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Understanding and Improving Coordination Efficiency in the Minimum Effort Game: Counterfactual- and Behavioral-Based Nudging and Cognitive Modeling

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UNDERSTANDING AND IMPROVING COORDINATION EFFICIENCY IN THE MINIMUM EFFORT GAME: COUNTERFACTUAL- AND BEHAVIORAL-BASED NUDGING AND COGNITIVE MODELING

A Dissertation submitted in partial fulfillment of the requirements for the degree of Doctor of Philosophy

by

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ABSTRACT

Hough, Alexander R. Ph.D., Department of Psychology, Wright State University, 2021. Understanding and Improving Coordination Efficiency in the Minimum Effort Game: Counterfactual- and Behavioral-Based Nudging and Cognitive Modeling

Individuals often need to coordinate with others to pursue and achieve goals. However, individuals often fail to coordinate on any choice or on efficient (i.e., higher reward) choices. Researchers addressing coordination failure often used invasive methods ranging in complexity and generalizability with minimal success. There are also no clear measures for coordination behaviors. Here, I used a more parsimonious and generalizable method: Using counterfactuals (i.e., hypothetical outcomes had they or other players chosen differently) to nudge (i.e., indirectly guide and allow for free choice) individuals towards choosing options that are more likely to result in efficient coordination. I simulated a coordination situation using a modified minimum effort game (MEG) with counterfactual manipulations and included effort-related trait measures. Participants played the MEG with other humans or bots based on a mathematical model from game theory. In the first experiment I used neutral bidirectional counterfactuals (i.e., outcomes if own choice or minimum was one lower or higher). I found higher coordination efficiency compared to previous experiments and no relationships with trait measures in human or bot groups. In the second experiment with only bot groups, I found evidence that those receiving upward counterfactuals performed better than those receiving downward. There was also evidence that one human can encourage other players to make
more efficient choices with behavior-based nudging (i.e., signaling) regardless of counterfactual condition. Since bot behavior was artificial, I developed a cognitive model within ACT-R that was able to approximate human behavior and processing in the MEG better than competing models. This dissertation contributed to the coordination literature by introducing: 1) novel methods to measure coordination, efficiency, and signaling, 2) a novel method to nudge individuals towards coordination efficiency, and 3) a novel model of coordination within a cognitive architecture that better explains behavior, cognitive processes, and group dynamics.
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I. Introduction

The goal of this dissertation was to better understand coordination efficiency, if and how an information manipulation influences coordination efficiency, and the interplay between the manipulation and dynamic repeated interaction with other players. To achieve these goals, a new technique was explored in two experiments within the Minimum Effort Game (MEG; van Huyck, Battalio & Beil, 1990) to test the effectiveness of a parsimonious information intervention (i.e., counterfactuals) designed to nudge individuals towards choices that could lead to better coordination with more efficient outcomes. The two experiments utilized both behavioral and model-based data. The MEG, a performance-based effort avoidance measure called the Demand Selection Task (DST; Kool, McGuire, Rosen, & Botvinick, 2010; Juvina et al., 2018), and effort related trait measures were used to collect behavioral data. For the model-based data, a learning model from game theory, the Experience Weighted Attraction model, (EWA; Camerer & Ho, 1999) was extended to correspond to the current game structure, serve as a synthetic player, and explore the behavioral data from the MEG. In addition, a cognitive model was developed and implemented in the ACT-R cognitive architecture after the first two experiments due to the inadequacy of the extended EWA model to simulate and explain human behavior.

Coordination is an interdisciplinary domain that spans across fields such as biology, economics, computer science, and of course, psychology. Malone and Crowston (1994) defined coordination as “managing dependencies between activities” (p. 4), where
dependences include things like sharing resources, assigning tasks, and working around constraints. In humans, coordination often involves emotion, motivation, incentives, and biases at the individual or group level (Malone & Crowston, 1994). This dissertation is focused on a type of coordination and context within the larger coordination domain: group coordination in humans where outcomes are a function of behavior at the individual and group level, resulting in unique or asymmetric outcomes for each individual. Groups of individuals often fail to coordinate (i.e., coordination failure) in this type of situation (Cooper, DeJong, Forsythe, & Ross, 1990, 1994, Camerer, 2003, Riechmann & Weimann, 2008; van Huyck et al., 1990, 1991), which is often attributed to the lack of a coordinating device or focal point that increases the saliency of efficient equilibria equally for all players (Blume et al., 1998; Mehta et al., 1994). This failure includes two related, but distinct phenomena: the failure to coordinate and the failure to coordinate on efficient equilibria (Riechmann & Weimann, 2008). This distinction is often ambiguous in the literature, so an explicit distinction is made here.

Coordination concerns the degree players settle or converge on a single equilibrium or choice. In this dissertation, coordination is approximated by calculating variance or the range of the choice distribution within the group (e.g., lower means better coordination). Players can coordinate on any equilibrium or choice. On the other hand, coordination efficiency (i.e., coordination on efficient equilibria) typically refers to how close this equilibrium is the highest possible payoff and can be considered as a point on a continuum between the lowest and highest possible payoffs.

A degree of coordination often occurs over time, making reaching efficiency and increasing efficiency after stabilization at inefficient equilibria more interesting topics of
study (e.g., Brandts & Cooper, 2006; Brandts, Cooper, & Weber, 2014; Brandts, Cooper, Fatas, & Qi, 2015; Chaudhuri, Schotter, & Sopher, 2009; van Huyck et al., 1991). Several techniques have been applied to address these two issues with varying degrees of cost, effort, and effectiveness (Brandts et al., 2014, 2015; Cooper et al., 1990; Sahin, Eckel, & Komai, 2015; Weber, 2001; van Huyck et al., 1990; van Huyck, Gillette, & Battalio, 1992; van Huyck, Battalio, & Beil, 1993). These two issues were addressed in this dissertation using the MEG as a simulated asymmetric coordination scenario where: 1) individuals make choices simultaneously or without knowledge of other’s choices, 2) they cannot communicate beyond making choices, and 3) the resulting outcomes are asymmetric or unique for each individual depending on their choice, an order statistic (e.g., mean, median, or minimum), and the choices of the other members of the group. In the MEG, outcomes are determined by the “weak link” or minimum choice of the group. Players typically converge towards the minimum choice resulting in inefficient outcomes for all members of the group. This type of coordination situation was chosen because it is very challenging to achieve coordination without an explicit coordination device or salient focal point (Blume, DeJong, Kim, & Sprinkle, 1998; Mehta, Starmer, & Sugden, 1994) and other players choices, particularly the lowest or weakest choices, can become the focal point and influence other players to coordinate on an inefficient choice (Brandts et al., 2014, 2015; van Huyck et al., 1990). Increasing coordination efficiency in this particularly difficult setting would be promising and could potentially generalize to other asymmetric coordination situations as well. Previous research used several techniques aimed at increasing coordination efficiency and some were successful to a degree; however, they involve changing the coordination situation, require substantial effort, and
only work when everything is “just right”. For instance, Brandts and colleagues (Brandts et al., 2014, 2015) found that allowing a leader to help other players could improve coordination efficiency, however, if this help was taken away too early it was actually worse than no help at all. In addition, leaders often stopped helping because it incurred a cost that was higher than the benefit.

In this dissertation, I used a new technique to improve efficiency: providing individuals with counterfactuals. A counterfactual is defined as a specific forgone outcome that could have occurred if a different choice was made (e.g., Byrne, 2016; Kahneman & Miller, 1986). Counterfactuals can focus on aspects of a situation that are within one’s control (e.g., one’s own choices) and/or outside of one’s control (e.g., choices of other’s that affect your outcome), and tend to involve how to improve future outcomes (Markman, Gavanski, Sherman, & McMullen, 1993; Roese, 1997; Roese, Hur, & Pennington, 1999). According to previous research on counterfactual thinking and coordination in game theory, these counterfactuals could influence coordination and coordination efficiency. For instance, when counterfactuals highlight a better hypothetical outcome, they may indirectly nudge or guide individuals (Thaler, Sunstein, & Balz, 2013) towards choices that could lead to more efficient coordination. The following sections provide background literature on coordination games, effort-related preferences, and nudging techniques (i.e., information-based counterfactuals and behavior-based signaling).

**MEG and Coordination**

The MEG involves multiple players who select a level of effort between one and seven. All player payoffs are determined by the lowest effort level or minimum in the
group. The payoff matrix (Table 1) is set up so that coordinating on the minimum results in higher payoffs, particularly when coordinating at higher levels of effort.

Table 1

**MEG payoff matrix (Adapted from van Huyck et al., 1991)**

<table>
<thead>
<tr>
<th>Player's Effort Choice</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
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<tbody>
<tr>
<td>1</td>
<td>70</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2</td>
<td>60</td>
<td>80</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>3</td>
<td>50</td>
<td>70</td>
<td>90</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>4</td>
<td>40</td>
<td>60</td>
<td>80</td>
<td>100</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>5</td>
<td>30</td>
<td>50</td>
<td>70</td>
<td>90</td>
<td>110</td>
<td></td>
<td></td>
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<tr>
<td>6</td>
<td>20</td>
<td>40</td>
<td>60</td>
<td>80</td>
<td>100</td>
<td>120</td>
<td></td>
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<tr>
<td>7</td>
<td>10</td>
<td>30</td>
<td>50</td>
<td>70</td>
<td>90</td>
<td>110</td>
<td>130</td>
</tr>
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The game structure provides seven coordination points or Nash equilibria (Nash, 1951) which are represented diagonally (in bold) from the payoff of 70 to 130. A Nash equilibrium specifies what a rational player should select to maximize their own utility or payoff according to all the other strategies available and the choices made by other players (Camerer, 2003). These Nash equilibria are “Pareto ranked” from one to seven (from left to right) according to their payoff. A Pareto equilibrium is a strategy that essentially maximizes the sum of payoffs for all players without increasing the payoff of one player at the expense of another (Camerer, 2003). In the MEG, there is some ambiguity regarding efficiency. Here, I explicitly distinguish between two types: local and global efficiency. I define local efficiency as maximizing the payoff for a given round (i.e., Nash equilibrium) by selecting the minimum. This is a short-sighted approach and is optimal for one round of play. I define global efficiency as striving for the highest possible payoff by choosing closer to 7. This is more similar to a Pareto equilibrium, as it considers the payoffs of everyone in the group. It is a more long-term strategy and is
more ideal when players are willing to take some risk by choosing higher in order to bring up the payoffs of everyone in the group. The previous references to efficiency are better aligned with global efficiency and since it is the focus of this dissertation, the term efficiency will continue to refer to this global efficiency.

There are two salient choice options at the start or first round of the MEG: a payoff dominant choice (i.e., seven) that could result in both the highest payoff (i.e., 130) or the lowest possible payoff (10) and a risk dominant choice (i.e., one) that does not depend on other player’s choices and always results in a guaranteed payoff (i.e., 70). van Huyck et al. (1990, 1991) demonstrated that players start the game using strategies such as the payoff dominant and risk dominant choices, but learning occurs during the game, which causes deviation from these initial strategies. Players often choose somewhere between the payoff and risk dominant choice in the first round, and in subsequent rounds, the minimum effort may become a third salient focal point as it determines payoffs.

![Figure 1](image.png)

*Figure 1*. Observed negative trend in effort from Leng et al. (2018) and convergence towards one from van Huyck et al. (1990). Error bars for van Huyck et al. (2010) are 95% confidence intervals. Note: the trend for Leng et al. (2018) is reproduced from first and last round effort choice data.
This is a simple explanation for the frequently observed negative trend in effort over time (Leng, Frieson, Kalayci, & Man, 2018, van Huyck et al. 1991) that often results in convergence towards one (van Huyck et al., 1990) in ten rounds of play (Figure 1).

In three different variations where players were given feedback about the minimum for each round, van Huyck et al. (1990) found outcomes are sensitive to payoffs, group size, and the potential risk or cost of allocating more effort than the minimum. However, players were only given the minimum choice per round and were not able to determine what other players were choosing. Therefore, players might assume all other players choose the minimum. van Huyck et al. (1990) addressed this issue by including a condition where players were given complete outcome information (i.e., the complete distribution of player choices and the minimum), however, this had little effect and players still converged to one. This finding was replicated and reported in Camerer and Ho (1998). In addition, Leng, et al. (2018) found additional outcome information and continuous time treatments with dynamic choices had no significant effects on coordination behaviors beyond only giving the minimum as feedback in typical discrete time with players choosing simultaneously.

The coordination game literature provides three additional points relevant to the MEG and this dissertation. First, players initial choice is very important, as it is very difficult to improve efficiency once it has already converged on an inefficient option (e.g., Chaudhuri et al., 2009; van Huyck et al., 1990). Typically, groups of players have a higher dispersion of first round choices compared to later rounds (e.g., Camerer & Ho, 1998; van Huyck et al., 1990), suggesting individual differences in strategies or preferences. Players might use previous strategies that are inappropriate for the current
context and even if they adapt to the current environment (Cooper & van Huyck 2018), that previous strategy or rule likely serve as a reference point (Costa-Gomes et al., 2009).

Second, people may have effort preferences or evaluate the benefits of applying effort in a given situation. In game theory, steps of reasoning refer to the extent of thinking prior to making a decision (Camerer, 2003). For instance, thinking a player might do “X” is one step and considering they might do “Y” because they think you are going to do “Y” involves two steps. Engaging in these steps of reasoning is taxing on memory and may be perceived as aversive or costly (Beard & Beil, 1994; Duffy & Nagel, 1997; Ho et al., 1998; McKelvey & Palfrey, 1992; Nagel, 1995; Rubinstein, 1989; Schotter, Weigelt, & Wilson, 1994; van Huyck, Wildenthal, & Battalio, 2002). In terms of efficiency, one step of reasoning is a good strategy as other player’s behavior is difficult to predict and people are sensitive to wasted effort (Camerer, 2003; Haruvy & Stahl, 2007; Ho & Weigelt, 1996). In repeated play, players might apply little effort towards initial choices, in order to size up the group before applying too much effort.

Third, players might attempt to signal cooperation at an efficient equilibrium by choosing higher effort than other players and may continue to do so as long as it is effective (Charness, Gneezy, & Henderson (2018). Interestingly, this signaling is more effective when it is costly to the signaler (e.g., time, effort, or currency) and other players are aware of the cost the signaler is paying to bring up efficiency (Spence, 1978). This is similar to Brandts et al.’s (2014, 2015) leadership experiments where leaders can improve efficacy by helping players (i.e., paying a cost) after they converged on an inefficient equilibrium. However, their experiments utilized communication or the leader publicly selecting first and if leaders stopped helping too early, it was actually worse than not
helping at all, suggesting signaling needs to be persistent to be effective. Rather than trying to improve efficiency, players might see the risk in choosing higher effort (e.g., Cachon & Camerer, 1996) or might negatively reciprocate when other player(s) choose a lower level of effort than they do (e.g., Offerman, 2002).

Coordination, coordination efficiency, and these additional points from the coordination literature are difficult to address in the MEG, because it involves a large number of players and possible equilibria. Previous MEG experiments (Bortolotti, Devetag, & Ortmann, 2016; Leng et al., 2018; van Huyck, 1990, 1991) typically focused solely on effort and minimum effort across rounds to analyze efficiency and signaling behavior. Leng et al. (2018) went a step further by comparing the frequency of individual effort changes and group minimum changes, and identified signaling as alternating between the minimum and higher effort. They found that this signaling behavior can increase the group minimum, which they label as a measure of efficiency. However, the increase is small at best or non-existent depending on how much information is provided about other player choices. Similar to van Huyck (1990, 1991), they also found that the minimum serves as a focal point early on and serves as an anchor for subsequent rounds. Bortolotti et al. (2016) also made some methodological contributions by identifying and analyzing the behavior of weak links within groups (i.e., the player setting the minimum) to assess their influence on the group’s coordination efficiency. They found weak links were the source of coordination failure early in the game. Despite the contributions from Bortolotti et al. (2016) and Leng et al. (2018), several methodological gaps remain. There are no well-defined methods to measure coordination, coordination efficiency, and signaling. Analyses also leave out payoffs as a dependent measure and do not address the
possible influence of pre-game preferences for influencing initial effort choices that set the stage for subsequent coordination. Furthermore, research has not analyzed behavior at the aggregate, group, and individual levels. These methodological issues are addressed in this dissertation, particularly in the first experiment, to better understand coordination behaviors, methods to measure them, and to gather converging evidence from different levels of analysis.

Another unresolved issue in the MEG is effort. There is no actual effort and is therefore difficult to relate to effort-related preferences and signaling behavior that involves costs. It is worth noting that a recent study used actual effort in a MEG (Bortolotti et al., 2016), however, there was substantial deviation from the typical game structure and effort was based on the number of errors made during an individual coin sorting task. Measuring errors is more related to the ability to perform a coin sorting task than it is willingness to expend effort, which was the focus of the original MEG. To address this effort problem and gaps in measuring and analyzing coordination efficiency, effort preferences, and signaling, a real-effort MEG (referred to as the REMEG) was developed for this dissertation.

In the REMEG, players selected a level of effort and then completed an arithmetic problem that involved adding single digit numbers without the use of a calculator or paper and pencil. The base problem for the effort level of 1 was adding two numbers and each additional effort unit added one additional number to the problem (i.e., max of eight numbers for effort selection of 7). Incorrect solutions resulted in a payoff reduction for the individual instead of the group to keep willingness to expend effort separate from errors. After making a choice and giving an answer to the arithmetic problem, players
received results. Similar to the full information condition from van Huyck (1990), players were given information about other players' choices and their payoffs. This was meant to complement the counterfactual manipulation so that players could assess whether other players were responding to the counterfactuals or any resulting signaling behavior. The REMEG is discussed in greater detail later as part of the experimental method.

As mentioned in the introduction, this dissertation involves modeling work in addition to behavioral experiments. Models provide additional data and help explain behavioral data in experiments. One such model is Camerer and Ho’s (1999) experience weighted attraction model (i.e., EWA), that was developed to explain behavior in a variety of games based on learning. EWA can fit and predict data for a wide array of games in game theory, including the MEG, better than other models focused on a single type of learning and/or captures fewer behavioral phenomena. Therefore, it is an ideal candidate for this dissertation and is used to predict and fit data for the MEG, as well as serve as a synthetic player in the two experiments. However, EWA needed some modifications to better correspond to the REMEG used here. The EWA and modifications are described in more detail in the following sections.

The EWA Model

Camerer (2003) identified reinforcement and belief learning as the most successful approaches for explaining and fitting experimental data. Reinforcement learning approaches assume that strategies or choices are reinforced by the payoff they earned and may generalize to similar strategies or choices, but are typically used when players lack information about payoffs they could have earned (i.e., forgone payoffs; Camerer, 2003). Belief learning focuses on other players and utilizes their previous
behavior to form and update beliefs about what they might do in the future (Camerer, 2003). These beliefs are then used to select the best response, assuming the beliefs are accurate. Belief learning typically does better than reinforcement in coordination games, while reinforcement does better than belief learning in mixed strategy games. EWA is essentially a combination of reinforcement and belief learning; however, it demonstrates general learning characteristics and can be used to model other types of learning by adjusting its parameters (e.g., direction learning and imitation learning). In addition, the model includes initial or starting attractions towards choices based on prior experience, attractions towards choices that are weighted based on recency and level of experience, and forgone payoffs that could have been earned has a different choice been made.

The EWA model includes elements of reinforcement and belief learning with four parameters that are meant to correspond to psychological phenomena, such as forgetting (i.e., rho \( \rho \)), recency effects (i.e., phi \( \phi \)), counterfactual thinking (i.e., delta \( \delta \)), and individual differences in stimulus discriminability (i.e. lambda \( \lambda \)). The model also has three unique features: 1) forgone payoffs (payoffs that could have been earned), 2) growth rate of attractions with separate decay parameters for past attractions and amount of experience, and 3) initial attractions (starting attraction based on prior experience) with experience weights (strength). These features are captured by the four EWA parameters (more specifically, the first three): 1) experience decay (\( \rho \)) controls the strength or impact of the current level of experience, \( N(t) \), that includes pregame experience, \( N(0) \), and previous round experience, \( N(t - 1) \), and is compared to general forgetting, 2) past attraction decay (\( \phi \)) controls the strength of previous attractions in comparison to the current one only within the current game and is also compared to forgetting, but has more
impact on attractions, 3) the forgone payoff parameter ($\delta$) controls the weight of forgone payoffs (i.e., outcomes) and is considered to represent counterfactual thinking, and 4) the sensitivity parameter ($\lambda$) controls the discriminability of or sensitivity to attractions.

Camerer and Ho (1999) consider forgone payoffs ($\delta$) to be the crucial element in EWA as it can direct behavior towards choices with higher forgone payoffs, serve as an aspiration level or reference for comparison, and capture some biased behavior. This parameter also controls the learning behavior of the model and changing it can shift the model more towards reinforcement (when lower) or belief (when higher) learning. The decay parameters ($\rho$ and $\phi$) capture the discounting rate of prior experience and forgetting. If the discounting rate is high, this would create a strong recency effect, where previous experience and attractions have less influence on current or future attractions. Initial or starting attractions are based on prior experience and might reflect strategies that worked in the past. The interaction between initial attractions and forgone payoffs is important because it specifies whether more weight is given to prior experience or learning about the current situation. The sensitivity or discrimination parameter ($\lambda$) is considered an individual difference in ability to discriminate between choice attractions, resulting in increased noise when low as there is lower ability to discriminate between choice attractions. The EWA model was intended to capture some specific psychological processes and their interaction during games. Before describing the innerworkings of the EWA model equations according to my implementation, I will discuss the model’s behavior and the corresponding psychological processes it is meant to approximate by explaining how the model would “play” the MEG.
When humans play the MEG in an experimental setting, they first receive instructions that includes rules and the payoff matrix. In addition, previous experience with related coordination scenarios may vary. The information provided by the instructions and payoff matrix interacts with their previous experience and influences the attractiveness of the seven possible choices. For the EWA model, I assume a similar process (See Figure 2).

**Figure 2.** Brief high-level summary of the EWA model’s behavior in the MEG.

At the start of the MEG, the EWA model has some preexisting experience and choice attractiveness, which are used to determine the probability weights for all possible choices. These choice probabilities are influenced by the sensitivity parameter ($\lambda$) that makes them easier or harder to discriminate between. When there is higher sensitivity, the higher choice probabilities are more salient and likely to influence choices. Choices are then determined by randomly sampling from the set of possible choices using the
choice probabilities as weights. There was not a clear way to determine first round choices for EWA without considering a minimum, so in my implementation I used human pilot data to set first round choice probabilities. Once a choice is made, a minimum is determined based on all player choices. The minimum determines the payoff earned for that round. In addition, EWA considers the forgone payoffs that could have been earned had a different choice been made and weights then according to the forgone payoff parameter ($\delta$). This could also be interpreted as the degree that counterfactuals are considered. Once these processes are completed, EWA updates attractions based on last round experience decayed by $\rho$ (i.e., forgetting), last round attractions decayed by $\phi$ (i.e., degree of recency effects), the actual payoff with full weight (i.e., payoffs multiplied by 1), and the forgone payoffs weighted by $\delta$ (i.e., payoffs multiplied by $\delta$). The attraction updating procedure is meant to capture learning with contiguity throughout the game, so all previous attractions influence the most current attraction. As mentioned, both decay parameters affect the strength of deviation from past attractions based on new experiences. These updated attractions are then used to determine choice probabilities weighted by $\lambda$ for the next round. Next, the model equations are explained in detail.

