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The Truth-Default, Diagnostic Utility, and the Value of Contextual Knowledge in Deceptive Interactions

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Abstract

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This dissertation describes and reports an experimental study examining the predictions of truth-default theory (TDT). TDT is an expansive, modular theory of deception detection which explains the processes by which people abandon the truth-default state and navigate judgments of message veracity in conversations. The first goal of this study was to test TDT's seventh proposition, which explains how communicators abandon the truth-default state through trigger events (i.e., actions which prompt communicators to be uncertain about the accuracy of the message) such as decreased sender believability and message coherence. The second goal of this study was to test the predictions of TDT's ninth hypothesis, which explains how messages are judged as deceptive through the same triggers that prompt the abandonment of the truth-default state. Finally, this study tests TDT's twelfth proposition, which claims that questioning which produces diagnostic utility will ultimately result in higher judgment accuracy than relying on perceptions of sender believability or message coherence when judging the veracity of messages. The proposed study sampled 361 participants from Amazon's Mechanical Turk (MTurk), who watched a sequence of two videos and completed an anonymous online survey. None of the study hypotheses concerning TDT's seventh, ninth, and twelfth propositions were supported, but the findings from this dissertation did contribute to the deception literature with regard to the prevalence of the truth-default and truth bias. Theoretical and practical implications of the results are discussed, as well as a review of the limitations influencing the interpretation of this dissertation's findings.

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CHAPTER I: REVIEW OF LITERATURE

Introduction

For close to seventy years, scholars have been making advancements in the field of deceptive communication. Scholars of deception have made different claims of what deception is and/or how it functions. Ekman and Friesen (1969) said there were certain nonverbal behaviors which were indicative of deceit and false information. Zuckerman and his colleagues (1981) argued forty years ago that the research on lying and the detection of lying were synonymous. Buller and Burgoon (1994) claimed that deception was more than just a singular message, as it is a communicative process. Vrij (2007) implied that people may be better off *not* improving our ability to detect lies because of the ease which accompanies not scrutinizing information from others. Levine (2020) recounted how in every taped deceptive interaction, the clues for who is deceiving and who is telling the truth are almost always present if one looks closely. Though each of these claims is diverse and represents an individual perspective of deception, they contain at least one commonality. Deception is pervasive, and humans want to understand the functions of deception and how it is detected. In that vein, what follows is a study which augments and extends others that have attempted to understand deceptive discourse. Using the logic of truth-default theory (TDT), this study examines the mechanisms between the abandonment of the truth-default and the ability to accurately detect deception.

This study is important to the literature of deceptive human interaction for two primary reasons. First, this study is a test of the causal claims of TDT, and Levine argues that this should be approached via the theory's modules. In other words, TDT is a

modular theory which predicts and explains the deception process in four distinct ways. The first module of TDT explains the truth-default and how it is primarily formed through the skewed distribution of rates of lying in the general public. The second module of TDT predicts how motives underlie the act of deception and how the motives of deceivers may be projected to receivers of deceptive messages. The third module of TDT explains how the truth-default is abandoned through a set of triggers, how the abandonment of the truth-default gives rise to suspicion, and how receivers then may judge messages as deceptive. The fourth and final module of TDT predicts how message veracity may be most accurately judged through confessions or comparing deceiver communication with relevant, corresponding knowledge. The proposed study will focus on a test of the third and fourth modules of TDT.

The third and fourth modules of TDT contain propositions about the abandonment of the truth-default, subsequent judgments of deception, and how deception detection accuracy increases through accurate contextual information about the interaction. Testing the causal mechanisms of the truth-default and its abandonment will advance the research of Clare and Levine (2019) which documented the presence of the truth-default. This study will extend their research through testing the nature of the truth-default in tandem with predictions of its abandonment. Evidence leading up to TDT's propositions has only been provided in pieces, therefore creating the modules of TDT which are woven together by the propositions. This study seeks to test these connections, specifically those propositions which are the foundation of TDT's predictions on the abandonment of the truth-default and subsequent active judgments of deceit (i.e., propositions 7 and 9). Finally, this study will test TDT's claim regarding how deception is most accurately

detected (i.e., proposition 13).

Another argument for the importance of this study is its practical value. Documenting the causal nature of the truth-default has important practical implications for applied deception detection. Levine, Clare, et al. (2014) showed that expert interviewers were able to achieve substantial deception detection accuracies when allowed to question the senders, indicating that professionals in law enforcement have access to an accurate strategy to deception detection. This study will add to Levine, Clare, and others' work by testing the causal predictions of the thirteenth proposition of TDT through passive observation of the prompting of diagnostic utility (i.e., viewing interviews where experts prompt diagnostic utility; Levine, 2007-2011) and comparing its effect on detection accuracy with the other triggers of deception detection, such as message coherence and sender believability. In a larger sense, this study will address how individual observations of cues compare with the presence of diagnostic utility as tools of deception detection.

Aside from law enforcement, there are other contexts which benefit from understanding the working mechanisms behind abandoning the truth-default and active judgments of deceit. For example, people in close relationships stand to benefit from understanding how the truth-default affects their perceptions of veracity for relationship talk or conflict, people consuming political communication benefit by understanding veracity judgments of political messages may not be made because of the presence of the truth-default, and health professionals benefit by understanding the usefulness of prompting diagnostic utility during interactions with patients who may be lying about behaviors that have negative health outcomes. In these contexts, and others,

communicators benefit from understanding how the truth-default is abandoned, what contributes to judging a message as deceptive, and how questioning can procure more accurate information, therefore reducing receivers' susceptibility to deceit.

Defining Deception

The definition of deception has evolved throughout the years, and deception scholars from psychology, sociology, and communication studies have all contributed to changes in the interpretation of what it means to deceive another. Zuckerman et al. (1981) concluded that deception was an intentional act to create a false belief in another, and as such, this definition was conditional upon the sender knowing the belief to be false. Deception was thought to consist of two parts wherein the deceiver would send false information while attempting to convince the receiver of her/his credibility. These two parts were a way of expressing deception's dual nature in which there was communication regarding the information (i.e., veracity of information) and metacommunication about the believability of the sender. The definition presented by Zuckerman et al. also rules out mistakes, self-deception, and performances (e.g., magicians) as deceptive behaviors, a qualification that is used by later deception scholars (e.g., Buller & Burgoon, 1994; Levine, 2014; McCornack et al., 2014; Vrij, 2007). The intentionality of deception has been used to distinguish truths from deception in all the other definitions covered in this section, indicating an agreement among deception scholars that sender intent is a necessary component of deception's conceptualization.

