between firms in the treatment condition and control condition in their *Starting Goal Value* or *Starting Goal Novelty*. The comparability of these measures across the two conditions provides further confidence in the efficacy of the experimental randomization. Online Appendix A2 details these analyses. We thank an anonymous reviewer for this suggestion.

[24] Git is a free, open-source, version-control system that enables distributed software development. Version control keeps track of all the changes developers individually make to the firm's "repository" of source code for a project. Git archives each firm's source-code repository at each update. Should errors be made during the development process, a developer can easily restore the firm's repository to that of a prior "commit" or update, which stores a snapshot of the firm's repository at the time the update was made.

[25] Online Appendix A3 presents the correlation matrix.

[26] An additional requirement of Google LLC's co-sponsorship of our event was the inclusion of a tutorial on advanced features of one of the competition's required app-development toolkits. This was offered to all firms late in the day, immediately after the second mentor check-in, and attendance was optional.

[27] $Treatment_i$ and $Post_t$ were not independently estimated in the model because they are collinear with the more precise fixed effects of α_i and δ_t, respectively.

[28] Following the best practice of recent field experimental research (e.g., Chatterji et al. 2019), we took a conservative approach using firm fixed effects to control for the chance possibility that there might be lingering unobservable time-invariant, firm-level heterogeneity, even after randomization. These firm fixed effects subsume all of the control variables that we use in the prior analysis of firm outcomes—for example, *Graduate Degree*. We need a pretreatment period to include firm fixed effects to ensure econometric identification of the key coefficient, which is the effect of treatment in the post period, or the coefficient on $Treatment_i \times Post_t$. When we include fixed effects, $Treatment_i$ is collinear with firm fixed effects and thus omitted from the regression estimation because it cannot be identified.

[29] Online Appendix A2 presents the post hoc mediation analysis.

[30] Online Appendix A4 presents the full details of the task.

[31] Pre-registration documentation is available at: https://osf.io/c7qmw/?view_only=e53498ecf5004ac695f830da6752497a.

[32] Using the G*Power software (Faul et al. 2007), the a priori power analysis suggests that we target a sample size of more than 42 divided across the three conditions. Because this varies depending on the effect of interest, we overshoot this number and use a sample of 70 in the actual follow-on laboratory experiment.

[33] To confirm the randomization, we confirm that there are not statistically significant differences across the three experimental conditions in *Age*, *Gender*, *Graduate Education*, *Current Student*, *Any Experience*, and *Years of Experience*. Online Appendix A4 presents further detail on this randomization check.

[34] To avoid deception, research team members introduce themselves as a member of the research team.

[35] The instruments, data, and statistical code for the follow-on laboratory experiment are available through the Open Science Framework portal: https://osf.io/c7qmw/?view_only=e53498ecf5004ac695f830da6752497a.

[36] Teams were explicitly encouraged to start with preliminary individual work on the individual sketch paper and wait before committing to a final product on the whiteboard. Accordingly, the timing of using the whiteboard indicates how long it took for the

team to finalize its decision about which product idea to focus on and use for the presentation slide (i.e., whiteboard drawing). Research assistants identified the moment when teams first use the whiteboard to confirm that the experimental design worked in the way we intended. In all cases, the first use of the whiteboard occurred only after individual sketches were completed and after the teams had one or multiple explicit conversations about which ideas on sketch paper should be combined and which idea to finalize and implement on the whiteboard.

[37] Online Appendix A4 presents the correlation matrix.

[38] We calculate person-hour productivity in condition 1 as the 1.8 pages of sketches generated by the average participant in an hour. In condition 2, person-hour productivity is 1.4 pages of sketches per hour.

[39] Online Appendix A2 presents this supplemental analysis.

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