EWA was designed to study normal form or matrix games, where players have information about how their behavior and behavior of others affects outcomes. Its notation is based on this game structure. Games have $n$ players and each player is indexed by $i$. In general, strategies are written as $s_i^j$, where $j$ denotes the specific strategy out of a set of possible strategies for player $i$. Each strategy consists of one or more choices for a given player, $m_i$. The functioning of the EWA model can be described in
three equations. Here, the equations are discussed in detail based on the approximate order they calculated; however, this is an oversimplification to explain the model.

**Figure 3.** Breakdown and interactions between the three main equations in the EWA model. The bold boxes represent the output value of each equation and the bold arrows show how these values are fed into the subsequent equation from left to right.

First, the experience equation (first column in Figure 3) represents the amount of experience, $N$, based on observations over time, $N(t)$. It is calculated at any given time based on previous experience, $N(t-1)$, and the accumulated experiences during the game, which are modified based on a depreciation rate parameter ($\rho$), $\rho \cdot N(t-1)$.

In the MEG, $N(t)$ could be considered experience based on rounds played, depreciated over time based on $\rho$. For example, consider a new MEG is started with one round played so that $t = 1$ in $N(t)$. In this case, $N(t-1)$ is equal to the initial or starting experience, $N(0)$, based on previous game experience. The current amount of experience is calculated by multiplying experience decay ($\rho$) by the level of experience from the
previous time period or round (i.e., \(N(t - 1)\) or \(N(0)\) for the first round), then adding one round of experience. Equation 1a below shows the experience equation for the first round, and equation 1b, shows the equation for all subsequent rounds.

\[
(1a) \quad N(t) = \rho \times N(0) + 1 \\
(1b) \quad N(t) = \rho \times N(t - 1) + 1
\]

The choice attraction equation (second column in Figure 3) determines the attraction for each choice. In the MEG, a player, \(i\), has an attraction, \(A_i^j\), for each of the seven choices, \(s_i^j\), at any given time, \(t\). The attraction for a choice at a given point in time is \(A_i^j(t)\). The attraction for each choice is updated after each round in three steps based on the weighted previous choice attraction, \(\phi \times N(t - 1) \times A_i^j(t - 1)\), weighted payoff, \([\delta + (1 - \delta) \times I(s_i^j, s(t))]\times \pi_i(s_i^j, s_{-i}(t))\), and the current amount of experience, \(N(t)\). Equation 2a represents the equation for the first round, and 2b represents the equation for all other rounds after the first. Similar to pregame experience, \(N(0)\), there is also pregame choice attractions, \(A_i^j(0)\), that could hypothetically exist from playing the game in the past.

\[
(2a) \quad A_i^j(t) = \frac{\phi \times N(0) \times A_i^j(0) + \left[\delta + (1 - \delta) \times I(s_i^j, s_i(t))\right] \times \pi_i(s_i^j, s_{-i}(t))}{N(t)}
\]

\[
(2b) \quad A_i^j(t) = \frac{\phi \times N(t - 1) \times A_i^j(t - 1) + \left[\delta + (1 - \delta) \times I(s_i^j, s_i(t))\right] \times \pi_i(s_i^j, s_{-i}(t))}{N(t)}
\]

The previous choice attraction, \(A_i^j(t - 1)\), is weighted by multiplying it with attraction decay, \(\phi\), and previous experience, \(N(t - 1)\). It is important to note that if pregame experience, \(N(0)\), was very large, meaning there is a lot of pregame experience and the decay parameter, \(\phi\), was also high, attractions will not change much with
subsequent experience. This would be like sticking to a previously learned strategy or preferred choice.

Next, the weighted payoff is calculated by first determining the value of the indicator function, \( I(s_i^I, s_i(t)) \). This function is equal to one when updating the choice attraction, \( A_i^I \), for the choice that was actually made for that round, \( s_i(t) \). The variable \( s_i^I \) just refers to the choice being updated. The value of the indicator function then determines the payoff weight, \( \delta + (1 - \delta) \). When the indicator function is equal to one, then this payoff weight is also equal to one. However, if the indicator function is equal to one, then this payoff weight is equal to the value of \( \delta \). Next, the payoff weight is multiplied by the payoff, \( \pi_i(s_i^I, s_{-i}(t)) \), which is determined by the current choice being updated, \( s_i^I \), and the minimum of the current round, \( s_{-i}(t) \). When the payoff weight is one because it corresponds to the payoff that was actually earned, then the payoff gets full weight or is equal to the payoff. In all other cases, the payoff weight is equal to \( \delta \) and the weighted payoff is a forgone payoff (one that could have been earned). This forgone payoff weight is important for distinguishing between payoffs actually earned and forgone payoffs. For instance, if \( \delta \) is zero, then only actually earned payoffs are considered, which makes it more similar to strict reinforcement learning. However, if \( \delta \) is one then EWA treats actually earned and forgone payoffs equally, making high payoffs more salient whether earned or not.

Figure 4 illustrates how attractions change based on different values of \( \delta \) when everything else is held constant. In this example, the actual choice is two, the minimum is one, and the forgone choice of one is considered. As \( \delta \) approaches .8, the attraction for the forgone choice (i.e., one with payoff 70) is nearly equal to the attraction for the actual
choice (i.e., two with the payoff of 60). When $\delta$ is higher than .8, the attraction towards the forgone choice is higher than the actual choice.

Figure 4. Plot showing the attraction value for the choice of 2 (solid line) and attraction values for the forgone choice of 1 (circles) based on different values of the forgone payoff parameter $\delta$.

Lastly, the Luce choice rule (Luce, 1959) equation (third column in Figure 3) determines choice probabilities for the next round based on the current choice attractions (Equation 3). The next round choice is then determined by randomly sampling a choice (i.e., 1-7) with the choice probabilities as weights. The exponent in the equation, $e$, represents Euler’s number, which has a value corresponding roughly to 2.72. The $\lambda$ parameter serves as a player’s sensitivity to attractions, which could be considered its degree of salience or the ability to discriminate between attractions.

$$P_i^j(t + 1) = \frac{e^{\lambda \cdot A_i^j(t)}}{\sum_{k=1}^{m} e^{\lambda \cdot A_k^j(t)}}$$

To determine the choice probability for a specific choice, the attraction towards that choice is multiplied by the sensitivity parameter. Next, a logistic transformation is performed by calculating Euler’s number to the power of the sum of the choice attraction
and the sensitivity parameter. Lastly, the choice attraction is normalized by dividing it by summing all logistically transformed choice probability and sensitivity parameter sums. This ensures that all seven choice probabilities add up to one.

The four EWA parameters influence which choices the model makes or predicts. To find appropriate parameter values, Camerer and Ho (1999) estimated EWA parameters for the median effort game (i.e., median instead of minimum as the order statistic) and other less related games. The forgone payoff parameter ($\delta$) was twice as large (.85) and initial experience weight ($N(0)$) was 20 times smaller compared to other games. These parameter estimations suggested players in the median effort game were very sensitive to forgone payoffs (larger forgone payoff parameter), learned more during gameplay (moved further away from initial attractions), and put less value on prior experience (smaller prior experience weight). The EWA model corresponds to the structure of the MEG, however, it had to be modified to correspond to the REMEG used here. The rationale for these modifications is discussed in the following section.

**Extended EWA**

In the first round of the MEG, EWA calculates choice attractions and with each new experience, choice attractions are updated (one for each possible choice) based on actually earned and forgone payoffs. This means that for a given round and minimum, EWA updates a set of seven choice attractions, one for each of the seven possible choices. One of these choice attractions is based on the actual payoff earned, which receives full weight (i.e., 1), while the other six are based on forgone payoffs weighted by the forgone payoff parameter. This is sufficient when players only have or consider information about the minimum for each round, but would present a problem if players
had or considered additional information. In Camerer and Ho (1998), Leng et al. (2018), and van Huyck et al. (1990) some groups were given the distribution of other player choices. Although there was less dramatic convergence to one, behavior was not statistically different from groups only given the minimum. The REMEG used in the current experiments, gives players choice distributions and counterfactual information (i.e., what could have been earned with a different choice or minimum). Adding counterfactuals could increase the saliency of the other possible choices and forgone payoffs that could result in higher payoffs. In its current form, EWA is not able to utilize additional outcome information and is not well suited for the REMEG. Therefore, it requires modification.

A modified version of EWA was developed (Collins, Hough, O’Neil, & Juvina, 2019) to correspond to the REMEG. Specifically, the choice attraction updating procedure was modified so that each player’s choice is treated as a potential minimum and a set of choice attractions (i.e., an attraction for each possible choice) is calculated for each potential minimum (total of four sets of choice attractions). Since this modified EWA extends the original EWA model to consider each player’s choice rather than just the minimum, it is referred to as the Extended EWA model (i.e., EEWA) for the remainder of this paper. The EEWA updating procedure involves updating four choice attraction sets (i.e., one for each player’s choice treated as the minimum). Since it considers each player’s choice as a minimum and updates all seven attractions four times each round, there are a total of 28 choice attraction updates per round. Each choice attraction set has one payoff that gets full weight and the other six are weighted by the forgone payoff parameter. Due to the structure of the model and this modified updating
procedure, the EEWA model treats the actually earned payoff exactly the same as the fully weighted payoffs based on treating each other player’s choice as a potential minimum. This is roughly equivalent to the original EWA model playing four separate rounds with different minimums. However, this modification makes the model more flexible and sensitive to other player choices, because forgone payoffs are considered for each player’s choice rather than just for the minimum. Therefore, the model can better respond based on the behavior of all players in the group. This EEWA model was used in the first experiment where some insights were learned and some issues were raised regarding its functioning and limitations.

In addition to coordination behavior identified in the game theory literature and methods for modeling it, individual differences are also important. The game structure is the same for everyone, but strategies, perspectives, motivations, and preferences may differ between individuals. However, the relationship between individual differences and coordination behavior in the MEG remains unclear. To better understand these relationships, individual differences were included in the two experiments and are discussed in more detail in the following section.

**Individual Differences in Cognition and Dispositions**

There is evidence that players make initial choices based on starting strategies or preferences (Cooper & van Huyck 2018; Costa-Gomes et al., 2009; van Huyck et al. 1990, 1991), give up on trying to increase coordination efficiency (Brandts et al., 2014, 2015), might be more sensitive to risks (Cachon & Camerer, 1996), and might reciprocate if they feel cheated by other players (Offerman, 2002). There is some evidence that coordination efficiency is negatively correlated to risk and positively correlated to trust.
(Bosworth, 2013; Engelmann & Normann, 2010). Relevant effort allocation preferences, abilities, and dispositions are discussed in the following sections.

**Effort-related Traits**

Hull (1943) proposed the law of minimum effort, where he suggested that organisms would take the path of least resistance when left to their own devices. This notion has been supported (Allport, 1954; McGuire, 1969; Zipf, 1949) and later extended by showing that animals act based on rewards and costs (Charnov, 1976; Stephens & Krebs, 1986), such as metabolic cost (Bautista et al. 2001) or number of responses (Walton et al., 2006). This law has also, not surprisingly, been applied to humans. Although humans often take “path of least resistance”, they tend to strive for decision-making efficiency by considering costs and benefits (Beach & Mitchell, 1978; Payne, Bettman, & Johnson, 1988; Simon, 1955). This is not surprising, considering that mental effort is generally considered to be aversive (Kahneman, 2011; Kurzban, Duckworth, Kable, & Myers, 2013) and does not always lead to better decisions (e.g., Cosmidies & Tooby, 1996; Gigerenzer & Brighton, 2006; Gigerenzer & Gaissmaier, 2011). This literature suggests humans strive for decision-making efficiency; however, some suggest humans differ in how much effort they are willing to exert (Juvina et al., 2018; Kool et al., 2010). In addition to the task and environment, Beach and Mitchell (1978) also pointed out the importance of individual characteristics, such as knowledge base, opinions, cognitive abilities, motivation, and emotion. Effort preferences and motivation are traditionally measured using a variety of self-report-based measures that lack precision. However, Kool et al. (2010) recently created a performance-based measure of effort preference called the demand selection task (DST). Since effort and motivation
appear to influence coordination, the DST is used here to explore the potential effects interaction between effort preferences and coordination behavior.

The DST consists of selecting between two different options presented as disks, where one option involves more frequent task switching and is more cognitive demanding than the other option with less task switching. There are several variants, but only magnitude/parity (Kool et al., 2010) and global/local tasks (Juvina et al., 2018) are discussed. The magnitude/parity task involves determining if a given number is less than or greater than five (magnitude) or if it is even or odd (parity). After selecting an option, a number appears and its color indicates which task to complete. The global/local task involves a large letter that is made up of small letters; for instance, a large “X” may be made up of small “X”s or small “T”s. The color of the letters indicates whether to press a keyboard key corresponding to the big letter or the small letters. For the global/local task, switching between responding with the small or large letter more often is considered more cognitively demanding. In both variants, individuals are presented with two disk options and are encouraged to explore them to determine which one they prefer. The high demand option has a 90% task switch (i.e., magnitude/parity) or stimulus incongruency rate (i.e., global/local), while the low demand option has a rate of 10%. In a range of tasks and demands, Kool et al. (2010) found a higher proportion of participants favored the low demand option and there were correlations between this “demand avoidance” and both cognitive control and motivation-related traits. However, there is a potential confound with these findings; a large portion of participants did not detect the difference between the two options. To correct for this, Juvina et al. (2018) extended Kool et al. (2010) by including demand detection point, or point at which the difference between
options was detected, assuming that detection itself is cognitively demanding. Juvina et al. (2018) suggested that those who did not detect which option was more cognitively demanding might be the most demand avoidant. This revised DST is used to measure and investigate if effort preferences are related to coordination behavior in the MEG, since it includes two different types of tasks and measurements for demand avoidance.

Juvina et al. (2018) and Kool et al. (2010) both examined relationships between the DST and other effort-related traits in an effort to help develop a profile for effort avoidant individuals. Juvina et al. (2018) reported that demand avoidance was negatively correlated with several effort related-trails, including self-control, need for cognition, and attentional control. In the MEG, effort avoidant individuals may prefer to stick to low effort choices to avoid harder arithmetic problems corresponding to higher effort choices. If so, they would likely be the weak link of the group by setting the minimum at lower effort and may not respond to other players signaling behavior. Since there are reported negative correlations with other effort-related traits, it is likely that those with lower scores for these trait measures may exhibit the same type of behavior. For instance, those sensitive to risks might start out using a risk dominant strategy and might be hesitant to choose higher effort due to the risk of receiving a lower payoff if they choose higher than the minimum. Similarly, those who do not trust others are likely to exhibit similar behavior, because they might think that other players are in it for themselves. There is some evidence that players start the game with certain strategies and may deviate from these strategies over time based on other players choices (Huyck et al. (1990, 1991). If this behavior is related to effort-related traits, these traits might have more influence at the beginning of the game, until the behavior of the group overpowers them.
The preference to avoid effort is also related to the preference for immediate rewards (Shenhav et al., 2017; Westbrook et al., 2013). This finding is from the intertemporal choice paradigm where participants are typically asked if they prefer a certain reward now or if they would prefer to wait and receive a larger reward.

**Intertemporal Choice**

Those who prefer immediate rewards are thought to discount future rewards depending on the distance in time, which is often non-linear (Frederick, Loewenstein, & O’Donoghue, 2002). For example, Thaler (1981) asked people how much money they would require after a specified period of time in order to be seen as equal to receiving $15 immediately. The amount of money required increased with the amount of delay. Exactly why this occurs is not completely clear, but it seems to be influenced by several factors, such as uncertainty, expectations of utility changes, changes in reward value, emotion or visceral responses (Loewenstein et al., 2002). Weber et al., (2007) suggested that a main component of prospect theory (Kahneman & Tversky, 1979), loss aversion, might also have a role in intertemporal choice. In this view, people may prefer the immediate reward, because it represents a sure gain and the delayed reward may be less preferred because it represents a gamble due to some uncertainty about the future.

Determining the subjective value of a delayed reward seems to require effort and this could be one reason why delay discounting is related to effort avoidance. Furthermore, the extent that an individual prefers the immediate reward may be related to self-control and there is evidence that it is a better predictor of academic achievement than cognitive ability (Duckworth & Seligman, 2005). In the MEG, this delay discounting could involve downplaying higher future payoffs (i.e., global efficiency) in favor of lower and
more immediate payoffs (i.e., local efficiency). For instance, choosing higher effort than other players in an attempt to influence them to choose higher effort (i.e., signaling) can lead to higher payoffs in the future if coordination is achieved at higher effort. However, there is the risk of earning lower payoffs in the short term by choosing higher than other players. On one hand, a player could signal higher effort and take a short-term loss in payoffs in order to achieve higher payoffs in the future; given that other players match that level of effort. On the other hand, a player could discount future payoffs by perceiving the short-term reduction of payoffs as offsetting the potential benefits of higher future payoffs and instead decide to maximize payoffs in the short term.

In addition to considering one’s own actions, players are likely to consider what other players might do. As previously discussed, players can form beliefs and continue to update them over the course of the game. According to research in metacognition, some players might be better able to form accurate beliefs about others, which could enable them to make more appropriate choices.

**Metacognition and Beliefs**

Individuals differ in their ability to evaluate their own thinking and beliefs, as well as that of others. Metacognitive ability is relevant to belief formation about oneself and others, and also involves the calibration (i.e., correspondence) between internal mental states and reality. The Cognitive Reflection Test (Frederick, 2005) and the Feeling of Rightness (Thompson, Prowse Turner, & Pennycook, 2011) were previously used to tap into metacognitive ability. The Feeling of Rightness appears to trigger some kind of conflict between one’s internal mental state and the environment, while the Cognitive
Reflection Test appears to capture this detection and the corresponding ability or motivation to engage in rethinking to produce a better answer or response to a problem.

In coordination games like the MEG, belief learning is often more effective than reinforcement learning (Camerer, 2003). However, there is some missing literature regarding the underlying cognitive mechanisms that influence how individuals form beliefs about other players and its precision. Literature on metacognition suggests that people differ in their awareness of their own ability and the ability of others, and this awareness affects their beliefs and its congruence with reality. Stanovich (2018) believes detecting inadequacies between a current approach and the environment, and the ability to shift to an alternative, more appropriate approach are critical aspects of effective decision making. This metacognitive ability includes both motivation and ability, leading some to suggest that it is more related to intelligence than traditionally used measures (Frederick, 2005; Toplak, West, & Stanovich, 2011). There is some empirical evidence to support this claim as metacognitive ability correlates with cognitive ability and tasks related to reasoning, analogy, and creativity (Barr, 2015; Toplak et al., 2011).

Sperber et al. (2010) also stresses congruence with between one’s beliefs and reality by suggesting that in communication, we cannot trust the communications of others unless we have some degree of vigilance. Following this reasoning, in the MEG, individuals may have a tradeoff between trust and vigilance. For example, an individual may trust that others will choose higher effort or continue to do so, and have a degree of vigilance as they may have been taken advantage of or experienced someone behaving in their own best interest in the past. Individuals might be more vigilant in the MEG than they would be in another coordination scenario, as the weak link of the group determines...
payoffs. This might lead towards more distrust at the start of the game resulting in risk or
cost being more salient.

As motivations of other player’s cannot be known, there are likely attempts to
infer motivations and these attempts might be inadvertently biased. For instance, players
could be influenced by something similar to the fundamental attribution error (Ross,
1977). If others choose low effort, they could overestimate the impact of an individual’s
personality traits and underestimate situational factors. In the context of the MEG, this
could result in lower levels of trust or increase the saliency of risk. Individuals may
assume that other players are selfish or are playing to win rather than taking a risk and
trying to coordinate at higher effort. This type of thinking might reduce the likelihood of
players choosing higher than the minimum.

These individual differences in traits and dispositions may be more influential
when there is little or no contextual information, and during the first interactions with
new individuals. In the MEG, certain dispositions could play a role in low first round
choices leading to inefficient coordination at the beginning of the game, which sets a
baseline for the remainder of the game. However, repeated interaction with other players
and interventions may nudge players towards more efficient choices over time.

**Interventions for Increasing Coordination Efficiency**

Although coordination failure is common (Cooper et al., 1990, 1994, Camerer,
2003, Riechmann & Weimann, 2008; van Huyck et al., 1990), there appears to be greater
difficulty improving efficiency after coordination has stabilized on an inefficient
equilibrium (Brandts & Cooper, 2006; Brandts et al., 2014, 2015; Chaudhuri et al., 2009;
van Huyck et al., 1991). A goal of this dissertation was to identify and gather evidence
for a parsimonious and low-cost method for encouraging coordination at more efficient equilibria. Thaler et al. (2013) suggest nudging (i.e., providing indirect guidance) is often more effective than direct suggestions, which are often perceived as aversive. The counterfactual manipulation and player signaling behavior can nudge other players and groups towards more efficient outcomes. The first experiment assesses the influence of counterfactuals and the second experiment assesses how the direction of counterfactuals influences coordination efficiency. Both experiments include measures of signaling behavior. Prior to discussing the experiments, some background literature about counterfactuals and behavioral signaling are discussed in the following sections.

**Counterfactuals**

Counterfactual thinking involves considering forgone outcomes (Byrne, 2016; Kahneman & Miller, 1986), which is more likely after failures or shortcomings (Gilovich, 1983; Hur, 2001; Roese & Hur, 1997; Roese & Olsen, 1997; Sanna & Turley, 1996; Sanna & Turley-Ames, 2000) and often involve correcting or improving upon previous behaviors (Markman et al., 1993; Roese, 1997; Roese et al., 1999). Epstude and Roese (2008) suggest that this may be dependent on the realization that there is a problem or goals are not sufficiently met, which is often signaled by negative affect (e.g., Lieberman, Gaunt, Gilbert, & Trope, 2002; Taylor, 1991; Schwarz, 1990; Schwarz & Clore, 1983). Behaviors to improve future outcomes might be achieved through goal-oriented reasoning (Epstude & Roese, 2008; Roese & Epstude, 2017) or by increases in motivation, persistence, and performance (Dyczewski & Markman, 2012; Markman, McMullen, & Elizaga, 2008). Previous research has investigated the direction and focus of counterfactuals.
**Direction.** Upward counterfactuals are more likely to lead to performance improvements compared to downward (Morris & Moore, 2000; Nasco & Marsh, 1999). They involve identifying an alternative action that could have led to a more positive outcome (Rim & Summerville, 2014; Roese, 1997; White & Lehman, 2005). They can increase motivation or effort (Markman & McMullen, 2003; Markman et al., 2008), particularly when improvement is believed to be possible (Dyczewski & Markman, 2012). Typically, upward counterfactuals target things that are easier to change (Kahneman & Miller, 1986), like one’s own behavior, and appear most helpful for repeat events with opportunities for change (Smallman & Summerville, 2018). In contrast, downward counterfactuals can lead to rationalization rather than improvement (Smallman & Summerville, 2018). They tend to focus on how a situation or outcome could have been worse (Rim & Summerville, 2014) and are more likely when situations are less malleable (Dyczewski & Markman, 2012), such as attempting to change another person’s behavior.