Vrij (2007) overviewed deception and defined it as an intentional act to create a false belief in another which the sender knows to be false, regardless of the success of the attempt. He proposed three separate aspects of deception. First, like scholars before him,

Vrij positioned deception as intentional, explaining how the average adult would find honest mistakes as non-deceptive and including the DePaulo et al. (2003) definition which cast deception as a deliberate act.

Another aspect of deception according to Vrij was that incorrect information did not define deception; rather, deception occurs when the deceiver intends to mislead the receiver, meaning that the communication of incorrect information in the absence of intent to mislead would be characterized as a mistake as opposed to deception. Finally, Vrij's third aspect of deception was that the receiver must not be forewarned of the deceit. Vrij points the reader to Ekman's (1985) analogy of the magician. According to Vrij's definition of deception, the magician never deceives during their show, as the audience members are aware that the magician will attempt to mislead them. In essence, Vrij argues that deceit only occurs when receivers of deceit do not expect or suspect misleading communication.

In the field of communication, scholars have addressed the definition of deception by framing it as a process. Buller and Burgoon (1994) argued for viewing deception objectively as a strategic behavior without consideration of its morality, citing that the study of deception had limited itself due to deception's often immoral label. Buller and Burgoon's perspective rests on the idea that deception and its detection are adaptive human behaviors which have been naturally selected over time as socially desirable skills. For their definition, Buller and Burgoon felt that previous definitions of deception were too narrow because they required a verbalization of a false belief. Instead, they argued deception can be enacted through ambiguity, omission, and nonverbal communication (e.g., false emotional expressions). Buller and Burgoon then define

deception as the intent to mislead through the manipulation of information, allowing for both verbal and nonverbal forms of deceit. Buller and Burgoon also go on to explain how the intent to deceive is not the only motive present within deceptive interactions, because various motives are often at play in any conversation that may lead one to deceive (and/or tell the truth). The alternative view of deception that Buller and Burgoon forward positions deception as a strategic, communicative act which is functional to the deceiver. Deception had been viewed as a morally reprehensible act by most scholars up until this point, which Buller and Burgoon point out weakened the scope of deception research.

Levine's (2019) view of deception is also process-based, but it contains a few distinctions from others' definitions of deception. Levine noted how most conceptualizations of deception fuel a false dichotomy of deception that may leave out certain types of deceit (e.g., lies of omission, equivocations, dodging the question). Studies of deception often compare absolutely truthful messages with what Levine refers to as "bald-face lies," or falsifications (p. 101, Levine, 2019). Bald-face lies come from the work of McCornack and others (2014) who sought to theorize about the different types of deceptive messages and their functions in discourse. Deception does not always include an outright falsification; rather, deception can occur through omissions, equivocations, half-truths, and other forms. Levine contends that deception's conceptualization should encompass all these types of deceit in the interests of inclusivity and accuracy, and because falsifications may not be the most common type of deceptive message. Thus, Levine's conceptualization of deception includes the intent to deceive, attempts to deceive (whether successful or unsuccessful), subjective perceptions of deception (i.e., individual perceptions of the veracity of messages), and the functions of

deceptive messages.

Levine's (2020) conceptualization is adopted for the purposes of this study. In his writings, deception is "intentionally, knowingly, or purposefully misleading another" (p. 102). Levine's definition is used here for two main reasons. First, the science and conceptualization of deception needs to encompass subjective perceptions of deceptive messages. TDT's predictions focus on veracity interpretations of receivers, and this study is exploring those predictions. Second, this study tests the causal claims of TDT which explore how deceptive attempts fail because of corresponding information and confessions (Levine, 2020). Levine explains how there are certain messages which are shared as if true but are functionally deceptive. For example, fake news sources are spread through social media by sharing. If a user shares the false information (unaware of its misinformation) by discussing the source with another, the misinformation is accomplishing its deceptive function, even if the user does not intend to mislead others. TDT predicts that corresponding knowledge of the receiver plays an influential role in detecting that deceit. Levine's definition is central to TDT, which is tested by this study.

The History of Deception Detection Accuracy

Most of deception research has attempted to expand what is known regarding how accurate individuals are at judging truthful and deceptive messages. A discussion of the history of deception detection research would be incomplete without reviewing nonverbal and verbal cues to deception. Scholars generally credit Ekman and Friesen (1969) with beginning the conversation on cues (or "clues" as the authors described them then) to deceptive communication. Primarily focused on the abilities of the face, hands, and feet to send information, Ekman and Friesen proposed that deceptive cues are attributed to

individual discrepancies in the ability to control communicative behaviors (e.g., speech patterns, adaptors, eye behavior, etc.). The increase in communication skill required to effectively deceive would then produce feedback for the receiver in the form of nonverbal behavior. The receiver would then rely on these nonverbal behaviors to distinguish deceivers from truth-tellers. Ekman and Friesen's work is so important to the deception literature because it spurred an overwhelming line of research which tested differences between deceivers and truth-tellers.

Meta-analysis is a useful tool for understanding trends associated with deception and its detection, and several meta-analyses have been conducted to find trends in cues to deception, deception detection accuracy, and individual differences in detection accuracy. Prior to the 1980s, there was not much consensus about how deceivers behaved in comparison to truth-tellers apart from a few perspectives and assumptions. Manuscripts ranging from 900 B. C. to the 1970s described deceivers as evasive, tense, and unable to control nonverbal behavior (Zuckerman et al., 1981), and with the arrival of meta-analytic tools for testing these assumptions, scholars discovered whether these trends were supported empirically.

One reason meta-analysis is useful is that it serves as a summary of scholarship and creates a historical account of a particular string of research (Bond & DePaulo, 2006). A final reason meta-analyses are a more effective way to describe trends in deception scholarship is because the use of quantitative evidence has shown to be more persuasive than specific examples or anecdotes (Allen & Priess, 1997). There are several studies of deception detection that, when examined in a vacuum, provide evidence to the effectiveness of nonverbal cues; however, meta-analysis provides a larger view of the

field as a whole, which can inform future researchers, practitioners, or consultants more holistically. Additionally, meta-analyses use effect sizes rather than statistical significance to make inferences. Although one or two individual studies may show a nonverbal cue to deception as statistically significant, it is meta-analysis which can confirm the effects of a given behavior (e.g., eye behavior, fidgeting, speech errors) on deception detection.