In terms of the MEG, upward counterfactuals (e.g., you could have earned a higher payoff had you chosen X) are more motivating than downward (e.g., you did well, you could have earned less), especially when outcomes are malleable. This is the basis of the information intervention used here and could be a potential method for improving equilibrium selection and coordination efficiency. Counterfactuals were used as the information-based intervention because they are general, cost-efficient, and may improve coordination efficiency in the MEG. In the present experiments, specific counterfactuals were provided and were expected to nudge individuals towards the forgone choices when they resulted in better outcomes. However, individuals can freely engage in
counterfactual thinking and generate these and many other counterfactuals or forgone outcomes. Although this intervention is designed to assess the effectiveness of explicitly providing counterfactuals, it also extends to counterfactual thinking. Since counterfactual thinking is general, it could be applied to other coordination scenarios and may help increase coordination efficiency. If so, the generality and cost-efficiency of counterfactual thinking makes it an ideal addition to training for teamwork and coordination.

**Focus.** There also appear to be differences in what individuals focus on, depending on whether one is directly or indirectly involved with a situation (e.g., Girotto et al., 2007). For instance, when participants read about a person in a situation, they tend to focus on things that are within the person’s control, such as that person making different choices (Girotto, Legrenzi, & Rizzo, 1991; Mandel & Lehman, 1996; McCloy & Byrne, 2000; McEloney & Byrne, 2006; Roese & Olson, 1995). However, when participants experience the situation, they tend to focus on things outside of their control, such as how other people could have behaved differently or how the situation could have been different (Ferrante, Girotto, Stragà, & Walsh, 2013; Girotto et al., 2007; Mercier, Rolison, Stragà, Ferrante, Walsh, & Girotto, 2017; Pighin, Byrne, Ferrante, Gonzalez, & Girotto, 2011). These findings suggest that counterfactuals may not always serve to improve future outcomes and can instead be used to understand or rationalize the behavior after it occurs (e.g., Girotto et al., 2007; Markman, Mizoguchi, & McMullen, 2008; Mercier et al., 2017). This research addressing the focus of counterfactuals has called some previous findings into question, such as stronger focus on things that easier to change (e.g., Kahneman & Miller, 1986). If these findings apply to the MEG, it
suggests that players would focus more on counterfactuals related to other player’s choices rather than their own. Interestingly, this is congruent with the game structure of the MEG, as player’s payoffs are determined by the lowest choice in the group. Even if a player chooses higher effort, the lowest performer determines their payoff.

In addition to the information-based intervention, the other goal was to assess how repeated interaction with other players can influence behavior. As previously mentioned, signaling is often discussed as the main method for players to influence others. It involves choosing higher effort than the minimum, or other players in general, to encourage other players to choose higher effort, resulting in higher payoffs (i.e., greater efficiency) for everyone. Literature provides evidence that players can improve coordination efficiency of the group, but often involves significant effort, leadership, or helping behaviors. Here, signaling is explored as a potential method for improving coordination by nudging other players towards choosing higher effort.

**Signaling**

Players can send signals to convey information that is not directly observable or is likely to be overlooked and does not require direct communication. Spence (1978) described signaling as revealing a player’s “type”, which should have a cost that is offset by the benefit if the signal receiver uses the information to act accordingly. To be perceived as credible, a signal should be costly and risky for the sender in terms of effort, time, or currency. Charness et al. (2018) demonstrated that effort can serve a social signaling role and effort commitment appears more valuable than money in effort related tasks. However, they also point out that stated or planned effort may not be predictive of actual effort. Effort preferences may change over time or in response to reciprocal actions.
and may be short lived. If a person receives a gift, they are more likely to reciprocate for a period of time (Lowenstein & Schkade, 1999; Lowenstein, 2005) or if they receive higher payoffs then expected, they might put in more effort for a short period of time until it returns to a baseline (Gneezy & List, 2006). Interestingly, negative events cause a stronger change in reciprocal behavior and tend to persist longer (Offerman, 2002).

Costly signaling is a valuable coordination tool, but there is a tradeoff between its cost and the resulting benefit (Thompson & Kaufman, 2010). Brandts and colleagues (Brandts et al., 2014, 2015) conducted experiments with the corporate turnaround game, where a performance trap is set up to create convergence to the lowest effort level, then the leader is introduced. The leaders brought up the minimum by helping other players. However, if help was taken away too early (i.e., before players could settle on a new equilibrium), it was actually worse than no help at all, perhaps due to negative reciprocity on part of the other players. Leaders stopped helping because it incurred a cost (i.e., payoff reduction) that was not sustainable, especially if it failed to bring up the minimum (i.e., low benefit). These findings are informative for behavior-based signaling in the MEG. Signaling higher effort should be sustained for a period of time, but to remain realistic, it should be sensitive to this cost-benefit tradeoff.

The following two experiments utilized both behavioral and model-generated data. The REMEG, DST, and effort-related traits were used to collect behavioral data and the EEWA model was used to generate data to explain and fit behavioral data and it served as a synthetic player. In the first experiment, a new method to improve coordination efficiency was introduced (i.e., counterfactuals) and newly introduced methods were used to measure and analyze coordination behavior. Coordination was
more efficient compared to previous experiments and there appeared to be little influence of demand avoidance or effort-related traits beyond the first round of the REMEG in four-human groups and one-human three-bot groups. In the second experiment with only one-human three-bot groups, four counterfactual conditions were included (i.e., upward, bidirectional, downward, and no counterfactuals as a control) to better understand how counterfactual direction influences coordination efficiency. The upward condition had the highest efficiency, the downward had the lowest, and there were several differences between human and bot behavior. After the two experiments, a new model was developed for the REMEG within the ACT-R cognitive architecture. The cognitive model included cognitive processes and strategies described in the literature that were missing from the EWA and EEWA models, and model comparisons revealed it was better able to fit and explain the human data.
II. Experiment 1

The goals of the first experiment were to: 1) measure coordination behavior in the MEG, and assess its relationship with effort preferences measured by the DST and individual dispositions, and 2) assess the effectiveness of an information intervention in the form of counterfactuals. A within subjects’ design was used by measuring both individual effort (i.e., the DST), and both stated and actual effort (i.e., arithmetic problem performance) related to coordination (i.e., REMEG) for each participant. The effortful task in the REMEG (i.e., arithmetic problems) was not designed to be very difficult; it was designed to be effortful. It involves mental addition (i.e., no paper or calculator) with single digit numbers ranging from two to eight digits, depending on effort level. Instead of using errors as a measure of effort, errors were deducted from individual players payoff and did not affect the group. In addition to actual effort, the REMEG has some other modifications. In typical MEGs, players receive pregame instructions that cover the structure of the game and payoffs, are not allowed to communicate, and are given outcome information about the minimum for each round. Players usually converge to or towards the least efficient or risk dominant option over time, even when they are given the distribution of other player choices (Camerer & Ho, 1998; Leng et al., 2018; van Huyck et al. 1990). In the REMEG, players are given access to a payoff matrix at all times, the anonymous distribution of player choices, and bidirectional (i.e., upward and downward) counterfactuals after each round. These modifications helped participants
understood how to play, could see the variation in player choices, and could potentially use choices as signals to bring up the minimum.

An additional individual condition was used where one participant plays the REMEG with three EEWA model-driven bots. This condition was included to evaluate how similar the EEWA model behavior is to humans, how humans and bots coordinate, whether the EEWA model is sufficient to simulate human behavior for data collection purposes. The EEWA model is sensitive to the distribution of payoffs per round and is more conducive to coordination compared the average human. The initial experience, $N(0)$, for each bot was set to 0, and the first-round choice was generated based on choice distributions and variance of the human data in the REMEG. The model does not have prior knowledge, strategies, or predispositions to behave a certain way beyond stochastic variation in the first-round choice. The model essentially drifts towards the highest payoff based on responses of other players. For simplicity, this condition is referred to as the bot REMEG and the groups with all human players are referred to the human REMEG.

Effort-related traits were included to compare with previous findings with the DST (Juvina et al., 2018) and explore potential relationships with coordination behavior. In addition, risk and trust were added based on Devetag and Ortmann’s (2007) literature review and findings from Bosworth (2013) and Engelmann & Normann (2010) suggesting that trust encourages more efficient coordination.

**Hypotheses**

H1: Players will coordinate towards more efficient equilibria compared to previous MEG experiments due to the counterfactuals presented at the end of each round and its potential influence on signaling behaviors.
H2: There are no expected differences in performance on the REMEG or DST dependent on which task was completed first (task-order conditions).

H3: There is an expected relationship between effort allocation and coordination efficiency, particularly between effort avoidance (DST) and effort selection (REMEG).

H4: Relationships are expected between effort selections in the REMEG (i.e., efficiency) and effort-related traits. Based on previous research, positive relationships with effort are expected between effort-related traits (Juvina et al., 2018; Kool et al., 2013) and trust (Bosworth, 2013; Engelmann & Normann, 2010), while a negative relationship is expected between effort and risk avoidance (Devetag & Ortmann, 2007). For example, those with higher need for cognition are expected to have higher effort selections (corresponding to efficiency). In addition, those with higher trust propensity are expected to choose higher levels of effort and those who are risk avoidant are expected to choose lower levels of effort.

H5: Coordination is expected to be more efficient in the bot REMEG. The bots are responsive to effort choices above the minimum, are likely to increase effort in response to any signaling behavior, and are less likely to be weak link players.

Method

Participants

Participants were recruited at Wright State University in Dayton, Ohio. Data was collected from a total of 104 participants (approximately 60% female) with a mean age of 23 ($SD = 6.8$). Participants completed either the human ($n = 76$) or bot REMEG ($n = 28$). All participants were compensated with a base pay of $10 and the opportunity to earn an additional $10 contingent on performance. Performance was based on the cumulative
payoff earned during the REMEG with a deduction for each incorrect arithmetic problem. Participants were informed about this performance pay in the experimental instructions.

**Materials**

The experiment was completed on a computer and consisted of the DST, the REMEG, and effort-related trait questionnaires. The DST was programmed and runs in JAVA, while the REMEG and surveys were programmed and run using the O-tree platform (Chen, Schonger, & Wickens, 2016). The computers were organized in a row of four cubicles with barriers between each computer booth.

**Design**

This experiment has a 2 (condition) x 2 (task-order) x 20 (round) mixed design. Condition was a between subjects’ independent variable, and task-order and round were within subjects’ independent variables. Effort selections were the main dependent variable for the REMEG, but exploratory analyses included payoff, and two metrics derived from effort: distance from the minimum and intergroup variance. There were two dependent measures for the DST: effort avoidance and demand detection. For the effort-related traits, the scale scores were the dependent measures.

Participants were pseudo-randomly pre-assigned to the human or bot REMEG condition. Since four participants were required to run the human REMEG condition, assignment to the REMEG condition was contingent on how many participants were available to participate for a given session. Therefore, participants were assigned to the human REMEG when there were four participants available, otherwise, they were assigned to the bot REMEG. There was considerable effort allocated towards recruiting four participants per session, but this was unfortunately dependent on participant
availability. Participants were randomly assigned to a task order condition (i.e., REMEG-DST or DST-REMEG) regardless of REMEG condition (i.e., human or bot).

**Procedure**

Before starting the experiment, participants read and signed a consent form, and received instructions. Participants were instructed not to communicate and were told the DST was an independent task, but the REMEG was a group task that includes all participants. Since there were not four players present in the bot REMEG, participants were told three online players would be playing with them. All participants completed the DST, REMEG, and questionnaires, which took approximately 90 minutes. The order of the DST and REMEG was counterbalanced to identify and avoid any potential confounding task order effects. For instance, the subjective cost of effort could increase over time as suggested by Westbrook and Braver (2014), which could be a confound for whichever task is completed second.

**DST.** The DST involves two different variants (Figure 5): the DST-S (task switching) and the DST-GL (global/local). In the DST-S, participants are presented and freely choose among two options (i.e., disks with a unique color and design). Once an option was selected a colored number appeared. If the number was yellow, participants were instructed to respond with a left mouse click if the number was less than five, or a right mouse click if greater than five (magnitude task). If the number was green, participants responded with a left click if the number was odd and a right click if the number was even (parity). In the DST-GL, Participants are also presented with two disks. Instead of colored numbers, a large colored letter appeared that was made up of smaller letters. For example, a large letter “X” could be made up of smaller “X”s or another
letter. During this task, participants were instructed to report the big letter if it was presented in yellow, or report the small letter if presented in green.

![Stimuli for the DST-S parity task (left), DST-GL congruent trial (middle), and DST-GL incongruent trial (right).](image)

*Figure 5.* Stimuli for the DST-S parity task (left), DST-GL congruent trial (middle), and DST-GL incongruent trial (right).

In both DST variants, there is a low and high demand option. The low demand option in the DST-S switches between tasks less often (10% compared to 90%), while the low demand option in the DST-GL has higher global/local (i.e., large/small letters) congruence (90% compared to 10%). To ensure participants understood each variant, they were required to pass a tutorial with a 75% accuracy rate in order to move on to the experimental task. Each variant lasted a total of 10 minutes. Following completion of each variant, debrief questions were asked and served to indicate whether participants detected a difference in demand between the options.

**REMEG.** In the REMEG (O-tree platform; Chen et al., 2016), participants are given a food catering coordination scenario where each individual has a separate task to complete, which is part of the overall group goal of completing the job. Each person can freely choose how productive to be (i.e., effort allocation), but no one can leave until the job is complete. In the REMEG task, each round begins by choosing how much effort to allocate towards the group task. This serves as the dependent measure of effort in the
REMEG and was the dependent measure in previous MEG experiments (Bortolotti et al., 2016; Leng et al., 2016; van Huyck et al. 1990, 1991). Participants will then be required to solve an arithmetic problem that corresponds in difficulty (e.g., number of digits to add together) to the level of effort chosen. To ensure participants are actually attempting to solve these problems, payoff points were deducted from individuals for each incorrect answer. After completing the arithmetic problem, they are shown the responses of all group members, the minimum effort of the group, and their own individual payoff. After these results are shown, participants were shown bidirectional (i.e., upward and downward) counterfactuals. As previously mentioned, there was a human REMEG condition where participants played the game with other people and a bot REMEG condition where each participant played with bots. The procedure was exactly the same for both REMEG conditions, except players in the bot REMEG were told the game was played with three online players.

**Effort-related Questionnaires.** Participants filled out effort related trait scales, which included: Need For Cognition (Cacioppo & Petty, 1982), Tolerance Of Mental Effort (Dornic, Ekehammar, & Laaksonen, 1991), Preference For And Tolerance Of The Intensity Of Exercise (Ekkekakis, Hall, & Petruzzello, 2005), Industriousness (Jackson et al., 2010), the Brief Self-Control Scale (Tangney, Baumeister, & Boone, 2004). In addition, risk avoidance (Holt & Laury, 2002) and Trait Trust (Collins, Juvina, & Gluck, 2016) were also measured.

**Results**

Previous research has not identified appropriate methods to analyze coordination behavior, therefore, analyses were performed at the round (i.e., average), group, and
individual levels to thoroughly test the hypotheses and explore new methods of data
analysis. Effort was the main dependent measure, but exploratory analyses were
performed for payoff, and two measures derived from effort: distance from the minimum
and intergroup variance. In addition, a weak link analysis was performed to compare to
Leng et al. (2018). These exploratory analyses served to establish additional methods to
analyze coordination behaviors beyond just average effort and minimum.

The results were separated into three main sections. First, human and bot REMEG
were analyzed separately. The round analysis with effort for the human condition served
as the main analysis to test H1 (more efficient coordination compared to previous studies)
and the round analysis for the bot condition was used as the main analysis to test H5 (bot
condition has greater coordination efficiency than the human condition). Potential task
order effects were explored (REMEG-DST and DST-REMEG) to test H2 (No task-order
effects). The individual analyses were used to assess the relationship between effort
choices and demand avoidance to test H3, and effort-related traits to test H4. Second, all
the data were pooled together and the same analyses were performed. Lastly, the EEWA
model was used to fit data from the human REMEG (REMEG-DST) and was compared
to the original myopic EWA. All data analyses were completed using MatLab version
R2018a, while the EWA model fitting was completed in R studio version 3.4.4.

**Human REMEG**

In the human REMEG, one group was thrown out because one participant
previously had completed the experiment, resulting in 72 participants (18 groups). There
were 32 participants (8 groups) for REMEG-DST and 40 participants (10 groups) for
DST-REMEG.
**Round Level.** To test H1, participant’s effort was averaged across 20 rounds and compared to van Huyck et al. (1990) and Leng et al. (2018). There were seven groups of 16 in van Huyck et al. (1990) and participants were only given the minimum each round. There was a significant difference in effort, $t(28) = 5.65$, $p < .0001$, $d = 2.78$, between the human REMEG ($M = 4.34$, $SD = .29$) and van Huyck et al. (1990) ($M = 2.78$, $SD = 1.18$). In Leng et al. (2018), 10 groups of six participants ($N = 60$) were given complete and continuous outcome information (i.e., effort, minimum effort, and cumulative payoffs).

![Figure 6.](image)

**Figure 6.** Line graphs comparisons showing average effort between the human REMEG, Leng et al. (2018), and van Huyck et al. (1990) (left) and average effort and variance between human REMEG order conditions across all 20 rounds (right). Error bars are 95% confidence intervals.

Without access to the complete data for Leng et al. (2018), statistical tests were not possible. However, the average first and last round effort were used to approximate a trend line for comparison with van Huyck et al (1990) and the REMEG (left in Figure 6). Coordination in the REMEG was more efficient (higher effort) than in van Huyck et al. (1990) and Leng et al. (2016) supporting H1.
To test H2 and assess any potential task order effects, with coordination behavior, the REMEG data was split by task order (right in Figure 6). There was a significant difference between the two task orders for effort, $t(38) = 7.6, p < .0001, d = 3.19$ and variance, $t(38) = -6.8, p < .0001, d = 3.95$. For REMEG-DST, effort was higher ($M = 4.84, SD = .43$) and variance was lower ($M = 1.75, SD = .67$), compared to DST-REMEG effort ($M = 3.94, SD = .67$) and variance ($M = 3.14, SD = .62$). There appears to be a task-order effect, where those completing the DST first had lower effort and higher variance compared to those completing the REMEG first. This task-order effect disconfirms H2.

**Group Level.** The relationship between coordination and coordination efficiency was compared for each group. Previously, coordination was measured with the group minimum (e.g., Leng et al., 2018) or average effort (van Huyck et al., 1990), however, neither actually measure the extent that players settle on a single choice. For instance, the minimum only includes the lowest choice in the group and remains constant even if the majority of the group was choosing higher (i.e., convergence above minimum). Furthermore, if half of the group chose 6 and the other half chose 2, the average would be 4 and does not reflect the degree of convergence, which is not high in this case. Here, intergroup variance was used to measure coordination as it better captures the degree of convergence on a single choice within the group. To compare the relationship between coordination and coordination efficiency for groups, average effort and variance were calculated for each group per round and then averaged over all 20 rounds. There was a non-significant negative trend between average effort and variance, $r(16) = -0.38, p = .12)$. This negative trend suggests that when players coordinate (i.e., variance is low), it is likely at more efficient equilibria (i.e., higher effort). On the other hand, when variance is
high, effort is lower. This could be the result of few weak link players choosing lower effort than other players or signaling players choosing higher effort in an attempt to increase efficiency. Next, data were separated by task order. There was a non-significant negative relationship between average group effort and variance for REMEG-DST, $r(6) = -0.69, p = .057$. Interestingly, there was a complete lack of any relationship between average group effort and variance for DST-REMEG. There was, however, a significant difference between order conditions for variance ($t(16) = -2.1, p = .05, d = .53$), where variance was lower for REMEG-DST ($M = 1.75, SD = 1.05$), compared to DST-REMEG variance ($M = 3.14, SD = 1.61$). These findings suggest that groups coordinated better, but not necessarily more efficiently, when REMEG was first.

**Individual Level.** Relationships between coordination behavior in the REMEG with effort avoidance (DST) and effort-related traits were explored. Average player effort and first round effort served as the measures of coordination behavior in the REMEG. First round effort was not an explicit part of H3 or H4 and is considered exploratory. There were no significant relationships between group REMEG and either DST measure or personality traits, indicating a lack of support for H3 and H4. Next, data were split by task order. For REMEG-DST, there was a significant relationship between first round effort and demand avoidance ($r(30) = -.45, p = .01$). However, the relationship with demand avoidance only held for the first round and was not significant for average effort across 20 rounds. There were no significant or notable relationships found in DST-REMEG. They were included as previous literature suggested players start the game with preferences and deviate from them over time based on experience (e.g., Camerer & Ho, 1998; Huyck et al., 1990, 1991). The relationship between demand avoidance and first
round effort for REMEG-DST, and the task-order effect suggests previous mental effort during the DST likely reduced effort allocation and reduced coordination efficiency.

Next, a novel measure called the distance from the minimum was used for exploratory analyses. It was calculated for each participant by taking the difference between effort and the minimum for each round and then averaging across rounds. This measure of coordination provided a more appropriate comparison, since each player was nested within a four-person group with its own dynamics and it relates to signaling behavior (i.e., choosing higher than the minimum). The mean difference from the minimum was positively related to first round effort \( (r(70) = .25, p = .036) \), indicating that those who chose higher effort in the first round tended to do so for the remainder of the game. Data was split by task order and explored. Mean distance from the minimum was lower for REMEG-DST \( (M = .98, SD = .55) \), compared to DST-REMEG \( (M = 1.39, SD = 1.09) \), and this difference was significant, \( t(96) = -2.34, p = .02, d = .47 \). In addition, there was an unexpected finding with task order: relationships with mean distance from the minimum were in opposite directions (Figure 7). REMEG-DST average distance from the minimum had non-significant negative relationships with both first round effort and average effort, and had a significant negative relationship with average group effort \( (r(30) = -.56, p < .001) \). DST-REMEG average distance from the minimum is positively and significantly related to both first round \( (r(38) = .51, p = .001) \) and average effort \( (r(38) = .76, p < .0001) \), but not average group effort. This finding is interesting because players in DST-REMEG are not coordinating well and appear to be choosing higher than the minimum, perhaps due to some players signaling in an effort to nudge other players to choose higher effort.
An exploratory weak link player analysis was performed to compare to Bortolotti et al. (2016). All players were ranked according to their mean distance from the minimum (i.e., players with the lowest ranking). Although this pattern was consistent, there were a few rounds where the minimum was not set by the weak link (left in Figure 8). To justify splitting data by ranks, a one-way ANOVA was performed. The ANOVA was significant, $F(3,76) = 58.91$, $p < .0001$, $d = .42$ and all groups were statistically different from each other. This result also suggests differences in signaling behavior, as stronger links are more likely to be signaling and weak links are not.

Average effort for weak links was higher for REMEG-DST ($M = 4.43$, $SD = 0.65$) than DST-REMEG ($M = 2.95$, $SD = 0.46$) and this difference was significantly different ($t(38) = 8.33$, $p < .0001$, $d = 5.28$). However, there were no differences between task orders for any other rank group. Next, the 18 weak links were separated for analysis. First round effort had a significant positive relationship with average effort ($r(16) = .48$, $p = .04$). The relationship between first round effort and average effort was not significant.
for REMEG-DST, but was for DST-REMEG ($r(8) = .73, p = .016$). Weak link effort declined slightly over time and weak links appeared to be influenced by task order.

Figure 8. Line plots showing average effort per mean distance from the minimum rank groups (left) and between task orders for the weakest links (right) over all 20 rounds for the human REMEG. Error bars are 95% confidence intervals.

**Bot REMEG**

There were 28 groups (one for each participant) in the bot REMEG. There were 14 participants for REMEG-DST and 13 participants for DST-REMEG. The data were analyzed using the same methodology as the human REMEG.

**Round Level.** Participant’s effort and variance were averaged for each of the 20 rounds and were compared to the human REMEG (left in Figure 9) to test H5. There was a significant difference for variance ($t(38) = -10.9, p < .0001, d = 7.85$), but not for effort. The bot REMEG had lower variance ($M = .94, SD = .53$) compared to variance in the human REMEG ($M = 2.52, SD = .36$). These results did not support H5. Players coordinated (lower variance) significantly better in the bot MEG, but according to the t-test, efficiency (average effort) was not significantly better.
Figure 9. Line graphs showing average effort and variance comparisons between human and bot REMEGs (left) and between bot REMEG task orders across all 20 rounds (right). Error bars are 95% confidence intervals.

Next, data was split by task order, and average effort and variance were compared across all 20 rounds (right in Figure 9) to further test H2. The difference between effort was significant ($t(38) = 5.9, p < .0001, d = 3.85$), but the difference between variance was not. For REMEG-DST, average effort was higher ($M = 4.62, SD = .24$) compared to DST-REMEG effort ($M = 4.27, SD = .12$). Similar to the human REMEG, coordination appeared to be at less efficient equilibria when the DST was completed prior to the REMEG. However, in the bot REMEG, there was no difference in variance.

**Group Level.** There was a non-significant negative relationship between average group effort and variance, however, variance is very low in the bot REMEG regardless of average effort. Next, data were separated by task order, which reduced the sample size to 15 groups for REMEG-DST and 13 for DST-REMEG. There were non-significant, negative relationships between average group effort and variance for REMEG-DST, $r(13) = -.40, p = .15$, and DST-REMEG, $r(11) = .16, p = .61$. However, the relationship is much weaker for DST-REMEG.
**Individual Level.** The relationship with demand avoidance in the DST tested H3 and the relationship with effort-related traits tested H4. This analysis included only 26 participants, because two participants did not complete the DST and were excluded. There were a few notable relationships between the bot REMEG and the DST, but not with personality traits. Demand avoidance had a negative non-significant relationship with average effort and group variance. However, demand avoidance did have significant negative relationships (Figure 10) with both average payoff ($r(24) = -.41, p = .04$) and group effort ($r(24) = -.39, p = .05$). These findings did not support H3, but provided weak evidence for a relationship between demand avoidance and coordination behavior.

![Figure 10. Scatterplot showing the relationship between average group effort and demand avoidance for bot REMEG.](image)

Although there were significant correlations between demand avoidance, average payoff, and average group effort, there were no significant correlations for task-order conditions. However, relationships were trending in the same direction and magnitude.

**Pooled Data**

All data was pooled together to explore the round, group, and individual levels using the same procedure as above. This was appropriate because the design was the
same for both human and bot REMEG. Pooling the data also reduces the noise or random variation and gives a better idea of the overall behavioral trends in the REMEG.

**Round.** Significant differences were found between task orders for both effort ($t(38) = 9.56, p < .0001, d = 6.28$) and variance ($t(38) = -6.4, p < .0001, d = 4.7$) (Figure 11). For REMEG-DST task order, effort was higher ($M = 4.7, SD = .24$) and variance was lower ($M = 1.14, SD = .47$), compared to DST-REMEG effort ($M = 4.13, SD = .12$) and variance ($M = 1.98, SD = .35$). This provided more evidence for task-order effects.

![Figure 11](image1.png)

**Figure 11.** Line graphs showing average effort and variance across all 20 rounds for the pooled data (left) and pooled data split by task order (right). Error bars are 95% confidence intervals.

**Group.** There was a significant negative relationship between average group effort and variance ($r(41) = -.31, p = .001$), which indicates that when players coordinated (i.e., lower variance) it was at higher effort (left in Figure 12). The higher variance when effort was low could be a result of weak link players or signaling players attempting to bring up the effort selections of the group. This finding relates to H1.

Next, data were split by order condition and the same procedure was used to compare effort and variance (right in Figure 12) for task orders. Interestingly, there was a
significant negative relationship between average group effort and variance for REMEG-DST ($r(20) = -0.44, p = .002$), but not for DST-REMEG ($r(20) = -0.07, p = .61$).

![Figure 12](image.png)

**Figure 12.** Scatterplots showing the relationship with average group effort and variance for the pooled REMEG data (left) and the pooled REMEG data for REMEG-DST (right).

**Individual.** The pooled data were analyzed at the individual level to explore relationships between the REMEG, DST, and personality traits. Analyses of personality traits revealed two significant negative relationships. Self-control was negatively correlated with average effort ($r(95) = -0.21, p = .04$) and need for cognition was negatively correlated with first round effort ($r(95) = -0.21, p = .04$). These relationships are in the opposite direction as expected, based on previous research (e.g., Kool et al., 2013; Juvina et al., 2018). This provides weak support for H4. There were no significant relationships for task orders separately.

Demand avoidance had non-significant, negative relationships with first round effort and average effort. The negative relationship with demand avoidance and average effort was not significant for either order condition.
However, the relationship between first round effort and demand avoidance was more interesting (Figure 13). For REMEG-DST (left), there was a significant negative relationship ($r(44) = -.31, p = .04$), but for DST-REMEG (right) this relationship disappeared ($r(49) = .18, p = .21$).

Since several significant differences were found between REMEG and task orders, demand avoidance data were explored for these potential differences as well. There was no difference in demand avoidance between the human REMEG and bot REMEG. However, there was a significant difference between task orders for the pooled
data, \((t(95) = -2.69, p = .008, d = 2.18)\). REMEG-DST had lower demand avoidance \((M = .59, SD = .42)\) compared to DST-REMEG \((M = .79, SD = .34)\) (see Figure 14). This is interesting and contrasts with the task-order effect that suggested increased subjective cost of effort. Perhaps there is a transfer effect from the first to second task.

**EEWA Model Fit**

The EEWA model (Collins et al., 2019) used in the bot REMEG was also used to fit the human REMEG data to provide additional data to explore why players may have coordinated at efficient equilibria. In order to fit the data, the four free parameters for the EEWA were estimated using a hill-descending algorithm that iteratively found good-fitting regions within the 4-dimensional parameter space (github.com/koneill1994/MEG_EWA_model/blob/master/HillClimber.R). Means and standard deviations were estimated for the forgone payoff weight, \(\delta (M = .2, SD = .0)\), depreciation rate of experience, \(\rho (M = .9, SD = .001)\), decay rate for previous attractions, \(\Phi (M = .21, SD = .17)\), and sensitivity to attractions, \(\lambda (M = .49, SD = .09)\). The mean parameter estimations correspond with previous literature; however, the forgone payoff weight is significantly lower than Camerer and Ho’s (1998) estimation for the median effort game. Based on the nature of the EEWA model, this is not surprising. Out of the 28 attraction updates per round, four attractions correspond to the actual choice and involve payoffs that are given full weight. This means that the actual payoff and three forgone payoffs receive full weights and are all treated like actual payoffs. Therefore, the forgone payoff parameter in the EEWA has a less dramatic effect on attractions values compared to the original EWA.
To produce model fits, EEWA models were used to simulate 100 four-person groups during the REMEG. The average effort and the average minimum for both EWA (Camerer & Ho, 1999) and EEWA model simulations are shown in Figure 15 along the human data for all 20 rounds. As expected, the EEWA fit the human REMEG data better for average effort \((r = .55, RMSE = .31)\) and minimum \((r = .5, RMSE = .48)\), compared to EWA fits for average effort \((r = .52, RMSE = .96)\) and minimum \((r = .47, RMSE = 1.02)\). However, the EEWA model was not penalized for extra complexity (i.e., considering each player’s choice as a potential minimum). Similar to the human data, the EEWA model had relatively stable average effort, compared to the gradual decline in effort for the EWA across the 20 rounds.

*Figure 15.* Line graphs comparing EWA model fits to the average effort (left) and minimum (right) for the human REMEG. Error bars are 95% confidence intervals.

Next, average group effort and variance for the EEWA model simulations were plotted and compared to the human REMEG data (Figure 16). There was a negative relationship between average group effort and variance for the human REMEG \((r(16) = -.38, p = .12)\). There was also a negative relationship between average group effort and variance for model simulation groups \((r(98) = -.25, p = .01)\).
Figure 16. Scatterplot showing the relationship between average group effort and variance for the human groups and EEWA model groups in the REMEG.

Based on the findings from the human data, the EWA (e.g., only the minimum for each round is used to update attractions), and EEWA (e.g., each player’s choice is used as a potential minimum and attractions are updated four times per round), players may have coordinated at more efficient equilibria by utilizing counterfactual information and engaging in costly signaling.

Discussion

Overall, there was support for H1, however, the reason(s) were unclear. Players coordinated on more efficient equilibria or at higher effort compared to van Huyck et al. (1990) and on average effort remained rather flat across 20 rounds. There was a negative relationship between effort and variance per group in the human, bot, and pooled REMEG data. There are three potential reasons for this increased efficiency: providing a complete player choice distribution, addition of counterfactuals, and small group size. Some previous work demonstrated that introducing player choice distributions has little effect on coordination (Camerer & Ho, 1998; van Huyck et al., 1990). In addition, Leng et al. (2018) found higher effort than van Huyck et al. (1990) with six person groups,
however, even with complete information players still showed a general trend towards lower effort over 10 rounds of play. Based on previous research where additional information did not improve efficiency (Camerer & Ho, 1998; Leng et al., 2018; van Huyck et al. 1990) and findings here, it is assumed counterfactuals were responsible for the higher coordination efficiency. In addition, there was some evidence that players were signaling based on the distance from the minimum analyses and relationship between group effort and variance.

The presence of the task-order effect disconfirmed H2 and suggested that completing the DST first had an effect on coordination and coordination efficiency in the REMEG. Specifically, effort selections in REMEG were lower (i.e., lower efficiency) and variance was higher (i.e., lower degree of coordination) for those completing the DST first (DST-REMEG). This finding relates to H3 and is discussed further.

Support for H3 was less straightforward. There was some direct support for H3 in the individual level analyses for the bot and pooled REMEG data, and indirect support from the task-order effect. The task-order effect where those completing the DST first performed worse in the REMEG corresponds with previous research (e.g., Kool & Botvinick, 2014) by suggesting the DST may increase the subjective cost of effort and promote effort avoidance. For instance, higher subjective cost of effort may have reduced effort allocated towards math problems or reasoning about strategies during the REMEG. Players may have paid less attention to player choice distributions and counterfactuals, preferred easier math problems, or wanted to make simple choices without much thought. Completing the DST first appeared to influence effort avoidance-like behavior or the subjective cost of effort in the REMEG, which reduced coordination and coordination
efficiency. This conflicts with Rand and colleges’ (Rand et al., 2014; Rand, Greene, & Nowak, 2012; Rand, Newman, & Wurzbacher, 2015;) idea that cooperation is less effortful than self-interest and is the default strategy. For example, in the MEG, players could cooperate by decreasing the discrepancy between their choice and choices of other players. Interestingly, there was a task order effect found for the DST in the opposite direction. For instance, effort selections were lower in the REMEG when the DST was completed first, but demand avoidance was lower when the REMEG was completed first. This suggests a possible transfer effect between tasks dependent on the task completed first. Interestingly, the task-order effect appeared to be stronger for weak links, suggesting an increased potential for weak links to bring down the performance of the group after a cognitively demanding task.

For H4, there was very little support. There were only three significant relationships exclusively in the pooled data. BSC was negatively related to both average effort and average group effort, while NFC was negatively related to first round effort. These results suggest self-control might help in wasting less effort during the game and those high in NFC might waste less effort at the very start of the game. These findings conflict with previous experiments with effort-related personality traits and demand avoidance, which suggest these relationships should be positive (e.g., Kool et al., 2013; Juvina et al., 2018). In addition, Devetag and Ortmann (2007) suggested risk and trust are related to coordination, and Bosworth (2013) and Engelmann & Normann (2010) found evidence that trust may encourage efficient coordination, while and risk may decrease efficiency. However, the current study failed to support these findings as risk avoidance and trust were not related to effort selections.
There was no evidence to support H5, as players in the bot REMEG did not coordinate on more efficient equilibria than the human REMEG. It is possible that the higher variance in the human REMEG was positive as it was higher at lower effort, which may have helped prevent coordination at lower levels of effort. Higher variance could result from one or two players signaling higher effort, or one weak link choosing lower effort than the other players. That being said, the average effort in the human REMEG is impressive given the increased probability for weak link players.

The EEWA model fit to the human data and comparison with the EWA model provides further support for the additional outcome information, particularly the counterfactuals, driving the increased efficiency observed here.
III. Experiment 2

In typical MEG experiments, players converge towards the most inefficient or lowest payoff option (e.g., van Huyck et al., 1990) and once this occurs it is difficult to improve in subsequent rounds (Brandts & Cooper, 2006; Brandts et al., 2014, 2015; Chaudhuri et al., 2009). However, in the first experiment, coordination was more efficient and stable across 20 rounds compared to previously reported experiments (e.g., Camerer & Ho, 1999; Leng et al., 2018; van Huyck et al., 1990). In addition, the analysis of average effort and variance per group revealed a significant negative relationship in the pooled REMEG data. These results suggested that when players coordinated, it was more likely at higher effort. Any increase in variance could be a result of a weak link player(s) avoiding higher effort or a potential “strong link” player(s) choosing higher effort in an attempt to signal others. These results were promising regarding increasing efficiency, but there were some unanswered questions.

It is not clear why efficiency was higher and remained stable over the course of the REMEG. Behavior could have been influenced by the real effort task, however, this experiment and a previous study (Bortolotti, et al. 2016) addressed the potential influence of effort. Although Bortolotti et al. (2016) found coordination at higher levels compared to previous experiments, they used smaller groups, had access to more information, and had several opportunities to size up other players (e.g., costly signaling). There is inconclusive evidence that their effortful task encouraged higher effort selections that increased efficiency. In addition, the first experiment suggests that exerting mental effort
prior to coordination (e.g., completing the DST prior to the MEG) has a negative effect on efficiency. Another possible explanation is that additional outcome information (i.e., distribution of all choices and counterfactuals) improved efficiency. However, previous studies found player choice distributions (e.g., Camerer & Ho, 1999; van Huyck et al. 1990) and even cumulative information during the game (e.g., Leng et al. 2018) had little effect on efficiency. Previous research, results of the first experiment, and the EEWA model fits suggest that the more efficient coordination observed here was related to counterfactuals. However, there is currently no direct evidence to support this conclusion and group size effects cannot be completely ruled out. In addition, there was some evidence that players were signaling and this was related to both coordination and coordination efficiency. A follow up experiment was necessary to investigate the extent that counterfactuals improved efficiency and further assess the role of signaling behavior. The second experiment extended the counterfactuals from the first experiment by assessing the effectiveness of four counterfactual conditions: upward (i.e., “you could have earned a higher payoff if you had chosen X or if the minimum was X”), downward (i.e., “you could have earned a lower payoff if…”), bidirectional (included both upward and downward counterfactuals), and no counterfactuals (control condition). The first experiment only used bidirectional counterfactuals, however, as mentioned, previous literature suggests there are distinct differences between upward and downward counterfactuals. Upward counterfactuals are more likely to increase motivation or effort (Markman & McMullen, 2003; Markman et al., 2008) and improve future performance (Morris & Moore, 2000; Nasco & Marsh, 1999). Downward counterfactuals may lead to rationalization rather than improvement of future performance (Smallman &
Summerville, 2018). Based on previous literature, the upward counterfactuals should be more likely to influence individuals to choose higher effort than downward. In the second experiment, differences between the control condition and counterfactual conditions would suggest that counterfactuals can influence coordination. Differences between counterfactual conditions would further suggest that counterfactuals influence coordination in different directions depending on the counterfactuals. Lastly, if participants in the upward counterfactual condition perform better than those in the other conditions, it would support the hypothesis that counterfactuals can increase coordination efficiency. Furthermore, the exploratory analyses in the first experiment provided some evidence that signaling can be measured using distance from the minimum.

The second experiment was designed to run on Amazon Mechanical Turk (Mturk) after the difficulty with running four-person groups in person. However, after two weeks of testing, it became evident that running a large number of four-person groups was also not feasible using Mturk. Therefore, the second experiment was re-designed to run single participants that play with three EEWA bots. The EEWA model needed some updates to address concerns raised after the first experiment. These updates are discussed in detail in the next section.

**Updated EEWA Model**

As previously discussed, the EEWA calculates four “earned” payoffs based on its choice and they are treated equally (i.e., same weight). The earned payoff is based on the real minimum and the three other “earned” payoffs are based on other players choices treated as potential minimums. This issue was addressed. The updated EEWA model gives full weight to the actual payoff (i.e., actual choice and minimum) and the three
forgone payoffs based on potential minimums (other player choices) are weighted by delta (i.e., forgone payoff parameter). In addition, the model was updating choice attractions sequentially in a random order and treated each update like a separate round that was subject to memory decay. This updating procedure was revised so that all sets of attractions are calculated separately then averaged for each choice so that all sets of attractions are treated equally for a given round.

As the second experiment includes additional counterfactual conditions, condition specific EEWA models were developed. For example, in the upward counterfactual condition, participants are given counterfactuals for choices and minimums one and two higher than their actual choice and the group minimum. As the EEWA model considers all possible choices for the minimum, the upward EEWA model only needs to include two additional choice attraction sets for counterfactual minimums. This minor change makes the condition specific EEWA models more closely correspond to the different counterfactual conditions. Two generations of condition specific variants were developed and compared to evaluate which method was most appropriate for serving as a synthetic player. These condition specific variants are labeled with “C” for condition and the letter that corresponds to the specific condition (e.g., EEWA-UC for the upward condition). Since the control condition has no counterfactuals, the EWA model was used since it is more congruent with that condition.

*First Generation EEWA-C*

The first generation of condition specific EEWA variants have two additional attraction sets based on the two counterfactual minimums. For example, the first-generation upward condition model (i.e., EEWA-UC1) generates four sets of attractions
(i.e., one for each player’s choice) where only the real payoff gets full weight (i.e., based on the player’s choice and the real minimum), and two additional sets of choice attractions for one and two higher than the real minimum. A major limitation with these models is the lack of data for validating the effects of counterfactuals. Previous experiments have used minimum effort games, but did not include counterfactual conditions. Therefore, the EEWA-C models are based on assumptions about human behavior in these conditions based on two separate lines of previous work with the minimum effort game and with counterfactuals. These underlying assumptions of the model are important and should be better informed. The goal of this dissertation is to see how the counterfactual conditions and signaling can influence coordination behavior. Ill-informed model-based bots could potentially have a stronger influence on the human player’s behavior than the counterfactuals, which would defeat the purpose of the experiment. The solution to this problem is to use bots that are heavily influenced by the human player’s choices rather than the counterfactuals. If the human player is influenced by the counterfactuals, then this should influence the bots as well. Therefore, before making assumptions about the influence of the counterfactuals, it was important to evaluate how they influenced human players during the REMEG. Since the bot EEWA model previously used in the first study bot REMEG condition was very sensitive to other players choices, it served as the base for the second-generation models. This bot EEWA model is quite different as it was intended to be conducive to coordination rather than be psychologically plausible, and has two potential issues.

First, the bot EEWA generates a set of attractions for all player’s choices and it does not distinguish between the real minimum and potential minimums. However, this
concern was already raised and addressed. Second, delta (i.e., the forgone payoff parameter) was set to 1, which means that all payoffs have full weight. Normally, only the actual payoff earned (i.e., based on the player’s real choice and real minimum for that round) gets full weight and everything else is weighted by delta. If delta is set to 1, then every choice attraction in every set gets full weight. Together, these two issues mean that the bot EEWA considers every possible choice for every possible minimum and treats them all equally regardless of what choice was made and what the minimum was for each round. To assess the bot EEWA and its response to signaling, three simulations were run with four bot EEWA bots (Figure 17). One simulation had no signaling (black), a second had one bots choice fixed at 7 (blue), and a third had one bots choice fixed at 1 with a shift to 7 at round 11 (red). The signaling bot has a strong influence on the mean, so the signaling bot was removed from the figure to show its effects on the other bots.

![Figure 17](image.png)

*Figure 17.* Average effort for three simulation runs (no signaling, signaling at 7, and signaling at 1 and 7) with the three non-signaling bot EEWA bots across all 20 rounds.

As shown in Figure 17, the bot EEWA is much more sensitive to other player choices and is a better candidate to serve as a bot for the first proposed experiment. It does not make strong assumptions about the influence of counterfactuals and allows the
one human player to have a strong influence on the group since the bot EEWA is sensitive to signaling behavior.

**Second Generation EEWA-C**

The second generation EEWA-C models are based on the bot EEWA and have mild influence from the counterfactuals per condition. All EEWA-C models generate four sets of choice attractions based on all player choices, but generate up to two unique choice attraction sets specific to the counterfactual condition. For instance, if all players chose an effort level of 3 in the upward condition, the EEWA-UC model would generate four sets of choice attractions that all consider 3 as the minimum. These are referred to as the potential minimums. Since the real minimum is 3, the EEWA-UC model would also generate two additional choice attraction sets for the minimums of 4 and 5, since upward counterfactuals show forgone payoffs for one and two higher than the minimum. These minimums are referred to as counterfactual minimums and the choice attraction sets for them are weighted less than potential minimums. This feature allows some influence from the counterfactuals, but makes the model more sensitive to player choices than the counterfactuals. Although this version is better suited to investigate how counterfactuals influence human coordination, it still imposes assumptions about the influence from counterfactuals. A solution to this problem is to use three bots that are not influenced by counterfactuals, but are sensitive to other players choices. Therefore, any difference between counterfactual conditions could then be attributed to the human’s behavior and any effects from the counterfactuals. Figure 18 shows the results of simulations using this set up. To test the reactivity to signaling-like behavior, the choices of one bot were manipulated in the same manner as the bot EEWA in Figure 16 above.
Figure 18. Average effort for all conditions with no signalers, one signaler always choosing 7, and one signaler choosing 1 for the first 11 rounds then switching to 7 for the remaining rounds. Note: the signaling bot was removed.

Since the bots are meant to approximate human behavior, they need to have some variability that approximates individual differences. Also, as previously mentioned, there are problems with setting delta or forgone payoff weights to one. To address both concerns, each bot had a unique delta that was sampled from a truncated normal distribution with a mean of .9, standard deviation of .1, and an upper limit of 1. This allowed for the actually earned payoff to receive full weight and all other payoffs are weighted by delta. This unique delta corresponds to individual differences where some bots place more weight on the choice actually made and others may place equal weight
on both actual and forgone choices. Another issue is that humans come into situations with previous experience. To approximate this, the human data for first round choices were pooled and used to generate first round choice probabilities for the bots and pregame experience was set at 20 (i.e., 20 previous instances of coordination). To allow for variability, the bots randomly sample a choice for the first round from a choice distribution weighted by the human first round choice probabilities. In addition, the bots start the game with choice attractions that correspond to these first-round choice probabilities. The settings for pregame attractions and experience mean that bots start the game with choice attractions, and they get larger as the game progresses.