Cues to Deception

The meta-analysis of Zuckerman et al. (1981) is the earliest meta-analysis of deception detection accuracy research. It primarily focuses on the nonverbal differences between deceivers and truth-tellers, behaviors which lead to higher judgments of deceit, and accuracy means. The authors review the research on deception from three distinct areas. One, that from the perspective of the deceiver, deceit was arousing. When comparing nonverbal and verbal behaviors between deceivers and truth-tellers, they found that deceivers used more blinking, shrugs, and adaptors. Deceivers also committed more speech errors, had more speech hesitations, greater pitch, and gave more negative statements and irrelevant information. Zuckerman and his colleagues believed that the secret to increase deception detection accuracy was to use the differences in nonverbal behavior between deceivers and truth-tellers as cues, and so they devised a theoretical perspective from this meta-analysis: four-factor theory.

Four-factor theory proposed that detection of deception could occur through four distinct processes which deceivers experience during deceptive discourse. First, deceivers show their attempts to control their behavior by either trying too hard to successfully deceive or by leakage from attempting to control too many channels of communication at

the same time. Second, the act of deception produces arousal. Arousal processes mainly deal with physiological responses to the act of deception, and deceivers should leak more physiological cues because of their aroused state. Third, four-factor theory projects that deceivers will experience a plethora of emotions which may produce nonverbal reactions to these affective states. The affective responses predicted range from guilt or anxiety to Ekman's (1981) duping delight. Finally, the fourth factor Zuckerman et al. propose are the cognitive factors to deception. This proposal rests on the idea of cognitive load, which is argued to be higher for those producing deceptive messages. Although four-factor theory would predict that the differences between deceivers and truth-tellers would be observable by receivers of deceit, evidence of meaningful improvement in deception detection accuracy was scarce. Further, the meta-analysis conducted by Bond and DePaulo (2006) which will be discussed later provides further evidence against the usefulness of the four factors in increasing detection accuracy.

The second area Zuckerman et al. (1981) reviewed was how receivers of deceitful messages differed in their interpretation of verbal and nonverbal behaviors. From a nonverbal perspective, receivers of deception thought a message was more deceptive if the sender had more postural shifts (e.g., fidgeting, moving around), but also believed the sender more if gazes and smiles were maintained by the sender. Verbally, receivers interpreted longer response latency, more speech errors, more hesitations, and higher pitch to be deceptive, while faster speech rates were interpreted as less deceptive. The final area of review by Zuckerman et al. tested averages of deception detection accuracy. Their findings on detection accuracy were indicative of what deception scholarship on nonverbal cues to deception still find: nonverbal cues to deception result in increased

detection accuracy, which is significantly, but slightly, greater than chance.

When introducing nonverbal cues to deception, Zuckerman and his colleagues found that facial cues were detrimental to deception detection, body movements helped improve detection (although still not meaningfully), and the introduction of tone with the verbal message significantly improved accuracy. Zuckerman et al. conclude that deception detection is a possible skill to acquire should an individual be able to correctly interpret the cues in the correct channel and context. As aforementioned, Zuckerman et al. interpret their findings around what is now known as four-factor theory as a tool to produce better detectors of deception. DePaulo et al. (2003) elaborate further on the likelihood of distinguishing deceivers and truth-tellers through nonverbal behavior.

The DePaulo et al. (2003) meta-analysis is a crucial piece of evidence for the evaluation of the use of cues in deception detection. First, there are several pieces of evidence which suggest deceivers and truth-tellers *are* in fact different. On average, deceivers talked less than truth-tellers, gave less detail than truth-tellers, and pressed their lips more, what DePaulo et al. see as a nonverbal indicator of holding back information. Second, deceivers objectively appear less compelling than truth-tellers on a few fronts. Deceivers give fewer plausible statements with less logical structure in addition to giving the impression of more ambivalence than truth-tellers. Deceivers are also less engaging in their communication than truth-tellers. Deceivers use less verbal and vocal involvement in the conversation in addition to using less illustrators.

Across the board, deceivers are less verbally immediate than truth-tellers and specifically give the impression of having less vocal and verbal immediacy. Deceivers also give off an impression of vocal and verbal uncertainty more than truth-tellers do, but

there was some evidence that deceivers raise their chin more than truth-tellers during deceptive discourse, a possible piece of evidence for the duping delight (DePaulo et al., 2003). Third, deceivers were shown to be less pleasant to communicate with during an interaction. Deceivers were less cooperative overall, had less facial pleasantness, and gave more negative statements and complaints. Fourth, deceivers did show evidence of strain during deceptive interactions. Deceivers were more nervous and tense overall in comparison to truth-tellers; deceivers had more vocal tension, used higher vocal frequency, had greater pupil dilation, and fidgeted more than truth-tellers did. Finally, there was some evidence that deceivers were more effective at communicating as truth-tellers. Deceivers had less spontaneous corrections of communication and admitted less to lack of memory on a topic or event.

Deceivers also mentioned events or relationships outside of the conversation more than truth-tellers did. Aside from the evidence that show differences between deceivers and truth-tellers in their nonverbal behavior, there was evidence against cues as effective detection tools, beginning with the distribution of effect sizes reported by DePaulo et al. The median effect size for all cues was .10, far from a moderate or large effect; however, DePaulo et al. note that there were two cues with effect sizes over .50, enough for them to be considered as large effects. The two cues were overall verbal immediacy and overall perception of cooperation. Additionally, most effect sizes of cues to deception are small, with just a few cues as large effects. The stem and leaf plot reported by DePaulo et al. is akin to a power function, with a few exceptional differences in deceivers and truth-tellers while most are small. In sum, although there are significant differences in deceivers' and truth-tellers' nonverbal behaviors, there are not enough reliable nonverbal cues to

deception that serve to improve detection accuracy.

Although there is evidence which shows deceivers and truth-tellers differ in their nonverbal behaviors, there is as much (if not more) evidence to strike down the reliance on cues as a tool for accurate deception detection. Bond and DePaulo (2006) conducted an extensive meta-analysis on deception detection accuracy. The grand mean average was around 54%, which is a crucial blow to cues-based perspectives of deception detection, even though 54% accuracy is significantly (but only slightly) higher than chance. Bond and DePaulo's 54% accuracy is based on a few findings worthy of mention. First, the accuracy rates of cue-based deception detection were contextualized with the channel of the communication, or medium. Medium dealt with the source of the deceptive message receivers had to evaluate. The deceptive messages were a video of someone, an audio recording, or a video with sound (i.e., audiovisual). Results showed that participants who judged audio messages were more accurate than video messages, and audiovisual messages were more accurately judged than video messages. Participants who judged messages through video content only performed half a percentage point better than chance (50/50), while audio and audiovisual receivers performed three and four percentage points better than chance, respectively. Overall, the results of comparing message channel are evidence against nonverbal cues aside from vocalic behaviors, as participants who were only provided nonverbal kinesic or proxemic behaviors did not perform significantly better than chance.

Second, Bond and DePaulo tested how motivation influenced judgment accuracy. Overall, deceivers who were motivated to successfully deceive were more likely to be judged accurately than deceivers without prompted motivation, although the difference in

accuracy was only one-half of a percentage point. Bond and DePaulo predicted that the motivation to be believed may hinder a deceiver's ability to get away with deceit because their fear of getting discovered may relate to them using stereotypical deceptive behaviors, therefore prompting receivers to call their messages deceitful. DePaulo et al. (1985) called this the motivational impairment hypothesis and this prediction was validated by the finding that motivated deceivers were judged as deceitful more often. Third, receivers of deceptive messages where the deceiver had time to prepare were less accurate than when deceivers had to lie on the spot, showcasing the performative aspects of deception.

Fourth, familiarity with the deceiver improves accuracy, as those who were exposed to the deceiver prior to the deceptive interaction performed over three and a half percentage points better. The main explanation for the increase in accuracy for receivers familiar with the deceiver was that receivers became better equipped to detect deception when deceivers communicated truthfully with receivers. This "baseline" of truthful communication then resulted in increases in accuracy overall (Brandt, Miller, & Hocking, 1980; Feeley, deTurck, & Young, 1995). Essentially, receivers of deception were using discrepancies in truthful and deceptive behavior over several exposures to the deceiver as evidence for the presence of deception. These discrepancies significantly increase veracity judgment accuracy (over 70% in cases where receivers had four exposures or more to the deceiver). The findings on familiarity and deception detection are important, however, the differences could be explained by the veracity effect (Levine et al., 1999). Studies of familiarity have found that receivers who are more familiar with the deceiver also judge more messages as truthful (Bond & DePaulo, 2006; Feeley et al., 1995);

because most experimental designs with random assignment produce 50/50 truth to lie base rate, truth bias will increase accuracy for truthful answers and decrease accuracy for deceptive answers. This increase in accuracy is added to the fact that there are some deceivers who are not effective deceivers (Bond & DePaulo, 2008), therefore overall accuracy increases. In sum, familiarity improves deception detection accuracy significantly, yet this significant difference only results in four percentage points above chance.

Fifth, expertise provided no evidence of improving detection accuracy, as expert receivers only performed a little over one- and one-half percentage points better than non-experts. Although there were many significant differences among channel, motivation, familiarity, and time to prepare, Bond and DePaulo's study never found meaningful increases in detection accuracy. From a lens of practicality, a mean accuracy of 54% does not equate to expertise in deception detection. Bond and DePaulo note how the 54% average accuracy presents an "unwanted implication" that people are not proficient at deception detection, implying that proficiency in deception detection through cues may not be practical (p. 231). In sum, using cues to deception to discriminate between deceivers and truth-tellers has shown little to no evidence worth pursuing in the quest to meaningfully increase deception detection accuracy.

Questioning Deceivers

The failure of cues as an effective deception detection tool does not spell an end for increasing deception detection accuracy. Cues are no doubt an interesting approach to deception detection, but detection strategies can fruitfully rely on more than just simple cues (often studied in isolation). Deception is, after all, a communication phenomenon

that is interactive (Buller & Burgoon, 1994), and the ability for a receiver to respond to a deceptive message allows the receiver of deception a few strategic avenues. It is also clear that cues may be ingrained into assumptions about how deceivers behave. People hold assumptions about subjective cues to deception (e.g., gaze aversion, more fidgeting) that are inconsistent with more objective cues to deception that have been identified by empirical studies (e.g., higher pitch, greater pupil dilation; Ekman & Friesen, 1969; DePaulo et al., 2003; Stromwall, Granhag, & Hartwig, 2004). This is crucial in addressing whether the use of cues will ever yield detection accuracies over 54% because assumptions surrounding cues are likely to inhibit people from objectively examining a deceiver's message. Receivers need to pull their focus from subjective, stereotypical cues and turn it to other pieces of evidence that are more objective to detect deception. Really the only way to prompt more evidence from the deceiver is to encourage them to keep communicating, and this is why scholars of deception have turned to questions as a source of extracting evidence from the deceiver. If deception is a communicative process, then one natural way receivers can respond to deceptive messages is to ask questions. Some highlights of the scholarship on questioning begin with how scholars have tested approaches to deception detection used by professionals.

Vrij's work is an important start for understanding how questioning changes deception detection accuracy because of his work with law enforcement officials (Vrij & Granhag, 2007). Some of the findings from scholarship with law enforcement and deception detection are built upon the idea that questions provide an opportunity to increase the cognitive work performed by criminal suspects, who are likened to deceivers in this context. Vrij and Granhag argue that the findings from meta-analyses showed that

deceivers did not spontaneously emit cues to deception that have any value for detection accuracy because the relationships between verbal and nonverbal cues were small to nonexistent (DePaulo et al., 2003). Vrij and Granhag then turn to questioning and interviewing as an avenue to prompt reliable verbal and nonverbal cues. Iterations of Vrij's work included diverse questioning techniques used by members of law enforcement, which is an important start for understanding the improvements made to deception detection.

Overall, interviewing has shown mixed results for improving deception detection accuracy. One of the more popular interviewing techniques used by professionals throughout the United States is the Behavioral Analysis Interview, or the Reid technique (BAI; Inbau, Reid, Buckely, & Jayne, 2001). The BAI uses a set of questions to prompt nonverbal behavior which would distinguish between deceivers and truth-tellers, which Vrij and his colleagues (2006) tested to see if it would improve accuracy. The questions of the BAI are designed to prompt a diverse range of nonverbal behaviors, but Vrij et al. were hesitant to predict its success. They were correct to be hesitant. Higher scores on the BAI indicated that a participant was honest, and participants assigned to the deceptive condition produced a higher likelihood of telling the truth according to the BAI strategy. Unfortunately, even when attempting to prompt nonverbal behaviors that should distinguish deceivers from truth-tellers, interviewing techniques are not helpful.