Prior to running the experiment, pilot data was collected for 10 participants. The results of the pilot run prompted one minor change to the EEWA model. Compared to four-human groups, the three EEWA bots and human tended to reach equilibrium faster than four-person human groups and had more resistance getting pulled out of equilibrium by signaling behavior. To increase congruence with four-person groups, a slight change was made to the attraction decay parameter ($\phi$). It was changed from .7 to .6, which resulted in a slight reduction in the speed of equilibrium and slight increase in reactivity to signaling after equilibrium is reached without sacrificing other qualities of the model. For consistency and reduction of unintended effects on the human, the same EEWA bots were used in the control condition. This also allowed for a more appropriate comparison across all conditions.

The second experiment used the same general design as the first experiment, specifically, the REMEG with four-human players (i.e., human REMEG). However, the second experiment had five modifications. First, the DST was removed. The findings
from the first experiment were interesting, but were sufficient to explore the potential influence of effort avoidance. Second, since there were no findings with trait measures, the trait measures were reduced to trait trust and industriousness. Third, to compare with the first experiment and to investigate differences between counterfactual conditions, there were upward, bidirectional, downward, and control counterfactual conditions. Each condition utilized a completely randomized between subjects’ design with effort selection as the dependent variable and counterfactual condition as the independent variable with four levels (upward, bidirectional, downward, and no counterfactuals as a control). Fourth, to assess whether participants paid attention to instructions and the counterfactual manipulation, an instruction quiz and manipulation check were added. Fifth, due to data collection issues with four-person groups, whether physically present or participating on Mturk, the REMEG was revised so that single human participants played the REMEG with three condition specific EEWA bots. This change is rather significant regarding the interpretation of the results and certainly reduces the generalizability to coordination with all human groups. Therefore, the hypotheses are placed within the context of one human playing with three EEWA bots that did not receive the experimental manipulation of counterfactuals. The goal of the second experiment was to investigate if and how counterfactuals influence coordination efficiency (i.e., where players land on the payoff continuum from lowest to highest) compared to past MEG experiments.

**Hypotheses**

There are three specific, theory driven, *a priori* predictions about the difference between counterfactual conditions in the context of one human interacting with three bots that lack the experimental manipulation. Since this experiment is focused on investigating
the influence of counterfactuals on effort selections and coordination efficiency, the bots in each condition are the same besides controlled variation in forgone payoff weights that correspond to individual differences. This standardization of bot behavior allows for more clear-cut comparisons between conditions, because any differences in behavior can be attributed mainly to the human and the experimental manipulation.

**H1:** Counterfactuals (i.e., upward, bidirectional, and downward) are expected to influence coordination efficiency (i.e., Average effort) in the REMEG.

**H2:** Efficiency was expected to be higher for the upward counterfactual condition compared to downward and control conditions.

**H3:** Efficiency was expected to be higher for the downward condition compared to the control.

These hypotheses were extended by running 100 game simulations for each counterfactual condition (Figure 19). Each group was comprised of three EEWA bots and one EEWA-C bot that served as the human player. To correspond to the second experiment, the three EEWA bots only received player choices (i.e., no counterfactuals) and the EEWA-C bot received condition specific counterfactuals. The EEWA-C bots gave half weight to the forgone payoffs based on the condition specific counterfactuals. An ANOVA was performed for average effort, revealing a significant difference between conditions, $F(3,76) = 13.9, p < .001$, and post hoc comparisons revealed that all pairs were significantly different ($p < .05$) except for upward and bidirectional, and downward and control pairs. The simulations predicted support for hypothesis 1 and 2, but not for 3 since downward and control conditions were not statistically different. According to these simulations, the control condition is predicted to perform better than hypothesized.
Figure 19. Average effort (left) and average effort per round (right) for all experimental conditions. Error bars are 95% confidence intervals.

Method

The REMEG and trait questionnaire set up was nearly identical to the first experiment, except for the changes outlined above: 1) the DST was removed, 2) trait measures were reduced to trait trust and industriousness, 3) there were upward, bidirectional, downward, and control counterfactual conditions, 4) an instruction quiz and manipulation check were added, and 5) one human participant played the REMEG with three condition specific EEWA bots. The whole experiment, including the REMEG and trait questionnaires, lasted approximately 25 minutes.

Participants

A total of 260 completed the experiment on Amazon Mechanical Turk (MTurk). Participants were randomly assigned to the control ($n = 63$), upward ($n = 66$), downward ($n = 68$), or bidirectional ($n = 63$) condition. The majority of participants were male (about 65%) and the average age was 35. All participants were compensated with a base pay of $3 and the opportunity to earn an additional $2 bonus contingent on performance during the experiment. Performance pay was calculated based on following instructions
and performance on the REMEG (i.e., cumulative payoff and deductions for incorrect arithmetic problem solutions). Participants were informed about the performance pay in the experimental instructions.

**Materials**

The entire experiment was run on computers, which consisted of the REMEG and trait questionnaires. Both the REMEG and trait questionnaires were programmed and run on the O-tree studio platform (Chen et al., 2016) using a Heroku server. Links to the study were provided to participants recruited through MTurk.

**Design**

The experiment had a 4 (counterfactual condition) x 2 (player type) x 20 (round) mixed design with condition and player type as between subjects’ independent variables, and round as a within-subjects independent variable. Participants were randomly assigned to counterfactual conditions and played the game with three bots that had the same design for all conditions. In an exploratory analysis, signaler was included as a between subjects’ independent variable after collapsing conditions. Effort selections were the main dependent variable for the REMEG, but exploratory analyses included payoff, and two metrics derived from effort: distance from the minimum and intergroup variance. For the effort-related traits, the scale scores were the dependent measures. Participants were randomly pre-assigned to counterfactual conditions.

**Procedure**

Participants read a consent form, received instructions, and were informed about their compensation. Participants completed the REMEG and trait measures, which lasted approximately 25 minutes.
**REMEG.** The REMEG procedure was the same as the first experiment except for the addition of the instruction quiz, manipulation check, and counterfactual page shown after the results of each round. Although participants played the REMEG with three bots, the game procedure remained the same. The instructions informed participants they are playing a game with three other players to reduce any potential effects from explicitly telling participants they are playing with bots.

**Results**

The nested ANOVAs and correlational analyses were performed using MatLab version R2020a and all linear effects modeling was conducted using R version 4.0.3 with the lmerTest (cran.r-project.org/web/packages/lmerTest) and r2glmm statistical packages (github.com/bcjaeger/r2glmm).

**Instruction Quiz and Manipulation Check**

Before running analyses, the instruction quiz and manipulation check were explored (see Figure 20). Participants took the instruction quiz immediately after instructions and were given feedback. In addition, participants were shown the payoff matrix at the start and end of each round. Participants passed if they answered at least two of the three questions correctly. More than half of the participants failed the instruction quiz. However, a linear mixed effects model for average human effort with condition and instruction quiz as factors, and group as a random variable, revealed no main effect of instruction quiz or interaction with condition. The manipulation check results were less straightforward. Participants passed the manipulation check if their answer corresponded to their experimental condition and about 70% of participants failed. However, the same linear mixed effects model for manipulation check did not reveal any effects or
interactions. Furthermore, participants gave similar answers for the manipulation check and a Chi-squared test revealed there were no differences in answers between conditions, $\chi^2(4, N = 259) = 4.54, p = .34$.

![Figure 20](image)

Figure 20. Proportion of participants ($N = 260$) that passed and failed the instruction quiz (left), and the manipulation check (center). Answers for the manipulation check for each condition are shown on the right.

Participants were asked two additional questions related to the counterfactual manipulation (Figure 21). To assess whether participants were using counterfactuals to inform their choices, they were asked the following opened-ended question: “If you received them, did the other possible choices and outcomes influence your choices during the game?”. Only 150 (out of 260) participant’s open-ended answers could be quantified (e.g., as a yes or no) and approximately 60% them indicated they were not influenced by counterfactuals. There were no differences in responses between those who passed and failed the manipulation check. The second related question asked participants whether they thought more about how they could have made a different choice, how other players could have made a different choice, or they thought about both equally. This question was aimed at assessing whether participants were focusing more on their counterfactuals
relating towards own choices, choices of other, or both. About 30\% of participants answered their own choice or others, and about 40\% answered they focused on both equally. There were no differences in choices between those who failed or passed.

![Bar chart showing proportion of participants influenced by counterfactuals (left) and reported focusing more on their own forgone choices, those of other, or both equally (right).]

*Figure 21.* Proportion of participants influenced by counterfactuals (left) and reported focusing more on their own forgone choices, those of other, or both equally (right).

In addition, time spent on results and counterfactual pages were also explored as a means of identifying participants who were not paying attention, but this was inconclusive. Overall, it is not clear why participants failed the manipulation check; however, the benefits of counterfactuals were not expected to be contingent on awareness. Therefore, it seemed reasonable to include all of the data for the analyses.

**Unique Counterfactuals**

Since each player received a unique set of counterfactuals, this was a potential confound that need to be explored. Unique counterfactuals were quantified for each participant by subtracting the actual payoff from each counterfactual payoff and the sum for each round was calculated. A linear mixed effects model was used for counterfactual payoff difference with condition and round as fixed effects and group as a random variable (left in Figure 22). The model revealed effects for the downward, $\beta = -57.55$,  

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$t(364) = 10.61, p < .0001, \text{partial } R^2 = .15,$ and bidirectional, $\beta = -24.64, t(364) = -7.85, p < .0001, \text{partial } R^2 = .031,$ conditions indicating differences between all conditions with the downward condition having the lowest counterfactual payoff difference, followed by the bidirectional condition. In addition, there was an effect for round, $\beta = -.54, t(3740) = -4.92, p < .0001, \text{partial } R^2 = .004,$ indicating a negative trend for all conditions for counterfactual payoff difference. At the individual level, there was no relationship between player’s average counterfactual payoff difference and average effort, suggesting it was not a confound (right in Figure 22).

*Figure 22.* Average counterfactual payoff difference across 20 rounds for counterfactual conditions (left) and the relationship between each player’s average counterfactual payoff difference and average effort (right). Error bars are 95% confidence intervals.

**Main Analyses**

Based on the bot REMEG results from the first experiment where bots coordinated much faster than humans (e.g., variance dropped below 1 after the 4th round compared to humans where variance remained between 2 and 3 for the entire game) and the hypotheses of the second experiment focusing exclusively on coordination efficiency, coordination or within group variance was not explored any further.
**Effort.** A nested ANOVA was used to test the hypotheses, since players were nested within groups. The nested ANOVA had effort selection as the dependent variable, condition as the independent variable, and players nested within group as a random variable. The hypotheses predicted the upward condition to have the highest effort and the downward condition to have higher effort than the control condition. The nested ANOVA was significant (left in Figure 23), $F(3,1036) = 4.42, p = .004, \eta^2 = .01$, and post hoc Tukey HSD tests revealed higher effort in the upward condition ($M = 4.47, SD = 1.10$) compared to downward ($M = 4.13, SD = 1.05$), $t(1036) = 3.61, p < .001, d = .31$.

There were also expectations to see trends across 20 rounds as the first study and model simulations suggested no differences in first round choices and differences emerging later around round 5. Here, a linear mixed effects model was used because it has less restrictive assumptions about variance and repeated measures (e.g., Krueger & Tian, 2004). Since only human players are unique and differences between conditions were only present with all player data, player type was added as an interaction term. The following formula was used: Effort ~ Condition * Round * Player Type + (1 |
Group/Player), which corresponds to effort as the dependent variable, an interaction effect for condition, round, and player type, and players nested within group as a random effect (right in Figure 23). This model serves as the standard for all subsequent analyses.

The model revealed a significant effect for round, $\beta = -0.03$, $t(19750) = -4.89$, $p < .0001$, $\text{partial } R^2 = .001$, indicating a negative trend across rounds for effort. In addition, there were some significant interaction effects. There was an interaction effect for bots and the upward condition, $\beta = .422$, $t(3724) = 3.02$, $p = .003$, $\text{partial } R^2 = .000$, indicating that, on average, bots had higher effort than humans in the upward condition. There was an interaction effect for the upward condition and round, $\beta = .02$, $t(19750) = 2.37$, $p = .02$, $\text{partial } R^2 = .000$, and the downward condition and round, $\beta = -.02$, $t(19750) = -2.02$, $p = .04$, $\text{partial } R^2 = .000$, indicating the weakest negative trend across rounds for the upward condition and the strongest negative trend for the downward condition. These same analyses were conducted for payoff as well.

**Payoff.** Payoff is a function of the player’s choice and the minimum and therefore, provides additional information about coordination efficiency. A nested ANOVA for payoff revealed a significant difference between conditions, $F(3,1036) = 3.74$, $p = .01$, $\eta^2 = .01$ (left in Figure 24). Post hoc tests (Tukey HSD) revealed the upward ($M = 80.85$, $SD = 13.36$) and bidirectional ($M = 80.40$, $SD = 15.93$) conditions had a higher average payoff than the downward condition ($M = 77.22$, $SD = 13.85$), $t(1036) = 3.01$, $p = .01$, $d = .27$ and $t(1036) = -2.60$, $p = .05$, $d = -.21$, respectively.

Trends were also expected for payoff and this was assessed with the standard linear mixed effects model (right in Figure 24). The model revealed a significant effect for round, $\beta = .97$, $t(8408) = 10.68$, $p = .02$, $\text{partial } R^2 = .004$, indicating that payoffs had
a positive trend over time. There were also significant effects for round and the downward condition, $\beta = -0.33$, $t(19750) = -2.65$, $p = .01$, $partial R^2 = .000$, and bidirectional condition, $\beta = 0.28$, $t(19750) = 2.21$, $p = .03$, $partial R^2 = .000$, indicating that the downward condition had the weakest positive trend and bidirectional the strongest. Lastly, there was a significant interaction effect for the upward condition and bot, $\beta = -4.22$, $t(8408) = -2.34$, $p = .02$, $partial R^2 = .000$, indicating that humans had higher payoffs than bots in the upward condition.

**Figure 24.** Average payoff (left) and average payoff per round for all conditions (right). Error bars are 95% confidence intervals.

**Exploratory Analyses**

**Comparing Humans and Agents.** Groups were comprised of one human and three bots, and there was an effect for player type where bots appeared to choose higher effort than humans. To better understand this unexpected result, signaling behavior was explored. Signaling behavior was quantified by subtracting each player’s choice by the minimum to get the distance from the minimum for each round, which is informative about instances of signaling and its strength (i.e., costliness). The standard linear mixed effects model with distance from the minimum as the dependent variable revealed an
effect for round, $\beta = .064$, $t(19750) = -10.49$, $p < .0001$, partial $R^2 = .005$, indicating a negative trend across rounds. In addition, there was an effect for the upward condition, $\beta = -.35$, $t(3018) = -2.71$, $p = .007$, partial $R^2 = .001$, and an interaction effect for the upward condition and bots, $\beta = .42$, $t(3622) = 3.04$, $p = .002$, partial $R^2 = .001$, suggesting the upward condition effect was driven by the counterfactual manipulation and the effect of human signaling on the bots’ behavior in the upward condition (Figure 25).

Figure 25. Average distance from the minimum across 20 rounds for all conditions (left) and player type (right). Error bars are 95% confidence intervals.

These results suggest that signaling behavior is not independent of condition and bot’s signaling behavior may be more frequent than humans. However, bots were by design, not capable of signaling like humans. Signaling is effective if it influences other players to choose higher effort, which brings up the minimum and therefore potential payoffs for all players. Signaling is often more effective when it is known to be costly to the signaler and persists over time as it has delayed effects on other players. Effective signaling requires short-term costs (i.e., lower payoffs), which are offset by higher payoffs over time as the minimum increases. Agents only best respond to other players choices, specifically the minimum and average of all player choices for each round, and
do not consider the short-term cost of signaling with long-term rewards of higher payoffs. As bots were based on the EEWA model that considers each players choice as a potential minimum, the average choice per round is likely more attractive than the actual minimum. Also, the actual payoff received is weighted higher than all forgone payoffs, which can make previous round choices more attractive than forgone choices. Therefore, it is possible that bots engage in signaling-like behavior more than humans due to their stickiness to average effort and previous choices. This explanation was explored with a linear mixed effects model for average distance from the average. As the main interest in whether bots stuck closer to the average than humans, the model only included round and player type as interaction effects (i.e., condition was dropped). There was a significant effect for bots, $\beta = .24, t(4445) = 5.74, p < .0001, \text{partial } R^2 = .002$, indicating that bots had higher average distance from the minimum compared to humans (Figure 26). On average, bots’ choices were every close to the average without much deviation. This is because bots are based on the EEWA model and consider all player choices as potential minimums, making it more likely to converge towards the average rather than minimum.

![Figure 26. Average distance from the average across 20 rounds for player type. Error bars are 95% confidence intervals.](image)
To further explore differences between human signaling and the signaling-like behavior of bots, additional linear mixed effects models were run. Players were identified as signalers if their distance from the minimum was above zero for at least 5 rounds in a row. Five was chosen after exploration of a number of criteria (e.g., self-report measures), correspondence with previous literature (e.g., Brandts et al., 2014, 2015), and it provided the most even split between signalers and non-signalers. The standard linear mixed effects model was used for to explore effort and payoff with signalers replacing condition (i.e., Effort ~ Round * Signaler * Player type + (1 | Group/Player)). Since the effects for round and player type, and their interaction effect were already discussed for effort and payoff, they are left out here. There were several interaction effects for effort, which are included in Table 2. For simplicity, only the three-way interaction for bot, round, and signaler, $\beta = -.10$, $t(19760) = -14.52$, $p < .0001$, partial $R^2 = .006$, is reported.

Table 2.

Results of linear mixed effects model with effort as the dependent variable, round, signaler, player type as factors, and players nested within group as a random effect.

<table>
<thead>
<tr>
<th>Effect</th>
<th>Estimate</th>
<th>Std. Error</th>
<th>df</th>
<th>t-value</th>
<th>p-value</th>
<th>R-squared</th>
<th>CI-lower</th>
<th>CI-upper</th>
</tr>
</thead>
<tbody>
<tr>
<td>(Model)</td>
<td>0.055</td>
<td>0.049</td>
<td>0.061</td>
<td>53.98</td>
<td>2.0e-16***</td>
<td>0.095</td>
<td>0.049</td>
<td>0.061</td>
</tr>
<tr>
<td>(Intercept)</td>
<td>4.23</td>
<td>0.08</td>
<td>677</td>
<td>-53.98</td>
<td>2.0e-16***</td>
<td>0.004</td>
<td>0.002</td>
<td>0.006</td>
</tr>
<tr>
<td>PlayerType(bot)</td>
<td>-0.16</td>
<td>0.06</td>
<td>4996</td>
<td>-2.54</td>
<td>1.1e-2*</td>
<td>0</td>
<td>0</td>
<td>0.001</td>
</tr>
<tr>
<td>Round</td>
<td>-0.04</td>
<td>0</td>
<td>19760</td>
<td>-11.55</td>
<td>2.0e-16***</td>
<td>0.004</td>
<td>0.002</td>
<td>0.006</td>
</tr>
<tr>
<td>Signaler(yes)</td>
<td>0.57</td>
<td>0.08</td>
<td>4840</td>
<td>6.88</td>
<td>6.9e-12***</td>
<td>0.002</td>
<td>0.001</td>
<td>0.003</td>
</tr>
<tr>
<td>PlayerType(bot):Round</td>
<td>0.05</td>
<td>0.08</td>
<td>4840</td>
<td>6.88</td>
<td>6.9e-12***</td>
<td>0.002</td>
<td>0.001</td>
<td>0.003</td>
</tr>
<tr>
<td>PlayerType(bot):Signaler(yes)</td>
<td>0.43</td>
<td>0.1</td>
<td>4175</td>
<td>4.38</td>
<td>1.2e-5***</td>
<td>0.001</td>
<td>0</td>
<td>0.002</td>
</tr>
<tr>
<td>Round:Signaler(yes)</td>
<td>0.04</td>
<td>0.01</td>
<td>19760</td>
<td>6.91</td>
<td>5.1e-12***</td>
<td>0.001</td>
<td>0.001</td>
<td>0.003</td>
</tr>
<tr>
<td>PlayerType(bot):Round:Signaler(yes)</td>
<td>-0.1</td>
<td>0.01</td>
<td>19760</td>
<td>-14.52</td>
<td>2.0e-16***</td>
<td>0.006</td>
<td>0.004</td>
<td>0.008</td>
</tr>
</tbody>
</table>

Note: * $p < .05$; ** $p < .01$; *** $p < .001$

The three-way interaction for effort reveals differences between signalers and non-signalers, however, that difference is greater for humans compared to bots (left in Figure 27). Similarly, the model for payoff revealed several effects and interaction effects.
(Table 3) and the same three-way interaction for bot, round, and signaler, $\beta = -1.48$, $t(20530) = -14.07$, $p < .0001$, partial $R^2 = .006$.

Table 3.

Results of linear mixed effects model with payoff as the dependent variable, round, signaler, player type as factors, and players nested within group as a random effect.

<table>
<thead>
<tr>
<th>Effect</th>
<th>Estimate</th>
<th>Std. Error</th>
<th>df</th>
<th>t-value</th>
<th>p-value</th>
<th>R-squared</th>
<th>CI-lower</th>
<th>CI-upper</th>
</tr>
</thead>
<tbody>
<tr>
<td>(Model)</td>
<td>0.078</td>
<td>0.071</td>
<td>0.085</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(Intercept)</td>
<td>78.60</td>
<td>1.07</td>
<td>723</td>
<td>73.67</td>
<td>2.0e-16***</td>
<td>0.078</td>
<td>0.071</td>
<td>0.085</td>
</tr>
<tr>
<td>PlayerType(bot)</td>
<td>-10.63</td>
<td>0.88</td>
<td>20630</td>
<td>-12.13</td>
<td>2.0e-16***</td>
<td>0.005</td>
<td>0.003</td>
<td>0.007</td>
</tr>
<tr>
<td>PlayerType(bot):Round</td>
<td>0.56</td>
<td>0.06</td>
<td>20530</td>
<td>9.81</td>
<td>2.0e-16***</td>
<td>0.003</td>
<td>0.002</td>
<td>0.005</td>
</tr>
<tr>
<td>Signaler(yes)</td>
<td>-19.30</td>
<td>1.13</td>
<td>20630</td>
<td>-17.05</td>
<td>2.0e-16***</td>
<td>0.01</td>
<td>0.007</td>
<td>0.013</td>
</tr>
<tr>
<td>PlayerType(bot):Signaler(yes)</td>
<td>0.73</td>
<td>0.07</td>
<td>20530</td>
<td>10.33</td>
<td>2.0e-16***</td>
<td>0.003</td>
<td>0.002</td>
<td>0.005</td>
</tr>
<tr>
<td>Round:Signaler(yes)</td>
<td>20.37</td>
<td>1.34</td>
<td>20690</td>
<td>15.18</td>
<td>2.0e-16***</td>
<td>0.008</td>
<td>0.006</td>
<td>0.011</td>
</tr>
<tr>
<td>PlayerType(bot):Round:Signaler(yes)</td>
<td>-1.48</td>
<td>0.11</td>
<td>20530</td>
<td>-14.07</td>
<td>2.0e-16***</td>
<td>0.006</td>
<td>0.004</td>
<td>0.009</td>
</tr>
</tbody>
</table>

Note: * $p < .05$; ** $p < .01$; *** $p < .001$

The three-way interaction for payoff reveals differences between signalers and non-signalers for both humans and bots (right in Figure 27). Human signalers showed the expected pattern of short-term costs (i.e., lower payoffs) with the long-term benefit of higher payoffs over time by surpassing that of non-signalers.

Figure 27. Average payoff per round for humans (black) and bots (gray) that were classified as signalers (dotted line) and non-signalers (solid line). Error bars are 95% confidence intervals.
However, differences between bot “signalers” and “non-signalers” highlighted the artificiality of their signaling-like behavior as “non-signalers” earned higher payoffs than signalers”, likely due to their choice stickiness. Agent’s signaling was artificial and human signalers showed the expected cost of signaling with the long-term benefits of higher payoffs. In addition, humans were the only unique players in the group. Therefore, it was valuable to explore how human signaling affected group effort and payoff (Figure 28). Linear mixed effects models were run for average group effort (left) and payoff (right) with round and signaler as factors, and group as a random variable (i.e., Average group effort ~ Round * Signaler + (1 | Group)).

![Figure 28](image)

*Figure 28. Average payoff per round for groups with human signalers (solid line) and non-signalers (dotted line). Error bars are 95% confidence intervals.*

For average group effort, there were effects for round, $\beta = -0.045$, $t(4938) = -10.3$, $p < 0.0001$, $partial R^2 = 0.013$, and signaler, $\beta = 1.38$, $t(403) = 8.53$, $p < 0.0001$, $partial R^2 = 0.032$, indicating an average negative linear trend for average group effort across rounds and higher average group effort for groups with human signalers. There was also an interaction effect for round and signaler, $\beta = 0.043$, $t(4938) = 6.16$, $p < 0.0001$, $partial R^2 =$
.005, indicating a positive linear trend for average group effort for signalers, and a negative trend for non-signalers.

The same effects and interacting effects were found for average group payoff. The effects for round, $\beta = .56$, $t(4938) = 10.57$, $p < .0001$, partial $R^2 = .016$, and signaler, $\beta = -7.25$, $t(519) = -4.47$, $p < .0001$, partial $R^2 = .007$, indicated an average positive linear trend for average group payoff across rounds and higher average group payoff for groups with human signalers. The interaction effect for round and signaler, $\beta = .96$, $t(4938) = 11.38$, $p < .0001$, partial $R^2 = .018$, indicated a stronger positive linear trend for average group effort for signalers, compared to non-signalers. These results suggest that on average the human was able to influence the group to choose higher effort by signaling, and the human signaling resulted in higher payoffs over time for the group.

**Discussion**

In the main analyses, there was evidence suggesting counterfactual nudging can increase coordination efficiency. The hypotheses predicted nudging in the form of counterfactuals to influence coordination efficiency with the upward condition having the highest effort and the control the lowest. The nested ANOVA only revealed differences between the upward and downward conditions. The linear mixed effects model extended the nested ANOVA findings by revealing different linear trends across rounds for the upward and downward conditions. In addition, bots had higher effort than humans in the upward condition. This provides evidence that counterfactual nudging influenced coordination efficiency and that humans and bots behaved differently, however, the effect sizes were small. Prior to collecting data, there was concern that counterfactual nudging effects would be weakened since only the human received the manipulation. Although
there was supporting evidence for counterfactual nudging, the small effect sizes support that concern. Interestingly, average effort was higher than expected in the control condition. There were no counterfactuals or additional outcome information beyond the minimum, however, the control condition was almost identical to the bidirectional condition and had higher effort (not significant) than the downward condition.

Furthermore, although all the data was included, the fact that 70% of the participants failed the manipulation check and about half failed the instruction quiz was still concerning. This could be due to participants not taking the experiment seriously, not paying attention to the counterfactuals, getting confused by the counterfactual manipulation (suggested by similar answers across conditions), or generating their own counterfactuals and ignoring the ones provided. It might also relate to metacognition, specifically how players form beliefs about themselves and others, and whether they can accurately update them over time. It is possible that participants were influenced by counterfactuals but were not aware of how it influenced their behavior. If that is the case, they may have been less able to form accurate beliefs about other players, which would reduce their ability to make appropriate choices to improve their payoffs.

In the exploratory analyses, evidence suggested that (1) humans and bots behaved differently in respect to effort, payoff, and signaling, and (2) one human signaler, in a group of three equivalent bots, increased coordination efficiency. The linear mixed effects model with distance from the minimum revealed a weak relationship with condition, as there was one interaction effect (i.e., upward condition and bots). Signaling could be seen as individuals nudging each other and may have been stronger or at least additive to counterfactual nudges. This could be due to counterfactuals lacking context
beyond the minimum, since all player choices were shown on the results page prior to counterfactuals. Signaling could help explain why the control condition performed better than expected. Although there were no counterfactuals, signaling was still possible and, without information of all player choices, players could not properly assess signaling effectiveness and if or when to give it up. Furthermore, bots in the control condition had access to all player choices, which could have helped keep effort higher and more stable.

Player type differences were found in the main analysis with effort and exploratory analysis with distance from the minimum, where bots had higher average effort and distance from the minimum than humans. In addition, an analysis with average distance from the average revealed that bots stuck very close to the average for all 20 rounds, suggesting they were more influenced by the average than the minimum or higher choices potentially related to signaling. To better understand these differences, these findings were followed up with exploratory analyses by classifying signalers and including it as a factor in separate linear mixed effects models with effort and payoff as dependent variables. For effort, “signaling” and “non-signaling” bot’s effort choices were similar, whereas signaling human’s effort was about 1.5 effort units higher than non-signaling humans. For payoff, signaling humans increased their payoff over time, compared to non-signaling humans who flattened out after about 5 rounds. On the other hand, “signaling” bots had the lowest payoffs and “non-signaling” bots had the highest. Also, there were more bot “signalers” (62%) compared to humans (39%). The differences between signaling and non-signaling humans is the expected pattern for effective signaling behavior. However, the behavior of signaling and non-signaling bots is unusual. As previously mentioned, bots signaling-like behavior is merely an artifact of the
influence of mode(s) and previous choices. That being said, bot’s choices could often be maladaptive in terms of their payoff and the coordination efficiency of the group.

The linear mixed effects models with signaling as a factor revealed differences between signaling and non-signaling humans and bots for both effort and payoff. However, since only humans are capable of signaling, subsequent exploratory analysis were used to explore the behavior of groups with signaling and non-signaling humans. The linear mixed effects models revealed groups with signaling humans had significantly higher effort and payoffs compared to groups with non-signaling humans. This result is promising because it suggests that the single human in the group had awareness of the need for signaling persistence and succeeded in influencing the behavior of three equivalent bots, regardless of counterfactual nudges.

Overall, the results are somewhat promising for counterfactual nudging and more so for signaling. The cards were stacked against the human since bots did not receive counterfactuals, were not capable of signaling, and may be hard to influence. There was some evidence that upward counterfactuals nudged humans towards choosing higher effort and this influenced the group. Furthermore, signaling humans were able to nudge bots towards more efficient choices regardless of experimental condition. A follow up study is necessary to rule out potential confounds by: (1) developing more human-like bots capable of signaling and giving them counterfactuals, or using four-human groups, (2) giving all players in the control condition the same information, (3) providing a clearer counterfactual manipulation with relationships to group behavior (e.g., if other players had chosen one effort unit higher, you would have earned X), and (4) finding a better way to assess whether participants paid attention and followed instructions.
IV. A Cognitive Model of the MEG

The first experiment suggested counterfactuals improved coordination efficiency and helped keep it stable over time, compared to previous experiments (Leng et al., 2018; van Huyck et al., 1990, 1991). The second experiment supported and extended this finding by revealing that counterfactuals, specifically upward and downward, can nudge individuals and influence coordination efficiency. Furthermore, players (i.e., humans) appeared to nudge other players (i.e., bots) to choose higher effort by signaling. However, groups in the second experiment were comprised of one human and three EEWA bots that did not receive counterfactuals, were not capable of signaling, and exhibited artificial choice stickiness. These behavioral constraints of the bots reduce the generalizability to groups comprised of humans. Unfortunately, there were issues with completing a follow up experiment with four human groups and there were additional constraints due to the COVID-19 pandemic. Therefore, instead of a third experiment using the same EEWA bots, a new model was developed with the intention to better capture and simulate human behavior and cognitive processes. As described in both previous experiments, the EEWA was an improvement over EWA, but it lacked the capability to form beliefs about or predict other players choices and there was frequent maladaptive choice stickiness. The EEWA model was able to approximate average behavior, but not the group dynamics observed in all human groups. In addition, the mathematical equations and parameters in EWA and EEWA had some psychological correspondence, but overall, lacked cognitive plausibility. The new model addresses these deficiencies and includes: 1) counterpart
choice predictions, 2) decision strategies, 3) counterfactual thinking (i.e., mental simulation), and 4) the capability to fit both average behavior and group dynamics. Due to its capabilities, the model is referred to as the prediction, strategy, and simulation model (i.e., PSS). The PSS model was implemented in a cognitive architecture, specifically ACT-R (Anderson & Lebiere, 1998; Anderson, 2009). To thoroughly evaluate the model, it was fit to the 4-human group data from the first experiment and compared to the EWA and EEWA models. The development of the PSS model, its main mechanisms, and a detailed description of the model processes are discussed, starting with a brief overview of the ACT-R architecture.

The ACT-R Architecture

ACT-R is a unified theory of cognition used to develop models of various tasks and phenomena (Anderson, 1993; Anderson & Lebiere, 1998; Anderson, 2007). It is a hybrid architecture with both symbolic and sub-symbolic structures and is composed of modules, which represent the systems of the mind. Each module possesses a buffer that serves as the interaction interface between modules and the contents of the buffers represent the current state of the model. There are several perceptual-motor modules (e.g., visual, motor, aural, and vocal) and two types of memory modules (i.e., declarative and procedural). The PSS model uses the goal, imaginal, declarative, and procedural modules. The goal module is used to determine what the model is currently focused on and the imaginal module is used to temporarily store information similar to visual short-term memory. The declarative memory module represents facts in long term memory that are stored as chunks and a sub-symbolic component determines the behavior of the memory system (e.g., the likelihood a chunk can be retrieved). The procedural module
represents knowledge about how to do things, which are represented as condition-action rules. The pattern matcher of the procedural module determines which, if any, rules match the current state of the buffers. If the condition of the production matches the current state, then it “fires” and the action changes the state of the model. The behavior of a model is represented as a series of production firings and corresponding changes to the state of the model.

The PSS Model

The main components of the PSS model include: predictions about other players, strategies, and learning. At the end of each round in the MEG, players are shown the choices of all players and receive feedback in the form of payoffs. Players accumulate information over time, are better able to predict other player’s choices, and identify which strategies tend to result in higher payoffs. The instance-based learning approach (IBL; Gonzalez, Lerch, & Lebiere, 2003) is used as a framework for how players accumulate and leverage this information. In IBL, information is stored as instance chunks, which include the situation, decision, and value of the decision in that situation. In typical IBL models (e.g., Gonzalez, Lerch, & Lebiere, 2003), instances are accumulated and leveraged to determine which decision leads to the highest utility for a given situation, resulting in a transition from relying on productions to relying on declarative memory. However, the PSS model uses a slightly different approach (e.g., Juvina, Lebiere, & Gonzalez, 2015). Instances store more information about the situation, are used only to make predictions about other players, and decisions remain a function of procedural memory (in combination with player choice predictions). In addition, there is no defined stopping rule regarding when to stop considering all possible decisions prior
to making a decision. Instead, a strategy is chosen and a decision is made in combination with player choice predictions, then the unchosen strategies are simulated to represent counterfactual thinking. In the PSS model, learning about counterparts is a function of declarative memory, and learning which strategies have higher utilities (i.e., rewards) is a function of procedural memory. These learning processes are discussed in more detail separately in the following sections, followed by a complete description of the model.

**Player Choice Predictions and Declarative Memory**

In the minimum effort game, players often have delayed responses to the choices of other players since they make choices simultaneously and often make choices in response to the previous round. Therefore, predicting what players are going to do involves how players react to previous rounds and tracking reactions over time. In the PSS model, round outcomes are stored as instances that contain all player choices from the previous round \([t - 1]\), all player choices from the current round \((t)\), the decision for the current round \((t)\), and the payoff of the current round \((t)\). Storing the previous round choices and the subsequent round choices carries information about how players choices change from one round to another.

In the model, player choice predictions involve recognizing a situation (i.e., previous round choices) by finding an instance that has corresponding previous choices (i.e., choices \([t - 1]\)) and using the known subsequent choices or reactions in the instance (i.e., choices \([t]\)) to make predictions. In retrieval terms, this means that previous round choices are the retrieval cues, choices \([t - 1]\) are the target in the instance chunk, and choices \([t]\) in the instance chunk are the values of interest within that instance chunk. As more instances are accumulated, there is more information to leverage and predict if and
how player’s choices vary across rounds. Rather than just recalling an instance from memory, the blending mechanism (Lebiere, 1999) is used here to aggregate information from all previous instances and serves as player choice predictions. Before describing the blending mechanism, it’s important to first consider activation. Every instance (i.e., chunk) has an associated activation, which helps determines the likelihood of its retrieval. The activation of a chunk is determined by the activation equation (equation 4).

\[
A_i = B_i + S_i + P_i + \epsilon_i
\]

The activation of a chunk, \(A_i\), is a function of the: 1) base level term, \(B_i\), that represents recency and chunk use, 2) spreading term, \(S_i\), that represents context effects, 3) partial matching term, \(P_i\), that represents how well the chunk matches the retrieval cues, and 4) noise term, \(\epsilon_i\), that represents variability in memory. However, for simplicity the PSS model uses blending instead of retrieval and therefore only includes the partial matching, \(P_i\), and error, \(\epsilon_i\), terms of the activation equation. The partial matching term was set to 1 (default is 0) and functions as a mismatch penalty for chunks (i.e., magnitude of the mismatching chunk activation is reduced) that are not an exact match to the retrieval request cues. The partial matching term was kept low, compared to the value of 10 used in Lebiere (1999), since the model does not start with instances and there are only 20 rounds. If partial matching was higher, mismatching chunks would be penalized more and fewer chunks would have an impact on the blended player choice predictions. The error term was left at its default value of 1. The error represents the standard deviation in a normal distribution with a mean of 0. A value is randomly sampled from this distribution and adds random noise or variation to activation.
Contrary to standard retrieval, which retrieves one chunk based on retrieval cues and activation, the blending mechanism retrieves a compromise value for all possible values of interest weighted by their probability of retrieval. This is accomplished with equation 5, which produces a value that minimizes the sum of all squared dissimilarities for values, \((1 - \text{sim}(V, V_i))^2\), of each chunk, \(i\), and weights it by its probability of retrieval, \(P_i\) (equation 6). The probability of retrieval is a function of the match score for a chunk, \(e^{M_i/t}\), that represents the degree of match between the retrieval cues and the target information in the chunk. The match score is normalized by the total match score of all retrieved chunks, \(\sum_j e^{M_j/t}\).

\[
\begin{align*}
(5) \quad V &= \min \sum_i P_i \times (1 - \text{sim}(V, V_i))^2 \\
(6) \quad P_i &= \frac{e^{M_i/t}}{\sum_j e^{M_j/t}} \quad \text{(i.e., Boltzmann equation)}
\end{align*}
\]

Blending can deal with different types of variables; however, the discussion is restricted to the integer values used in the PSS model. When using integers, the compromise value is the sum of all chunk values of interest weighted by their probability of retrieval (normalized). If two chunks in memory: Chunk A has target of 2 and value of 3, and chunk B has a target of 3 and value of 4. If the retrieval cue was 2.1, then the probability of retrieval for chunk A is going to be higher and its value will receive more weight. Let’s say that the probability of retrieval for chunk A is .9 and .1 for chunk B. The blended value is then the sum of each chunk’s value weighted by its respective probability of retrieval (equation 7).

\[
(7) \quad (3 \times .9) + (4 \times .1) = 3.1
\]
As mentioned, in the PSS model the blending mechanism produces player choice predictions by leveraging the instances in memory. At the start of a new round, player choices from the previous round are used as retrieval cues and the blending module retrieves instances that have matching or similar values in the target slots representing choices at time \( t - 1 \) (see Figure 26). The values for time \( t \) are blended by multiplying the set of player choices for time \( t \) by their own probability of retrieval and calculating the sum. Equation 8 below is an example expression of how three sets of player choices would be blended to produce player choice predictions.

\[
\begin{align*}
(8a) \quad & (\begin{bmatrix} 4 & 4 & 5 & 6 \end{bmatrix} \times 0.7) + (\begin{bmatrix} 2 & 4 & 5 & 4 \end{bmatrix} \times 0.2) + (\begin{bmatrix} 2 & 3 & 4 & 3 \end{bmatrix} \times 0.1) = \\
(8b) \quad & [2.8 \ 2.8 \ 3.5 \ 4.2] + [0.4 \ 0.8 \ 1.8] + [0.2 \ 0.3 \ 0.4] = \\
(8c) \quad & [3.4 \ 3.9 \ 4.9 \ 5.3] = \\
(8d) \quad & [3 \ 4 \ 5 \ 5]
\end{align*}
\]

In expression 8a there are three sets of player choices, in brackets, being multiplied by their probability of retrieval (e.g., expression 6) and expression 8b shows the product for each player choice in that set. Next, each specific player choice for each set is summed (expression 8c). Lastly, these summed player choices are rounded to get discrete integers that corresponded to effort choices (expression 5d). Notice how these blended values are unique and represent all the information in chunks weighted by their degree of match to the retrieval cues, which is formally represented by the probability of retrieval. These blended values then serve as the prediction for players choices for the next round. Just like retrieval requests, blending can fail if the activation for the blended chunk is below the activation threshold (default of 0). The activation of the blended
chunk is a function of the activation of all chunks that were blended. If it fails, then the model uses the choices from the previous round as player choice predictions (Figure 29).

Figure 29. Depiction of how instances are used to predict player choice patterns with blending.

**Player Choice Strategies and Procedural Memory**

In order to make a choice, players use some kind of strategy or rule based on preferences or previous experience. There are, of course, several ways to interpret how players make choices and if there are changes over time. The EWA model uses choices and the most “attractive” choice at a given point in time is selected. Attractions for all choices are updated at the end of each round based on the reward for the actual choice and lower weighted payoffs for all forgone choices (i.e., choices not made). There are two potential side effects of this approach: EWA bots may stick close to previous choices since forgone choices receive a fraction of the payoff and bots are likely to be pulled towards the minimum because it leads to the highest payoff possible. The EEW model was developed to improve the coordination ability of the EWA model, reduce choice stickiness, and reduce the pull towards the minimum by considering each player’s choice as a potential minimum. However, this has computation costs as the EEW model considers the actual outcome and 27 forgone ones. To reduce this computation cost and incorporate unique player choice predictions for each player, the PSS model uses a combination of player choice predictions and strategies to make choices. As outlined in
the previous section, at the start of a new round, the PSS model uses instances and blending in order to predict player choices. These predictions are then used in combination with strategies to make a choice. The model includes four strategies: 1) the min-strategy selects the lowest predicted choice, 2) the ave-strategy selects the average predicted choice, 3) the max-strategy selects the highest predicted choice, and 4) the signal-strategy selects one higher than the average predicted choice. Once a strategy is selected and all other players have made a choice, then the model receives feedback and updates the utility of the chosen strategy based on the actual (not predicted) outcome.

Strategy utility is updated using the ACT-R utility learning equation (equation 9).

\[
U_i(n) = U_i(n-1) + \alpha [R_i(n) - U_i(n-1)]
\]

(9)  \( U_i(n) = U_i(n-1) + \alpha [R_i(n) - U_i(n-1)] \)

In the utility learning equation, the current utility, \( U_i(n) \), is a function of: 1) the previous utility value, \( U_i(n-1) \), 2) the utility learning rate, \( \alpha \), and 3) the current reward value, \( R_i(n) \). In addition, there is an optional noise component that can be added to utilities, which adds some stochasticity (Anderson, 2007). When the utility is updated, the previous utility is added to the difference between the current reward and the previous utility multiplied by the utility learning rate. For example, if the previous utility of a strategy is equal to 70, a new reward of 100 is received after using that strategy, and the learning rate was .2 (i.e., default value in ACT-R), then the updated utility would be:

\[
70 + (.2 \times (100 - 70)) = 76
\]

(10)  \( 70 + (.2 \times (100 - 70)) = 76 \)

Notice that even though the most recent reward is 30 more than the previous utility, the new utility only changes by 6. This is because the learning rate is set at .2, so only 20% of the difference between previous utility and current reward is added to the previous utility. If the reward for this strategy continued to be 100, it would take 19
updates until the current utility would be equal to the reward of 100 (after rounding up). Therefore, the first or starting utility of a strategy is important as it influences which strategies are initially selected and how quickly utilities changes over time. In the PSS model, there are two different patterns of starting strategy utilities that represent the two most commonly discussed player types in the MEG: risk and payoff dominant (e.g., van Huyck et al., 1990, 1991). A risk dominant player (i.e., RD) is motivated to reduce risk or potential cost and would prefer choosing lower effort as there is less potential risk (e.g., choosing 1 guarantees a payoff of 70). A payoff dominant (i.e., PD) player is more willing to take risk in order to seek higher rewards (e.g., choosing 7 can result in the highest payoff of 130 or the lowest of 10). The RD player type is approximated by organizing strategies along a riskiness continuum (i.e., min, ave, max, and signal) and setting the min-strategy at the highest attainable payoff (i.e., 130) and linearly decreasing utilities along this riskiness continuum (Figure 30). The PD player type is defined as the opposite of risk dominant.

Figure 30. Line plot showing starting strategy utilities for RD and PD player types.

In the PSS model, payoffs are the rewards given to strategies and the utility of all strategies are updated each round. To add some stochasticity, the noise component is
added to the utility equation. The noise component for utility is the same as activation noise (i.e., values sampled form a normal distribution with a mean of 0 and standard deviation equal to the noise value) and the default value is 1. However, since payoffs can be as high as 130, the noise component had to be higher as well. In the model, the noise component was a free parameter that was determined to be 7.5 through model fitting. On a given round, the strategy with the highest utility is chosen and the player choice predictions are used to determine the choice along with the chosen strategy. Although the chosen strategy uses player choice predictions, it receives a reward equal to the payoff earned at the end of the round based on the real player choices. For instance, if player choice predictions were 2, 3, 5, 6 and the average strategy was used, the choice would be 4. If the actual player choices were 3, 3, 5, 6, then the minimum of 3 and choice of 4 would result in a payoff of 80, instead of 60 if the minimum was 2 as predicted. After the chosen strategy receives a reward, the model engages in counterfactual thinking and simulates outcomes for the unchosen strategies in the same manner (i.e., using player choice predictions and real player choices determine the payoff). However, the unchosen strategies receive a fraction of the payoff that could have been earned to better correspond to counterfactual thinking (Byrne, 2016; Kahneman & Miller, 1986) and the idea that it is treated differently than actual outcomes (Camerer & Ho, 1999). In the PSS model, unchosen strategies receive 75% of the forgone payoff (e.g., payoff * .75) and this is referred to as the counterfactual weight in the model. Following from the example above, when updating the utility of the unchosen max-strategy, this would mean choosing the predicted highest choice of 6 and receiving a weighted payoff of 45 (60 * .75) based on the actual minimum of 3.
Complete Model Description

So far, the declarative and procedural components of the model have been discussed separately. In this section the complete model is described, starting with the model parameters.