However, interviewing can prompt more than just nonverbal behavior, of course, and later work using interviewing techniques turned to cognitive load as a distinguishing factor between deceivers and truth-tellers (Vrij et al., 2008). Cognitive load has often been a suggested difference between deceivers and truth-tellers, mainly with the

prediction that more cognitive ability is required of deceivers in order to deceive during conversation. Even IMT (McCornack et al., 2014) proposes that deceptive intent stems from a discrepancy in the cognitive ease between lying and telling the truth. In Vrij and his colleagues' (2008) study, they tasked deceivers and truth-tellers to tell their story in backward chronological order to increase cognitive load. They found that, under these conditions, deceivers differed significantly from truth-tellers in several nonverbal behaviors common in deception detection research such as lower speech rate, more leg movements, and more hesitations. Although Vrij and colleagues (2008) did not attempt to test whether interviewing actually increased deception detection accuracy, the findings from Bond and DePaulo's (2006) meta-analysis would suggest that the increase would likely only be slightly better than chance.

Questions Producing Diagnostic Utility

The problem with using interviews to prompt discrepancies between the behaviors of deceivers and truth-tellers is that nonverbal behavior, no matter how it is perceived or interpreted, is not a reliable means for increasing detection accuracy (with a few exceptions). To increase deception detection accuracy, receivers of deceitful messages need to utilize diagnostic utility, which is the ability of information to allow a person to form an inference based on evidence (Levine, Blair, & Clare, 2014). Receivers of deceptive communication should employ questioning strategies which provoke senders to provide diagnostically useful information.

There are several ways in which diagnostic utility can improve detection accuracy. First, diagnostic utility is about equipping receivers with contextual information about the interaction. Park et al. (2002) showed that people really detect lies

through contextual knowledge, confessions, and third-party warnings rather than the nonverbal behaviors of the deceiver. Contextual information and knowing the content within the context of communication has also been shown to increase detection performance (Blair et al., 2010). Across several studies, Blair and colleagues found that receivers who are provided with contextual information about an interaction are consistently and significantly more accurate deception detectors than receivers with no contextual information. For example, a receiver of deception may know the work schedule of her/his relational partner when the relational partner is attempting to deceive about the previous week's activities. Knowing the work schedule of the relational partner gives the receiver an advantage in noticing inconsistencies in the relational partner's story. Blair et al. conclude with the argument that knowing accurate content in the context of the interaction is a crucial part of detecting deception.

The findings presented in Blair et al. corroborate previous scholarship on familiarity and questioning in deception detection. Early scholarship found that familiarity harmed detection accuracy, but this impairment could be reversed by asking the deceiver questions which repeated earlier information and/or questions which were unexpected (Burgoon et al., 1994). In a sense, earlier scholarship on familiarity provides some evidence pointing to the importance of using questions to compare outside contextual knowledge to the information presented during deceptive discourse, given that both expert and amateur deception receivers who were familiar with the deceiver performed better when asking questions which were repetitive or unexpected. In sum, using diagnostic utility in a deceptive interaction occurs when a receiver of deceptive discourse is familiar with the content of the conversation (e.g., expert knowledge,

personal knowledge about the content of the deceit, possesses third-party evidence) and can prompt contextually useful information from the deceiver to compare to what is already known.

A second reason diagnostic utility can improve deception detection accuracy is that there is evidence that direct questioning can prompt accurate and contextual information with diagnostic utility. Levine, Shaw, and Shulman (2010) found that when questioning senders, direct questions which asked cheaters of a difficult trivia experiment to explain why they did so well, if they were lying, and what they expected their partner's response to be resulted in much higher judgment accuracy than questions which just inquire about background elements of the participant's general trivia/game experiences outside the context of the study (e.g., how often the participant played trivia, how well the participant performs in teams). The more effective questions directly asked if the sender was lying and what the sender's partner (someone with contextual information about the interaction) would say if the receiver asked them the same question. These questions primarily baited a confession or prompted contextual information (e.g., an explanation of how they knew the difficult trivia questions), which resulted in responses that were much shorter than those who responded to general background questions.

These results have a few important implications for diagnostic utility. Although knowing contextual information is important for increasing the detection of lies (Blair et al., 2010; Park et al., 2002), asking questions which prompt background information does not necessarily provide *accurate* contextual information that communicators can use to judge the veracity of messages. The increase in amount of talk time for background questions may have also overloaded receivers with too much information, while the

direct questioning instead prompted quick responses without soliciting explanations. Communicators who are highly competent may have increased their believability with their explanation of the background. Therefore, information with diagnostic utility does not only contain background information, and strategic questions are most effective when they create a divide in sender believability through prompting the sender to provide information which corresponds with or contradicts contextual knowledge or to confess (given that Levine, Shaw, & Shulman reported that the variances in sender believability between participants who used the background questions and those who used questions which prompted diagnostic utility were significantly different). Levine, Blair, and Clare (2014) explain that, when questioning deceivers, receivers should strategically withhold any relevant evidence they have until after the deceiver contradicts that evidence (i.e., SEU; Hartwig, Granhag, Stromwall, & Vrij, 2005; content in context; Blair et al., 2018). Practically, receivers should ask questions that lead to a discussion surrounding the key evidence they know and compare what deceivers say with that relevant knowledge.

To be clear, there is no general, ideal strategy that has been identified as effective in eliciting diagnostic utility across various types of deception (Levine, 2020). The underlying premise of diagnostic utility is that some information shared during communicative experiences has more usefulness than other information, depending on the context. It is in the best interest of the receiver of deceit to be well informed with relevant facts of the deceptive interaction in order to compare communication from the deceiver with what is true. In the case of Levine, Shaw and Shulman (and every other study on questioning and diagnostic utility), deceivers were participants who had cheated in an extremely difficult trivia game for money which they played with a confederate

assumed to be another participant. In this very specific type of interaction, questions which appeared to have diagnostic utility were those which asked participants how they knew the correct answers, whether or not they were telling the truth, or what would their partner say. Questions that did not elicit diagnostic utility included inquiries about the participant's history with trivia games, how well the participant has worked with teams, and whether teamwork played a role in the participant's success in the trivia game. In sum, direct questioning *can* be a proficient tool for deception detection if used to prompt information that leads to a correct inference of dishonesty, but not all direct questions have diagnostic utility (i.e., some questions have negative utility).