Model Parameters. The declarative memory component of the model has two architectural parameters: partial matching and activation noise. Since there is not default for partial matching, it was a free parameter determined to be 1 through model fitting. Setting it at 1 minimized the mismatch penalty so that all chunks can influence player choice predictions since there are no instance in declarative memory at the start of the game and there are only 20 rounds. Activation noise is required for blending and was left at its default value of 1. For the procedural memory, there are two fixed architectural parameters (i.e., utility learning rate and utility noise) and two parameters based on theoretically justified assumptions (i.e., starting utilities and counterfactual weight). The utility learning rate is set at the default value of .2 and utility noise was scaled up to better correspond to payoff values and was set at 7.5 (i.e., default is 1) during model fitting. The counterfactual weight (i.e., cfw) parameter was added to differentially weight payoffs for simulated strategies during counterfactual thinking. The cfw parameter was set to .75 so that counterfactual payoffs have 75% of the value as actual payoffs. There are two player types that correspond to a pattern of starting strategy utilities and they can compose five unique groups. In the final model run, there were slightly more PD player types (57%) resulting in uneven unique group frequencies (Table 4), suggesting that there might be more PD than RD player types in the sample. The model was modified to consistently reproduce these frequencies.
Table 4.

*All possible groups with risk and payoff dominant player types.*

<table>
<thead>
<tr>
<th>Unique Groups</th>
<th>Player 1</th>
<th>Player 2</th>
<th>Player 3</th>
<th>Player 4</th>
<th>Group Frequency</th>
</tr>
</thead>
<tbody>
<tr>
<td>Group 1</td>
<td>Risk</td>
<td>Risk</td>
<td>Risk</td>
<td>Risk</td>
<td>2.0%</td>
</tr>
<tr>
<td>Group 2</td>
<td>Payoff</td>
<td>Risk</td>
<td>Risk</td>
<td>Risk</td>
<td>23.0%</td>
</tr>
<tr>
<td>Group 3</td>
<td>Payoff</td>
<td>Payoff</td>
<td>Risk</td>
<td>Risk</td>
<td>32.0%</td>
</tr>
<tr>
<td>Group 4</td>
<td>Payoff</td>
<td>Payoff</td>
<td>Payoff</td>
<td>Risk</td>
<td>35.0%</td>
</tr>
<tr>
<td>Group 5</td>
<td>Payoff</td>
<td>Payoff</td>
<td>Payoff</td>
<td>Payoff</td>
<td>8.0%</td>
</tr>
</tbody>
</table>

**Model Processes.** The model goes through the same general process for each round; however, the first round is unique. During the first round, the model randomly selects a player type, predicts choices for the three other players, and makes a choice. As mentioned, one of the two possible player types is randomly selected. The player choice predictions and choice of the model are randomly sampled from the probability distribution of pooled human first round choices from both the first and second experiment. Once all players make a first round choice, the model skips the situation recognition and instance blending (i.e., declarative memory), and strategy productions (i.e., procedural memory) in order to move directly to the results. For all subsequent rounds, the model goes through the same general process, which is now described in detail (Figure 31). After the first round, the model is focused on actual player choices from the end of the last round, which are stored in the goal buffer. The model then attempts to recognize the situation by making a blending request with those player choices as retrieval cues. The partial matching parameter penalizes mismatching chunks so that the best matching chunks carry more weight in the blended choices and activation noise is added to the activation equation that determines the blended chunk’s activation.
If blending is successful (i.e., the blended chunk’s activation is above the default threshold of 0), the blended choices replace the last round choices in the goal buffer and represent player choice predictions. If blending is not successful (i.e., the blended chunk’s activation is below the threshold), then last round choices are not replaced in the goal buffer and serve as the player choice predictions. In both cases a chunk is created in the imaginal buffer to store last round choices in a new instance (i.e., player choices \( t-1 \)). Next, the model selects the strategy (i.e., min, ave, max, or signal) that has the highest utility and uses those player choice predictions to make a choice. If one or more counterparts are taking longer to make a choice, the model waits. After all counterparts have made a choice, the model moves to the results and is “shown” all player choices, its own payoff, and a reward is triggered equal to that payoff. Although the choice was determined by the player choice predictions, the reward is determined by the actual player choices and is a function of the minimum and the distance from the minimum. The

**Figure 31.** Simplified diagram of the PSS model processes (see Legend).
utility of the chosen strategy is updated using the utility equation that includes the previous utility, the reward, the learning rate (i.e., .2), and the utility learning noise (i.e., 7.5). In addition, during the results production, information is added to complete the instance chunk in the imaginal buffer, including the new player choices (i.e., player choices \([t]\)), the decision made by the model (i.e., decision \([t]\)), and the payoff (i.e., payoff \([t]\)). However, the predicted player choices remain in the goal buffer and the instance chunk remains in the imaginal buffer so that counterfactual thinking can take place. Next, the unchosen strategies are simulated one at a time using the player choice predictions, that are still in the goal buffer, in the same manner as actual strategies. The forgone payoffs, based on the actual choices stored in the imaginal buffer, are weighted by the cfw parameter (i.e., .75) and are used as the reward in the utility equation to update the utility of the strategy productions. Once all the unchosen strategies are simulated, the model stops counterfactual thinking. The model replaces the player choice predictions in the goal buffer with the actual choices from the current round, that are stored in the imaginal buffer, so they can be used at the start of the next round. The instance chunk is then cleared from the imaginal buffer and is added to declarative memory. At this point, the round ends, and the model repeats the whole process again for subsequent rounds.

**PSS Model Results**

The PSS model was fit to the 4-human group data from the first experiment. There were few groups for MEG-first \((n = 8)\) and DST-first conditions \((n = 10)\), so the data was pooled \((N= 18)\). The PSS model had a good fit to the pooled data for average effort, \(r(38) = .4, RMSE = .27\), but the fit to variance was less than desired, \(r(38) = -.13, RMSE = 1.6\) (left in Figure 32).
Figure 3.2. Average effort and variance across 20 rounds for PSS model and human data (left) and relationship between average group effort and variance for PSS model and human data (right).

The decrease in variance over time for the PSS model suggests that individual models (i.e., players) were better able to coordinate over time compared to humans. This pattern was also observed in the first experiment where there was no difference in effort, but bot groups (i.e., one human and three EEWA bots) had lower variance than all-human groups since the EEWA bots were conducive to coordination. In addition to average effort and variance, behavior was also explored at the group level by comparing the relationship between average group effort and variance (right in Figure 3.2). There was a strong negative correlation between average group effort and variance for the PSS model, \( r(98) = -.48, p < .0001 \), compared to the non-significant, negative relationship with the human data, \( r(16) = -.38, p = .12 \).

So far, the PSS model appears to have a relatively good fit to average effort and captures the relationship between average group effort and variance. However, average behavior and average group behavior can be misleading, and there was a discrepancy with average variance across 20 rounds. Therefore, group dynamics are explored. In
Figure 33, the average effort of eight different 4-human groups is shown (left) and they can be qualitatively compared to the average effort of eight different PSS model groups (right).

*Figure 33. Average effort for eight 4-human groups (left) and eight 4-PSS model groups (right).*

About three of the eight human groups displayed dramatic fluctuations in effort and the remaining five groups were more stable. The PSS model groups show fluctuation, but lack these dramatic shifts in average effort. For a more quantitative comparison of group dynamics, the relationship between average player distance from the minimum and variance was explored (Figure 34). A player’s average distance from the minimum is informative for the frequency and magnitude of choices above the minimum and average player variance shows the consistency or “stickiness” of player choices over time. If a player had no choice variance and high distance from the minimum, this would suggest maladaptive behavior. Signaling is costly and results in lower payoffs, however, if the signaling if effective and other player choose higher, this cost is offset by the resulting higher payoffs. On the other hand, if a player has no choice variance and low distance
from the minimum, the player is likely sticking to the minimum regardless of other players choices.

*Figure 34.* Scatterplots showing the relationship between average player distance from the minimum and variance for the human groups (left) and the PSS model groups (right).

There was a significant positive relationship between average player distance from the minimum and variance for human groups, \( r(70) = .32, p = .007 \), and PSS model groups, \( r(398) = .54, p < .0001 \). Humans had higher average player variance (\( M = 2.77, SD = 1.87 \)) than the models (\( M = 1.01, SD = .85 \)), and this difference was significant, \( t(470) = -12.9, p < .0001, d = -1.66 \). Approximately 3% of humans and 7.5% of PSS model players had no choice variance, which means they made the same choice every round regardless other player’s choice. Humans also had higher average player distance from the minimum (\( M = 1.32, SD = .96 \)) than the models (\( M = .92, SD = .65 \)), which was significant, \( t(470) = 4.37, p < .0001, d = -0.56 \). This is another piece of evidence suggesting PSS model “players” were more efficient at coordinating in groups. To better understand why, effort and payoffs (Figure 35), as well as strategy utilities (Figure 36) for all PSS model “players” are explored for one group.
Figure 35. Effort (left) and payoffs (right) for each player (i.e., model) in the example group across 20 rounds.

In the first round, player 2 (i.e., gray line with “x” marker) sets the minimum (i.e., 3), and all other players choose higher (left in Figure 35). Player 3 (i.e., black dotted line with diamond marker) then sets the minimum between rounds 2 and 13, then appears to switch strategies at round 14 by choosing the highest effort of the group. At this same time, player 2 becomes the new minimum setter after a slightly erratic pattern of choices. This pattern of minimum setting is easier to see by looking at payoffs (right in Figure 35), because the minimum setter has the highest payoff in a given round. Behavior of player 2 and 3 is better understood by looking at strategy utility changes over time (Figure 36). In this group, player 3 is the only risk dominant player-type as all other players are payoff dominant. Between rounds 2 and 13, player 3 sets the minimum and earns the highest payoff. However, during this time, the utility of the minimum strategy gradually declines while the signal strategy gradually increases. The minimum strategy starts out with a utility of 130 and between rounds 2 and 13, the average payoff earned was about 95. This decline is explained by the utility learning equation, which adds the difference between the new payoff and previous utility (e.g., $95 - 130 = -35$) weighted by the learning rate (e.g., $0.2 \times -35 = -7$) to the previous utility (e.g., $130 + -7 = 123$).
Figure 36. Strategy utilities for each player (i.e., model) in the example group across 20 rounds. The starting utilities for strategies are shown at round 0.

As long as the payoff is less than the previous utility value, the utility will continue to decrease. The signal strategy starts off with a utility of 36 and between rounds 2 and 13, the average forgone payoff is approximately 106 (i.e., 80 after counterfactual payoff weighting). Therefore, the increase in utility for the signal strategy is due to the difference between the new payoff and previous utility (e.g., 80 – 36 = 44) weighted by the learning rate (e.g., .2 * 44 = 8.8) added to the previous utility value for the signal strategy (e.g., 36 + 8.8 = 44.8). At round 14, player 3 switched from minimum to the signal strategy and chose 6 and paid a cost as the minimum was 4 that round. Player 3 received a payoff of 80, which was applied to the signal strategy, and a forgone payoff of
100 for the minimum strategy. This resulted in the minimum strategy becoming dominant again for player 3. On the other hand, player 2 is payoff dominant and starts out using the signal and average strategies. After paying the cost of choosing higher than the minimum several times during the first 13 rounds, the minimum strategy becomes dominant for the remainder of the game. Interestingly, the payoff dominant players were able to influence the risk dominant player (i.e., player 3) to choose higher and eventually to make the highest choice in the group. However, it was “too little too late” as the players that chose strategies leading to higher effort paid a cost and received lower payoffs, and eventually learned that the minimum or average strategies tend to result in higher payoffs. This trend towards selecting minimum and average strategies due to learning over time helps explain why the PSS model has a negative trend in variance over time.

Overall, the PSS model is able to capture average behavior over time, the relationship between group effort and variance, and to a lesser degree, group dynamics. Next, the PSS model is compared to the EWA and EEWA models.

**Model Comparisons**

To properly evaluate the PSS model, it is compared to the EWA and EEWA to assess whether it can better capture human behavior in the MEG. To allow for an apples-to-apples comparison, EWA and EEWA were both implemented in ACT-R. They are referred to as ACT-R EWA and ACT-R EEWA. Due to the nature of ACT-R models as process models, in addition to quantitative model comparisons, the models are compared qualitatively as well. Prior to comparing the PSS, ACT-R EWA, and ACT-R EEWA models, the EWA and EEWA are compared to their ACT-R implemented counterparts to ensure they overlap and have a comparable fit to the human data.
ACT-R Implementations of EWA and EEWA

The choice attractions in the EWA and EEWA models correspond with utility learning for procedures in ACT-R, which already has equations to handle procedural memory. Therefore, both ACT-R EWA and EEWA models use only utility learning with choice rules and only three parameters: utility learning rate, utility noise, and forgone payoff weight (i.e., delta in EWA).

At the start of each round, a model selects the choice rule with the highest utility (see Figure 37). After making a choice, the model receives a reward equal to the payoff earned based on its choice and the minimum choice in the group. Next, the model simulates the unchosen choice rules to represent counterfactual thinking where the actual minimum is used to determine the weighted forgone payoff for each choice (excluding the actual choice). After all player made a choice and simulated forgone choices, the round is over.

![Diagram showing the processes in ACT-R implementation of EWA (left) and EEWA (right) models.](image)

*Figure 37.* Processes in the ACT-R implementation of the EWA (left) and EEWA (right) models.

Starting utilities were set between 0 and 2, and corresponded with first round choice probabilities of humans. This resulted in similar first round choices as humans and setting them low allowed first round choices to have a strong initial impact on utilities.
just like the EWA model. For the ACT-R EWA model, utility noise was fixed at 1 (default), utility learning rate was a free parameter that was determined to be .1 through model fitting, and delta was sampled at random for each player and varied between .1 and .15. Just like the EEWA, the ACT-R EEWA model was extended to include forgone outcomes for the minimum and other player’s choices as potential minimums. The ACT-R EEWA went through the same process as EEWA, but for four sets of simulations for unchosen choice rules (i.e., one for each player’s choice). For the ACT-R EEWA, utility noise was fixed at 2, utility learning rate was fixed at .2 (default), and delta was a sampled at random for each player and varied between .98 and 1.

*Figure 38.* Model fits to human average effort and variance for EWA and ACT-R EWA (left) and EEWA and ACT-R EEWA (right)

The ACT-R EWA fit to the human data (left in Figure 38) for average effort, $r(38) = 0.51$, $RMSE = 0.82$, and variance, $r(38) = -0.10$, $RMSE = 0.49$, was comparable to EWA for average effort, $r(38) = 0.52$, $RMSE = 0.96$, and variance, $r(38) = -0.10$, $RMSE = 0.53$. However, the ACT-R EEWA fit to the human data (right in Figure 38) for average effort was not quite as good, $r(38) = 0.47$, $RMSE = 0.42$, as EEWA, $r(38) = 0.55$, $RMSE$
= 0.31. The difference in fit for human variance was even greater, with EEWA fitting better, \( r(38) = -0.06, \text{RMSE} = 0.42 \), than ACT-R EEWA, \( r(38) = -0.19, \text{RMSE} = 1.78 \).

In addition, the relationship between average player variance and distance from the minimum was explored for each model. There was a significant positive correlation between average player distance from the minimum and variance for the EWA model, \( r(398) = .31, p < .0001 \), but no correlation for ACT-R EWA, \( r(398) = -.03, p = .49 \) (Figure 39). Approximately 40% of all EWA players had no variance (i.e., player variance of zero in Figure 39) compared to about 70% for ACT-R EWA.

![Figure 39](image)

*Figure 39. Scatterplots showing average player distance from the minimum (i.e., MinDist) and variance for the EWA (left) and ACT-R EWA (right) models.*

This significant amount of choice stickiness is resulting in artificial fits to both average effort and variance, since only about 3% of humans had no choice variance. The EWA and ACT-R EWA models are eliminated from the model comparison because of their failure to capture group dynamics and the artificial fitting of average behavior. Next, the average player distance from the minimum and variance for EEWA and ACT-R EEWA are compared (Figure 40).
Figure 40. Scatterplots showing average player distance from the minimum (i.e., MinDist) and variance for the EEWA (left) and ACT-R EEWA (right) models.

There was a significant positive correlation between average player distance from the minimum and variance for EEWA, $r(398) = .15$, $p = .003$, and ACT-R EEWA, $r(398) = .45$, $p < .0001$. Similar to EWA models, there is also evidence for choice stickiness. Approximately 40% of EEWA players had no choice variance, compared to about 15% for ACT-R EEWA players. Although the EEWA model was able to fit both the average effort and variance of humans rather well (right in Figure 38), the large portion of EEWA “players” with no variance suggests these fits to human behavior are artificial. Therefore, the EEWA model is eliminated from the model comparison as well. In the following section, the PSS and ACT-R EEWA models are compared in greater detail.

**PSS and ACT-R EEWA**

With the exception of average variance, both the PSS and ACT-R EEWA models are able to capture human behavior relatively well and do not appear to do so artificially. In this section, the models are compared in greater detail to evaluate which model is better able to capture human behavior in the MEG.
The PSS model was not penalized for its additional complexity (i.e., the parameters within the ACT-R architecture and additional added features like player types) in these comparisons should be considered in the context of model fitting. The PSS model had a better fit for both average effort, $r(38) = .4, RMSE = .27$, and variance, $r(38) = -.13, RMSE = 1.6$, compared to ACT-R EEWA for effort, $r(38) = .46, RMSE = .42$, and variance, $r(38) = -.19, RMSE = 1.78$ (left in Figure 41).

Next, the relationship between group effort and variance was explored (right in Figure 41). There was a statistically non-significant, but practically significant negative relationship between average effort and variance for the human data, $r(16) = -.38, p = .12$. However, this negative relationship was significant for the PSS model, $r(98) = -.48, p < .0001$, and the ACT-R EEWA, $r(98) = -.29, p = .003$. Compared to the human data and PSS model, the ACT-R EEWA groups tended to be more tightly packed around the mid-point of effort and within an average variance of 2. There is a more detailed explanation for this pattern of behavior later in this section.
Figure 42. Average payoff (left) and distance from the minimum (right) across 20 rounds for humans, PSS model, and ACT-R EEWA model.

To further compare PSS and ACT-R EEWA models, average payoff and distance from the minimum are explored. For average payoff (left in Figure 42), ACT-R EEWA, $r(38) = .26, RMSE = 8.87$ had a slightly better fit to the human data than the PSS model, $r(38) = .34, RMSE = 10.4$. However, for average distance from the minimum (right in Figure 42), the PSS model $r(38) = .65, RMSE = 0.5$, had a better fit than ACT-R EEWA, $r(38) = .59, RMSE = .58$. So far, the PSS model appears to be better able to capture average behavior and average group behavior, but not by much. Next, the group dynamics of both models are compared to that of humans.

Figure 43. Average effort for eight groups of humans (left), PSS models (middle), and ACT-R EEWA models (right).
As discussed, the group dynamics of humans involves some dramatic shifts in average effort over time (left in Figure 43) and although the PSS groups have similar group dynamics (middle in Figure 43), they lack these dramatic shifts. The ACT-R EEWA (right in Figure 43) shows some variation in average effort of groups, but its rather flat compared to humans and PSS groups. For a more quantitative exploration of group dynamics, average player distance from the minimum and variance are compared as well (Figure 44).

![Figure 44](image_url)

*Figure 44. Scatterplots showing the relationship between average player distance from the minimum and variance for humans (left), PSS models (middle), and ACT-R EEWA models (right).*

The relationship between average player distance from the minimum and variance was weaker for the human data, \( r(70) = .32, p < .01 \), compared PSS, \( r(398) = .54, p < .0001 \), and ACT-R EEWA models, \( r(398) = .45, p < .0001 \). A one-way ANOVA for average player variance revealed a significant difference between humans, PSS, and ACT-R EEWA, \( F(2,869) = 130.1, p < .0001, \eta^2 = .23 \), and a Tukey test further revealed they all differed from each other \( p < .03 \). Average player variance was highest for humans \( (M = 2.78, SD = 1.87) \), followed by PSS \( (M = 1.01, SD = .85) \), and ACT-R EEWA models \( (M = .83, SD = .78) \). Furthermore, there were more players with no choice variance for the ACT-R EEWA model \( (15\%) \) compared to PSS \( (7.5\%) \) and humans \( (3\%) \).
A one-way ANOVA was also performed for average player distance from the minimum, which was significant, $F(2,869) = 14.33$, $p < .0001$, $\eta^2 = .23$. Average player distance from the minimum was highest for humans ($M = 1.32$, $SD = .96$), followed by PSS ($M = .92$, $SD = .65$), and ACT-R EEWA models ($M = .88$, $SD = .55$). A Tukey test revealed the models differed from the human data ($p < .0001$), but not from each other ($p > .05$).

The ACT-R EEWA model has lower average variance, more choice stickiness, and flatter average group effort. To better understand why, one ACT-R EEWA group is explored at the individual level by looking at each player’s choice and payoff (Figure 45), and choice rule utility profile (Figure 46).

![Figure 45. Effort (left) and payoff (right) for each player in one ACT-R EEWA group.](image)

The EEWA model considers the minimum and other player choices as potential minima. It only considers what players chose the previous round; therefore, it tends to “focus” on the average group choice. In the example group, ACT-R EEWA “players” choices vary between 2 and 7 for the first round followed by convergence toward 4, which becomes the average at the end of the fourth round (Figure 45). By round 12, there is pure coordination at 4, which is quite different from behavior of humans and the PSS
model. This type of behavior helps explain why the ACT-R EEWA model has rather flat average group effort, and player variance and distance from the minimum are low.

Due to the design of ACT-R EEWA, choice attractions for “players” are highest for initial choices and quickly change so that the average choice is most attractive. In Figure 46, choice attractions are shown for ACT-R EEWA model “players”. There is little variation in choice rule utilities (i.e., attractions). For “Players” 1 and 2, choice rule 4 becomes dominant by round 3. For “players” 3 and 4, they start with higher utilities for choices below 4, until around round 10, where the choice of 4 becomes dominant.

Figure 46. Choice rule utility profiles for all four players (from left to right).

These model comparisons reveal that overall, the PSS model is able to capture average behavior, average group behavior, and group dynamics of human behavior better
than the ACT-R EEWA model. In some cases, the ACT-R EEWA model performs nearly as well (i.e., average variance and distance from the minimum) and in one case better than the PSS model (i.e., average payoff). However, after a more detailed look at the ACT-R EEWA model group dynamics, it is clearer that its behavior is rather artificial. As mentioned, the PSS model was not penalized for additional complexity, which may have contributed to its better fit to the human data. One last thing to compare is correspondance to human cognition and game theory. The PSS model was developed to align with cognitive processes and includes features from game theory (i.e., player types, simple strategies, signaling, and beliefs about other players) and counterfactual thinking.