Diagnostic utility can be used to produce higher deception detection accuracies only with an understanding of which questions prompt contextually useful and accurate information. There are two studies which support the strength of prompting diagnostic utility. Levine, Blair, and Clare (2014) found that questions which prompted information with diagnostic utility would perform better than the meta-analysis average (54%) for a student sample, trained student sample, and expert sample. Each sample performed well, with over 70% accuracy rates. They also found that the questioning had a stronger effect on experts than it did for non-experts, indicating that experts perform particularly well when accurate contextual knowledge is prompted. They also found questions with negative utility to have a strong, negative impact on accuracy. Negative utility is when information is used to form an incorrect inference, and ill-conceived questioning can prompt information which harms detection accuracy by making honest people appear deceitful either through having a difficult time remembering details or making verbal mistakes (Levine, Blair, & Clare, 2014). Background questioning performs significantly

worse than questioning for information which corresponds with contextual knowledge of the conversation, so Levine, Blair, and Clare (2014) projected that the freedom to ask follow-up questions may increase accuracy further, a projection tested later (Levine, Clare, et al., 2014).

Similarly, Levine, Clare, et al. (2014) also provide strong evidence for questioning's effect on deception detection. In their study, a trained expert interrogator interviewed cheaters in a trivia experiment, which continued the trend of prompting diagnostic utility through expert questioning. Using the BAI, a trained interrogator persuaded four cheaters in a trivia game who lied about cheating to confess their lies. Past research would suggest that confessions are one of the primary ways deceptions is detected (Park et al., 2002), and Levine, Clare, et al. (2014) marked the expert interrogator as achieving 100% accuracy because of the prompted confessions. They then showed the tapes of the expert interrogations to undergraduate students (without showing the confessions). Simply by watching the video tapes, students averaged an accuracy rate of 79%, which is drastically higher than the average meta-analysis accuracy rate and higher than the accuracies reported in Levine, Blair, and Clare (2014). Studies on prompting diagnostic utility through strategic persuasion and direct questioning may have important implications for deception detection research.

The scholarship on diagnostic utility and expert questioning shows strong support for the use of questioning and persuasive tactics in increasing detection accuracy in laboratory settings, in the context of playing a game. The findings from such scholarship have two implications. First, these findings suggest that expertise in lie detection cannot solely rely on passive observation of behavior. An example of this is the comparison of

Vrij and colleagues (2007) and Levine, Clare, et al. (2014) in their respective use of the BAI. The passive observation of nonverbal behavior in response to the BAI questions resulted in inconsistencies between truth-tellers and deceivers, whereas Levine, Clare, et al. (2014) showed how powerful the persuasive questions of the BAI can be when implemented correctly by a trained expert interrogator rather than a police officer as Vrij and his colleagues did. Another example is in the question sets found in Levine, Shaw, and Shulman (2010) and Levine, Blair, and Clare (2014). Questions which relied on background information were more passive in their attempt to garner information than questions which prompted deceivers to divulge information with diagnostic utility, namely explanations for why cheaters performed so well in the trivia game.

Second, questions for diagnostic utility attempted to create a contradiction in the deceiver's messages through using relevant corresponding knowledge, resulting in the sender giving shorter responses. The shorter response times were likely because of the use of confessions as a form of deception detection, as the study relied on the argument that most lies in everyday life are detected through confessions, third-party warnings, and physical evidence rather than nonverbal behaviors (Park et al., 2002). Improving deception detection might be best accomplished through questioning for information that the receiver can compare with relevant contextual knowledge. TDT uses this logic to explain the social science of deception detection.

Truth-Default Theory

Truth-default theory (TDT) is a modular theory which provides an extensive explanation for the process of deceiving and detecting lies in human communication. The theory competes with the ideas of other deception scholars because of its critical

evaluation of past deception detection research. TDT has historically been tested with high stakes lies (e.g., cheating in a federally funded experiment, using the BAI to create confessions, using law enforcement experts to detect deception), and its applicability to white lies or everyday lies is not yet known. Thus, in keeping with TDT's approach, the focus of this project is on higher stakes lies. TDT contains four different modules which house the propositions of the theory. The theory's modules are considered "free-standing…effects, models, and mini-theories" (p. 5, Levine, 2014), while the propositions "provide a string of assertions, predictions, and conjectures that weave the constructs and modules together" (p. 97, Levine, 2019). As Levine asserts, the various modules, propositions, and hypotheses can be understood and tested independently, "without reference to the larger theory" (p. 96).

The propositions connect the modules in four main arenas. First, the prevalence of deception shapes the way humans have engaged with and behaved around deceptive behavior. TDT predicts that 1) most people are honest most of the time and 2) people believe most of the communication they receive most of the time, mainly because believing others is an adaptive and efficient form of communicating (Levine, 2014). Second, the lack of deceptive communication by most people suggests that, when people do lie, they have specific motives for deception. Therefore, these motives may be inadvertently transmitted to receivers of deceit, increasing suspicion and the likelihood of the message being judged as deceptive.

Third, the truth-default is a state of being where communicators do not scrutinize the information they receive. People need a reason to closely analyze information (e.g., motives, third-party warnings of deceit, tangible evidence), so people rarely exit the

truth-default. The abandonment of the truth-default is generally caused by triggers, which include the awareness (or suspicion) of a sender's motive, dishonest behavioral displays, a lack of logical consistency in the claims of the deceiver, or other prompts for suspicion like third-party warnings or a lack of correspondence between the deceiver's message and contextual knowledge (Levine, 2014). With enough evidence, receivers of deceit actively judge a message as deceptive, but TDT explains that deception detection need not occur during the interaction, as most deception is detected afterwards (Park et al., 2002).

Finally, TDT provides propositions about deception detection accuracy and how to improve accuracy. TDT predicts that most deception is not detected accurately through passive observation of cues because veracity is not related to honest sender demeanor. Rather, TDT proposes that deception detection is most accurate when either deceivers confess or when receivers of deceit compare information from a deceiver to contextual knowledge of the interaction. Therefore, higher detection accuracy occurs through prompting information with diagnostic utility through expert questioning which uses contextual information to ask the right questions (Levine, 2014).

Prevalence of Deception

The first module of TDT focuses on propositions one through four, which center on the prevalence of deception. The question of how common lying is has important implications for strategies and explanations of detection accuracy. Prior to the turn of the twenty-first century, there was no definitive evidence for how often people lied. DePaulo et al. (1996) sought to examine deception's prevalence and conducted a diary study. In their diary study, participants recorded daily social interactions which lasted longer than 10 minutes and/or any interaction in which they deceived the other person, regardless of

the duration of the deceptive interaction. The results were used as a baseline for average lies per day for some time. DePaulo and her colleagues found that people averaged between one and two lies per day and averaged lying in around 20-33% of interactions reported. Women reported more lies per day, yet men and women did not differ in their rate of lying. Differences in the traits and motives of deceivers and truth-tellers have also been examined. Kashy and DePaulo (1996) found that Machiavellianism and elements of impression management positively predicted the rate of lying, a finding which laid the groundwork for DePaulo's later theorizing of deceptive self-presentation. DePaulo and Kashy (1998) explored the two datasets from DePaulo et al. (1996) and Kashy and DePaulo (1996) again to observe the interpersonal nature of deceit. The authors predicted that the closer and more frequently interactants communicated, the less often they would lie to each other. This prediction was supported, as they found both closeness and frequency of interaction to be negatively related to lie rates.

TDT asserts that people are susceptible to occasional deception because of truth bias and the truth-default (Levine, 2014). Zuckerman et al. (1981) detected what they called "truthfulness bias" when they noticed that participants were much more accurate at judging truthful messages than they were at judging deceptive messages. It was thought that this increase in accuracy was likely because of a predisposition to judge messages as truthful. McCornack and Parks (1986) later tested the idea that relational partners would be biased in their interpretations of deceptive and truthful communication from a partner. Additionally, they expected truth bias to be a hindrance to accurate deception detection, and they were right. The more confident people were in the ability to detect deception, the more likely they believed the speaker. Truth bias was found to have a negative

relationship with detection accuracy, so TDT continues this conversation on truth bias and argues for a reframing of how deception scholars understand the deceptive interaction. While most deception research has focused on differences between deceivers and receivers in the deception process, TDT focuses on how people are deceived, explaining the working mechanisms of deceitful communication and its detection.

The prevalence of lying is a foundational element of the predictions of TDT and comprises the basis of the first four propositions of the theory. The first proposition predicts that communication is mostly honest (Levine, 2014). The infrequency of deception, according to TDT, is a key reason why people are vulnerable to deceit. The fact that people lie between one to two times per day may not seem important to the history of deception detection accuracy on the surface, yet the prevalence of deception is a foundational quibble for TDT. Serota et al. (2010) sought to pull back the curtain on the prevalence of lying, and their argument was that if the average person lied between one or two times a day, the research on deception and its detection needs to simply examine how each person lies because of the large amount of lies told. The average of one to two lies per day also suggests that people should be proficient at distinguishing between deceivers and truth-tellers, because understanding the act of lying should be improved with practice, given that everyone lies *and* it is an everyday occurrence (Serota et al., 2010). Unfortunately, this had not been the case, as meta-analyses averages hovered around 54% total accuracy, with deception accuracy worse than chance (Bond & DePaulo, 2006). Therefore, deception scholars needed to further examine the prevalence of deception beyond descriptive means.

Following this rationale, Serota and his colleagues conducted a national survey

which asked participants to report on how many lies they told in the last 24 hours. The results of this survey, a re-analysis of the student sample from DePaulo et al. (1996) showed that most of the reported lies were told by a *few prolific liars*. Serota et al. also doubted that average lies told were correctly describing the prevalence population, so they tested the distribution of the lies reported against a power function and found a steep, negative curve. This was further evidence that the average communicator is not lying one to two times per day, but rather there are a few prolific liars who drastically change the distribution of lies.

Serota and Levine (2014) later replicated and extended this finding in a sample from a large-scale survey in the United Kingdom. The study accomplished two major goals for Serota and Levine. First, the fledgling idea of prolific liars needed to be tested outside the borders of the United States to understand whether the phenomenon was somehow geographically bound (Levine, 2020). Second, the study provided a replication of the Serota et al. power function for prolific liars because the power function for UK and US liars was almost exactly the same. Levine (2020) reflects on the finding of prolific liars and its importance to the detection of deception:

> While I see some utility in thinking about prolific liars as a discrete type of person different from the majority of the population, who lie less frequently, I see deception and honesty as a person-in-situation phenomenon. For me, the question is not so much "Who lies?" as "When and why do people lie?" (p. 151)

What, then, is the importance of the skewed distribution of reported lies told to the study of deception detection accuracy? According to TDT, the distribution of lying is a direct explanation for why accuracy rates are never much higher than chance (Levine,

2014, 2019). Simply put, people are susceptible to occasional deception because they are so accustomed to interacting with truthful people.

Cultural, religious, and ideological beliefs tend to deter deceptive behavior, and it is more efficient and adaptive for communicators to believe information from others (Levine, 2014). Evidence for the prevalence modules has been present in deception scholarship for the better part of thirty years, as studies have consistently shown that receivers of deception are truth biased and receiver accuracy judging truthful messages tends to be slightly higher than chance (Bond & DePaulo, 2006). TDT proposes that this assumption is generally correct, as most people are honest most of the time. The second proposition of TDT therefore predicts that the distribution of lying is skewed mainly because of the presence of prolific liars who tell most of the lies.

The prevalence of lying certainly shapes human perception surrounding truthfulness and veracity, and TDT predicts that truth bias is a root cause of that. Within the modules of TDT, this is called the veracity effect. The veracity effect occurs when the judgment of truthful messages is more accurate than the judgment of deceptive messages. It occurs because people are generally truth biased, meaning that they believe others most of the time. This tendency to believe others then results in a higher accuracy in judging honest messages than deceptive ones. Given that the prevalence of deception is perhaps not as high as originally thought, it stands to reason that people are accustomed to receiving truthful messages.

The general theme of deception research before the turn of the century averaged accuracy rates across both deceptive and truthful messages. This led to an overall accuracy rate just greater than chance (Bond & DePaulo, 2006), yet this average accuracy

may be misleading, as is using averages in the prevalence of lying. Levine and his colleagues (1999) sought to unpack the accuracy of deception detection by testing how the veracity of the message changed overall detection accuracy. They found that people, regardless of relational context or suspicion, were much more accurate at judging truthful messages than lies. This is the veracity effect, and it is strong evidence for the influence of truth bias in deception detection. It was found that truth bias (usually measured as the ratio of messages judged as truthful compared to all messages received) had a strong positive relationship with the accuracy of truthful messages judged and a strong negative relationship with the accuracy of deceptive messages judged (Levine et al., 1999). This work was later extrapolated to predict a rate of accuracy, mainly because of the disparity between truth detection accuracy and deception detection accuracy. For years, scholars held fast to the idea that people averaged slightly better than chance in deception detection accuracy. Yet Levine et al. found that only truthful statements were judged at higher than chance accuracy, and deceptive messages were judged at less than chance accuracy.