The player types in the PSS model come directly from van Huyck et al.’s (1990, 1991) conclusion that players start the game using payoff dominant and risk dominant strategies and begin to deviate from this strategy as they learn. The simple strategies used in the PSS are based on previous research indicating that more complex strategies are often aversive or costly (Beard & Beil, 1994; Duffy & Nagel, 1997; Ho et al., 1998; McKelvey & Palfrey, 1992; Nagel, 1995; Rubinstein, 1989; Schotter et al., 1994; van Huyck et al., 2002) and since it’s difficult to predict behavior of other players, simple strategies can be more efficient (Camerer, 2003; Haruvy & Stahl, 2007; Ho & Weigelt, 1996). A signaling strategy was included since players often attempt to signal in an attempt to influence other players and receive a higher payoff, however, since there is a cost to signaling (Thompson & Kaufman, 2010), it only continues if it is effective (Charness et al., 2018). In the PSS model, signaling behavior is possible and may continue until the cost (i.e., reduction in payoffs) outweighs the benefits (i.e., potential higher future payoffs). The PSS model uses player choice predictions because players are
strongly affected by other player’s choices (Brandts et al., 2014, 2015) and tend to use their counterpart’s previous behavior to predict what they might do in the future in order to best respond (Camerer, 2003). Lastly, the PSS model weights payoffs for unchosen strategies (i.e., counterfactuals) 75% as much as actual payoffs for correspondence with counterfactual thinking (Byrne, 2016; Kahneman & Miller, 1986) and the idea that it is treated differently than actual outcomes (Camerer & Ho, 1999).

The EEWA model does not have the previously described capabilities that correspond to cognitive processes in humans. The ACT-R EEWA does have improved cognitive plausibility and correspondence to human behavior (e.g., less sticky choices and better group dynamics) over the EEWA. However, in order to fit human behavior, the forgone payoff weight (i.e., delta) was set to vary between .98 and 1. This means that the payoffs for forgone choices are basically weighted the same as actual payoffs. However, if it was set lower, the model “players” would have even lower choice variance and coordination, which means that it would not be able to capture human behavior well.

In addition, the ACT-R EEWA engages in a significant amount of counterfactual thinking to approximate human behavior. After the actual payoff is received, the model considers the forgone payoff for all possible choices and each player’s choice as a potential minimum. This means the model calculates four sets of choice utilities (i.e., attractions) for each possible choice, for a total of 28 choice utility updates per round. For comparison, the PSS model updates the utility of all four strategies once per round including the chosen strategy. Lastly, ACT-R EEWA players are not capable of signaling and if a “player” chooses a high level of effort, it would be due to the first-round choice or a delayed reaction to one or more player choosing higher for a few rounds.
Discussion

The PSS model includes player types, simple strategies, player choice predictions, and an improved capability to signal. It is able to fit average behavior and average group behavior of humans in the MEG reasonably well. The model is also able to approximate group dynamics, but to a lesser degree. It is better at coordinating than humans, which results in a stronger decline in variance over time. Model comparisons show that the PSS model is more successful than competing models in capturing the overall behavior of humans and better corresponds to psychological processes and concepts from game theory and counterfactual thinking. The EWA, ACT-R EWA, and EEWA models appeared to have an artificial fit to human behavior due to the large number of players that had no choice variance (i.e., 40% or greater). Although the ACT-R EEWA was better than the other models, compared to the PSS model, it had a larger number of players with no choice variance (i.e., 15% compared to 7.5%), was not able to capture the human behavior as well, and lacked psychological correspondence to human behavior and previous literature. Although the PSS model was an improvement over the ACT-R EEWA, there are some opportunities for future work to further improve the model.

Future work with the PSS model could improve: 1) player choice predictions, 2) player types and strategies 3) sensitivity to the counterfactual manipulation, and 4) realistic signaling. The player choice predictions in the PSS model are rather basic. They use instances, specifically, the change from one round to another, to predict what players are going to do next. Choice predictions are based on instances, which do not vary much between “players” beyond the first round where the first generated instance includes starting choice predictions based on the first-round choice probability of humans. This is
because instances after the first round contain actual choices of all players. Ideally, each model “player” would include predictions about player types in combination with the player choice predictions already included here. Another potential method is including aspects of theory of mind (Baron-Cohen, Leslie, & Frith, 1985) into the PSS model, which has previously been implemented in ACT-R, specifically ACT-R embodied (Trafton et al., 2013). This could involve the capability to make inferences about what others are attending to, are aware of, and might do next. In the context of the MEG, these capabilities could be useful. However, most theory of mind models involve more interaction with humans rather than just interacting solely through choices during a game.

There were two player types and four strategies included in the PSS model. The two player types were described in the literature and the strategies were based loosely on the literature and comments by participants. PSS model fitting suggested there is a slightly higher frequency of payoff dominant player types (57%). However, there is likely more than just two player types and four strategies. Ideally, an experiment would be run to directly address player types and strategies to better inform this decision, rather than doing so arbitrarily. In addition, some existing datasets (e.g., Leng et al., 2018, van Huyck et al., 1990, 1991) could be used to infer strategies and player types using Bayesian inference to provide converging evidence.

The PSS model does not include the counterfactual manipulation. There is evidence that counterfactuals influenced humans, however, there is also evidence that few used them (35%) and used them consistently throughout the experiment (Hough, O’Neill, & Juvina, 2021). A counterfactual generation parameter on a continuum between 0 and 1 could be used to represent the presentation and use of counterfactuals. At zero the
“player” generates downward counterfactuals, in the middle it generates bidirectional counterfactuals, and at 1, it generates upward counterfactuals. These generated counterfactuals could simulate outcomes where the other players had chosen lower (downward) or higher (upward). To allow for change over time based on the context, the value of this parameter could be influenced by payoffs so that it can move along the continuum. For instance, a player in the downward condition would start out generating downward counterfactuals. If they were to earn higher payoffs than expected based on those counterfactuals, the value of the parameter would increase and the player would shift towards generating bidirectional counterfactuals. These counterfactuals could directly affect strategy utilities or generate “counterfactual” instances to influence player choice predictions. Adding such a parameter would increase complexity and have to be incorporated with strategies, player choice predictions, and instances. However, it is possible that strategies might not be necessary with the counterfactual generation parameter, as it can represent risk and payoff dominant behavior by considering if players were to choose lower or higher in subsequent rounds.

Lastly, the PSS model “players” have the capability to signal, however, it is very constrained. The counterfactual generation parameter could give rise to signaling. When at the high end of the continuum, a player would be more likely to signal or choose higher as it would be “optimistic” and believe other players will start choosing higher or in response to their behavior. Another alternative is formulating a cost/reward function that allows for individual differences in the value of costs and rewards associated with signaling or choosing higher than other players. This function could result in a shift to or away from signaling depending on the starting values and the group context.
V. General Discussion

Summary of Findings

There were four goals of this dissertation: 1) better understand coordination behavior in a simulated coordination situation, 2) assess if and how counterfactual nudges can improve coordination efficiency, 3) explore if and how individual traits, motivations, and signaling behavior affect group dynamics, and 4) develop a new model to approximate human behavior in the MEG and the corresponding psychological processes. Summaries for the two experiments and model development are presented below.

In the first experiment, there were four main findings. First, coordination was more efficient and stable compared to previous MEG experiments (e.g., van Huyck et al. 1990). These results were attributed to counterfactuals, as previous research with complete outcome information (Camerer & Ho, 1998; van Huyck et al., 1990) and small groups sizes (Leng et al., 2018; van Huyck et al. 1990) found little improvement of coordination efficiency and stability over base conditions (e.g., minimum only and larger groups of 16). Second, there was little to no evidence that cognitive effort avoidance and individual traits relate to coordination behavior. However, there was an unexpected task-order effect where participants who completed the Demand Selection Task (i.e., DST) first appeared to have greater difficulty coordinating (e.g., lower average effort and higher variance), compared to participants who completed the REMEG first. This finding also suggested that the real effort task did not increase coordination efficiency and if anything, may have reduced it. Third, there was no difference in coordination efficiency
between human and bot conditions, however, bot conditions had lower variance due to better coordination. Lastly, the EEWA model better fit the data compared to the EWA model that only considered the minimum. This provided some additional evidence that players consider what could have happened beyond changing their own choices.

The second experiment served as a follow up to explore and assess the potential effects of different types of counterfactuals by including upward, bidirectional, downward, and control (no counterfactuals) conditions. In all conditions, one human received the counterfactual manipulation and played the game with three EEWA bots (modified after first experiment) that did not receive the manipulation and had access to all player choices. There were three main findings. First, about two thirds of the participants failed the manipulation check, which suggested many were not paying attention or the manipulation was not clear (e.g., too much information). Second, despite only humans receiving the manipulation, which potentially weakened its effects, there were differences in effort between the upward and downward conditions. Lastly, there were differences between humans and bots for effort, payoffs, and signaling. Agents had higher effort and humans had higher payoffs. The differences in signaling were particularly interesting. Signaling is effective if it encourages other players to choose higher effort and results in higher payoffs over time. Players typically stop signaling once it becomes ineffective (e.g., Charness et al., 2018). However, more bots were classified as signalers (62%), they had lower payoffs, and there was no difference in effort compared to non-signalers. This suggests that the signaling-like behavior of bots was artificial and often maladaptive. For humans, there were less signalers (39%) and they had higher effort and payoffs than non-signalers. Overall, there was evidence that one human player
receiving upward counterfactuals or engaging in signaling behavior can nudge bot players in a group towards more efficient coordination.

The PSS model was developed to correspond to game theory, counterfactual thinking, and psychological processes of humans. It has player types, choice strategies, counterpart predictions, and some signaling capabilities. The model was able to capture average and group behavior of humans in the MEG better than competing models. All models struggled to capture elements of group dynamics. Three of the four competing models (i.e., EWA, ACT-R EWA, and EEWA) artificially fit average human behavior due to the large number of players without choice variance (40% or higher). The ACT-R EEWA had fewer no variance players (15%), but the PSS model had half that amount (7.5%), which was closer to amount observed in the human data (3%). Both the ACT-R EEWA and PSS model had better coordination than humans. However, the PSS model had more realistic group dynamics, better corresponded to the literature, and had more cognitive plausibility than the ACT-R EEWA.

In summary, the two experiments and modeling work addressed the goals of this dissertation. First, coordination behavior in the MEG involves individual predispositions and interactions between players within a group, however, group dynamics have more influence. Second, both experiments provided evidence that counterfactuals, specifically, upward can nudge players towards choosing higher effort and influence the group to do the same. Third, there was no evidence that effort avoidance or individual traits influence behavior after the first round, however, there was evidence that the signaling behavior of one player (i.e., human) can nudge other players (i.e., bots) and increase coordination efficiency. Therefore, it appears that both information and behavior can serve as nudges
for players. Lastly, the PSS model was able to fit human behavior at the average and average group levels, and to a lesser extent, the individual level and group dynamics over time. In addition, the model had clear correspondence to game theory, counterfactual thinking, and approximated psychological processes.

**Contributions**

This dissertation made several contributions to the game theory literature. Previous experiments primarily used average effort and minimum effort to make inferences about coordination, coordination efficiency, and signaling behavior. In this dissertation, I analyzed behavior at average, group, and individual levels and introduced several new methods to analyze behavior. Previous work used the minimum effort to measure coordination (e.g., Leng et al., 2018; van Huyck et al., 1990), which leaves out other player choices and does not assess whether the minimum was much lower than other player’s choices. In this dissertation, I used several metrics: average intergroup variance, the relationship between group effort and variance, and average distance from the minimum. However, the minimum is important and has been used to identify weak links for further analysis (e.g., Bortolotti et al., 2016). Here, I extended the weak link analysis by using distance from the minimum to rank players from weak links to strong links. This analysis revealed weak links were not consistently the weak links, but did influence other players particularly in the beginning of the game. This suggests that weak links players have a predisposition to choose lower effort and aligns with results from the first experiment where effort-related traits were more related to first round choices.

Signaling is often discussed in coordination (e.g., Brandts et al., 2014, 2015; Charness et al., 2018), but there is no clear method to measure or identify it in the MEG.
Leng et al. (2018) operationalized signaling as alternating between the minimum and higher effort. However, effective signaling involves paying a cost, other players need to be aware of that cost, and it needs to persist long enough to influence other players (Brandts et al., 2014, 2015). Alternating between the minimum and higher effort would not be effective and is not enough evidence to label a player as a signaler. Here, I used distance from the minimum to measure signaling and its extent, and identified signalers as those players who had distance from the minimum above zero for five consecutive rounds. This method is better aligned with the literature and those identified as signalers showed the expected pattern of effective signaling: short-term costs (i.e., lower payoffs by choosing higher than the minimum) followed by long-term benefits (i.e., higher payoffs as the minimum increases).

Previous research in coordination games has applied various methods for increasing coordination efficiency with varying degrees of cost, effort, and effectiveness (Cooper et al., 1990; Sahin, Eckel, & Komai, 2015; Weber, 2001; van Huyck et al., 1990; van Huyck, Gillette, & Battalio, 1992; van Huyck, Battalio, & Beil, 1993). In some cases, the intervention actually backfired if did not persist long enough (Brandts et al., 2014, 2015). Here, I used counterfactuals to give players information about how outcomes could have been different if they made different choices or the minimum was different. Research highlighted the lack of a coordination device or salient focal point as the cause of coordination failure (Blume et al., 1998; Mehta et al., 1994). The provided counterfactuals provided additional focal points and players knew that everyone in the group received them. This did not require changes to the game structure and did not relay on other players to communicate or help others. It reduced the effort required to consider
different outcomes, was tailored to each participant, and was provided consistently. Upward and downward counterfactuals influenced coordination efficiency, even when only one player in the group received them.

In this dissertation, a cognitive model of the MEG was developed within the ACT-R cognitive architecture. Prior to this dissertation, the mathematical EWA model was the only model that was used to explain and fit data for the MEG. It has some traits with some psychological correspondence and has the potential to fit data after estimating parameters. The PSS model included behavioral features described in the literature and the parameter estimations were cognitively plausible. The model can be inspected and evaluated at the process level, which is more informative about the nature of cognitive processes, strategies, and the interaction between different cognitive mechanisms. For instance, in the earlier discussion regarding Figure 35, a PSS model player switched strategies over time and this behavior was traced back to how the utility of that strategy changed over time due to learning and pregame preferences.

Limitations and Future Work

There is potential for future work to address limitations with measurements, the influence of counterfactuals, how player types and strategies evolve over time, and insight into how players react to counterparts. There were a lot of exploratory analyzes for the first experiment to assess coordination, coordination efficiency, and signaling. The second experiment extended these analyses to identify signalers. However, more work is needed to find and assess better assess methods of measurements for coordination related behaviors. Intergroup variance and the relationship between group effort and variance were used to assess coordination. However, variance is not normally distributed and
poses a potential problem for parametric statistical tests. Effort was the primary metric used in this dissertation to measure coordination efficiency. Realizing its limitation, I analyzed behavior at different levels of analysis: average, group, and individual to better measure coordination efficiency. It is still not clear which method is most appropriate to measure coordination efficiency.

The second experiment extended the counterfactual manipulation by including several conditions to assess how counterfactual direction influences coordination efficiency. However, using groups comprised of one human and artificial bots limits generalizability to groups of humans. Also, only the human received the counterfactual manipulation and bots received more information than humans in the control condition. The same bots were used in all conditions because there was no clear evidence about how counterfactuals influence behavior in the MEG and therefore, no clear guidance on how the bots should respond to them. A future experiment could address the effectiveness of the counterfactual manipulation and assess the degree that they are used when all players receive them and have access to the same information. Ideally, this experiment would use all human groups. I predict that the counterfactual manipulations would have a stronger effect on coordination behavior for human groups where all humans receive the manipulation.

Another issue was the manipulation check, it is not clear how many participants used counterfactuals as 70% failed the manipulation check and less than half indicated whether they used them. Some exploration is needed to assess the best way to present counterfactuals, how to properly assess if participants pay attention to them, and whether
participants used them. This would be valuable to assess in a pilot study prior to running an experiment with a counterfactual manipulation in the MEG or related game.

In addition to the counterfactual manipulation, beliefs about other players, player types, strategies, and group dynamics could be further explored. This could involve using the methods from the second experiment to gather process level data from the human participant to better understand player types, choice strategies, signaling, attention allocation, and how group dynamics influence behavioral changes. For instance, in addition to outcome measures, players could give verbal reports or short descriptions with a structured format in real time, and their attention could be tracked using either eye or mouse tracking. Using available literature and Bayesian inference, some of these implicit qualities could be inferred using these multiple sources of behavior. This process data and inferences could better inform and validate the PSS model.
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APPENDIX A

REMEG INSTRUCTIONS SCREENSHOTS

Scenario
You and the other players must work together to complete a goal. For instance, consider that you all work for a catering company. The goal could be to prepare food for a customer. In order to complete the goal and leave for the day, the group must prepare dinner and clean up the kitchen. Each person is working on a separate task (gathering materials, cooking an item, or cleaning) but no one can leave until everyone has completed their task. There is an allotted time to complete the goal, but everyone has the option of increasing their productivity to get their task finished earlier. Each person can increase productivity by allocating a number of units of effort, which determines how much time they want to cut off from the allotted work time.

How to Play
There will be multiple rounds in the game. During each round, you can choose a level of effort (work productivity) between 1 and 7. In the catering example, putting in more effort means working harder. To simulate the effort of working harder, you will need to solve a math problem. The higher the level of effort you choose, the harder the math problem becomes. In each round, you will receive a payoff, which depends on your amount of effort, the amount of effort of the group, and the degree of coordination (how well you work together). If you choose to put in more effort than everyone else and complete your task first, you still have to wait until all tasks are completed, which decreases your payoff. Due to this fact, the lowest level of effort by a member of the group (minimum effort) determines the payoffs for everyone.

Here is an example of a problem of effort level 1:
4 + 7
In this example you would enter in 11 to answer the problem correctly.

Here is an example of a problem of effort level 7:
9 + 1 + 2 + 3 + 2 + 1 + 9
In this example you would enter in 31 to answer the problem correctly.

Your performance bonus will be determined by the number of points you earn in the game. Incorrect solutions to the math problems will be deducted from your pay.

Example

<table>
<thead>
<tr>
<th>Minimum choice in group</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
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<td>50</td>
<td>70</td>
<td>90</td>
<td>110</td>
<td>130</td>
</tr>
</tbody>
</table>

The minimum effort in the group determines the payoff of all the group members.

Let's say you chose 4 as your level of effort and the other three group members chose 3, 5, and 7. The lowest effort level chosen (3) is the minimum effort choice in the group. Looking at the table below, your own choice is 4 and the minimum effort choice is 3, which would give you a payoff of 80 points.

Players:

<table>
<thead>
<tr>
<th>Level of effort</th>
<th>Payoff</th>
</tr>
</thead>
<tbody>
<tr>
<td>1 effort units</td>
<td>80 points</td>
</tr>
<tr>
<td>2 effort units</td>
<td>70 points</td>
</tr>
<tr>
<td>3 effort units</td>
<td>60 points</td>
</tr>
<tr>
<td>4 effort units</td>
<td>50 points</td>
</tr>
</tbody>
</table>

(You are highlighted in yellow, and the minimum effort is highlighted in red)
If you were to choose 1 and everyone else chose 7, you will receive 70 points and everyone else will receive 10 points.

Players:

<table>
<thead>
<tr>
<th>Level of effort</th>
<th>Payoff</th>
</tr>
</thead>
<tbody>
<tr>
<td>1 effort units</td>
<td>70 points</td>
</tr>
<tr>
<td>7 effort units</td>
<td>10 points</td>
</tr>
<tr>
<td>10 effort units</td>
<td>10 points</td>
</tr>
<tr>
<td>17 effort units</td>
<td>10 points</td>
</tr>
</tbody>
</table>

(You chose the minimum effort, so you are highlighted in orange.)

Potential payoffs are highest when everyone allocates higher levels of effort. For instance, if everyone chooses 7, each player makes 130 points.

Players:

<table>
<thead>
<tr>
<th>Level of effort</th>
<th>Payoff</th>
</tr>
</thead>
<tbody>
<tr>
<td>7 effort units</td>
<td>130 points</td>
</tr>
<tr>
<td>7 effort units</td>
<td>130 points</td>
</tr>
<tr>
<td>10 effort units</td>
<td>130 points</td>
</tr>
<tr>
<td>17 effort units</td>
<td>130 points</td>
</tr>
</tbody>
</table>

(You chose the minimum effort, so you are highlighted in orange. Other players who chose the minimum effort are highlighted in red.)
APPENDIX B

REMEG GAMEPLAY SCREENSHOTS - CHOICE OF 3 AND MINIMUM OF 1

Effort Selection and Arithmetic Problem

![Level Of Effort Table]

![Answer Math Problem Table]

Math Problem: \(8 \times 6 + 5 \times 6\)
Answer: 64
Results and Feedback

Math Problem Feedback

Time left to complete this page: 3:04

Minimum choice in group

<table>
<thead>
<tr>
<th></th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
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<tbody>
<tr>
<td>1</td>
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</tr>
<tr>
<td>Own choice</td>
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<td>50</td>
<td>70</td>
<td>90</td>
<td>110</td>
<td>130</td>
</tr>
</tbody>
</table>

Feedback:
Incorrect
The answer was 25.0
And you said 140

Results

<table>
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<tr>
<th>Minimum choice in group</th>
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<th>5</th>
<th>6</th>
<th>7</th>
</tr>
</thead>
<tbody>
<tr>
<td>Own choice</td>
<td>1</td>
<td>70</td>
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<td>50</td>
<td>70</td>
<td>90</td>
<td>110</td>
<td>130</td>
</tr>
</tbody>
</table>

Payoffs:

<table>
<thead>
<tr>
<th>Level of effort</th>
<th>Payoff</th>
</tr>
</thead>
<tbody>
<tr>
<td>1 effort units</td>
<td>60 points</td>
</tr>
<tr>
<td>2 effort units</td>
<td>70 points</td>
</tr>
<tr>
<td>3 effort units</td>
<td>10 points</td>
</tr>
<tr>
<td>4 effort units</td>
<td>50 points</td>
</tr>
</tbody>
</table>

(You are highlighted in yellow, and the minimum effort is highlighted in red)

Upward Counterfactual - Choice

If you had chosen 4, you would have earned a payoff of 50

<table>
<thead>
<tr>
<th>Minimum choice in group</th>
<th></th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
</tr>
</thead>
<tbody>
<tr>
<td>Own choice</td>
<td>1</td>
<td>70</td>
<td></td>
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<td></td>
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<td>90</td>
<td>110</td>
<td>130</td>
</tr>
</tbody>
</table>

Your actual score is the bold number outlined in a box

Upward Counterfactual - Minimum

If the minimum was 2, you would have earned a payoff of 70

<table>
<thead>
<tr>
<th>Minimum choice in group</th>
<th></th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
</tr>
</thead>
<tbody>
<tr>
<td>Own choice</td>
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</table>

Your actual score is the bold number outlined in a box

156
Downward Counterfactual - Choice

<table>
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<th>Minimum choice in group</th>
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<th>2</th>
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</tbody>
</table>

If you had chosen 2, you would have earned a payoff of 80.

Your actual score is the bold number outlined in a box.