The findings from this research led to a probability model which explains the slightly better than chance detection accuracy (Park & Levine, 2001). Overall accuracy is a function of three parts. First, the ratio of truths to lies shapes overall accuracy. Second, truth accuracy is high, so the more truthful messages encountered, the higher overall accuracy. Third, lie accuracy is below chance, and relates to lower overall accuracy. These three components contribute to the slightly better than chance overall accuracy found by meta-analyses (i.e., Bond & DePaulo, 2006; Zuckerman et al., 1981). These findings lead to the third proposition of TDT, which predicts that communicators will

believe most of the communication they encounter because of truth bias and the truth-default. If people are honest most of the time, truth bias and the truth-default increase the efficiency of communication because they prevent communicators from needlessly scrutinizing every piece of information to which they are exposed.

The third proposition of TDT leads to the description of the truth-default, which is a passive state which all people are in during typical conversations (Levine, 2014). It can be broken down into five distinct conceptual pieces (Levine, 2020). First, that people tend to be biased towards the truth. Levine (2020) discusses how the operational definition of truth bias is the ratio of messages judged as truthful over the total number of messages. Second, the truth-default state is the underlying cause of the truth bias (Levine, 2020). The evidence for the strength of the truth-default can be seen in the results of scholarship examining suspicion (Levine & McCornack, 1991; McCornack & Levine, 1990; Millar & Millar, 1997). Even under conditions of suspicion in which participants are forewarned of potential deception from their interaction partner, people are still truth biased. Interestingly, when participants were placed in high suspicion conditions, deception detection accuracy actually decreased in comparison to the moderate suspicion condition (McCornack & Levine, 1990). It is evident that the truth-default is there, so why has truth bias persisted when it leaves people open for deception?

TDT explains that truth bias is adaptive (Levine, 2014, 2019). TDT leans on the logic of its partner theory, IMT (McCornack, 1992; McCornack et al., 2014), by suggesting that most communication is accomplished as efficiently as possible. This logic is evident in how message production and listening differ given that scholarship has shown that the recognition of speech is processed much faster than the production of the

same speech (Griffin & Ferreira, 2006). Communicators simply place more of their cognitive skill into encoding than they do into decoding, leaving few resources for the scrutinization of information. It is also important to note that the cognitive resources used by encoders (i.e., speakers) are used efficiently because speakers on average produce many messages with minimal errors (Berger, 2010). Therefore, TDT would suggest that accepting messages as truthful is a way of reserving the resources required to scrutinize information to use them elsewhere.

The fourth proposition of TDT adds that the truth-default is generally justified, as the prevalence of lying is relatively low. The truth-default is considered to be a baseline state by TDT because believing information without going through a process of scrutinization increases efficiency by reducing cognitive load and speeding up the judgment process in addition to being correct most of the time. However, TDT's fourth proposition also adds that this increase in efficiency through the truth-default leaves people open for occasional deceptive discourse, explaining why most deception is not detected during the interaction (i.e., truth bias). In sum, the first module of TDT is grounded in the central idea that truth bias is deeply rooted within human communication practices through the lack of prevalence of deception (with the exception of a few prolific liars). In the next set of modules, TDT is focused on deception motives.

Motives for Deception

The second module and related propositions (i.e., propositions five and six) center on motives of deception. Though TDT is primarily about the receiver of deception, the theory does offer explanations about the behavior of deceivers. TDT explains that, aside from psychopathy, deception is functional, thus people who lie usually have a reason for

doing so. Aside from white lies, which can be pro-socially motivated (e.g., to encourage conversation or to help another feel competent; Camden, Motley, & Wilson, 1984), deception is discouraged in many social circles, so the decision to deceive must stem from a goal or purpose that outweighs the social consequences of deceit. Discovering motives to deception supports the next two propositions.

Motives are a crucial consideration of deception. Levine, Kim, and Hamel (2010) set out to understand the primary reason for lying. The principle of veracity states that there is a moral divide between the truth and deceit and is used as a baseline for explaining why individuals rarely deceive on average (Bok, 1999). Specifically, the truth is always morally preferable to deceit except for certain reasons (e.g., lying for personal safety or prosocial lies such as lying to improve another's self-esteem). This principle was tested through three experiments investigating whether communicators behaved in accordance with the principle based on motives (Levine, Kim, & Hamel, 2010). Findings from these experiments were informative for TDT and its fifth and sixth propositions. It was found that across many different scenarios, the absence of a motive to deceive produced a near 0% deception rate.

In other words, participants would not lie if there was no reason for it. When provided with a motive, people deceived more often than they told the truth. The third experiment (one which has been used in a plethora of experiments to create the logical argument for TDT) contained what Levine, Kim, and Hamel (2010) describe as unsanctioned lies. Participants were given an opportunity to cheat in a trivia game that awarded money. Like the other experiments, none of the participants who played the game fairly deceived about playing fairly; however, those who cheated during the game

deceived more often than they confessed to the cheating.

Levine, Kim, and Hamel's (2010) study leads to the underlying premise of the fifth proposition, which states that deception is purposive, and there is usually an underlying goal that motives the deception. In the case of the participants in the third experiment of Levine, Kim, and Hamel, their motive was to keep the money earned from their performance in the trivia game. Participants who cheated both told the truth and deceived. On one hand, cheaters are motivated to tell the truth so that they are not punished for cheating in a game for money. Conversely, they are motivated to lie to keep their earnings. For both the cheater and the non-cheater, the decision to lie or tell the truth stemmed from motives unique to the trivia game, and TDT's fifth proposition predicts that these motives are the driving force behind the decision to deceive.

Having established that there are motives to deception, TDT comes back to the receiver of deceptive messages to describe how motives are transmitted to interactants. From a practical viewpoint, the context of communication can provide interactants with a purpose to communication. According to TDT, deceptive motives can be projected in similar ways (e.g., avoid conflict, for competition, to save face). The fifth proposition also predicts that without any projected motive to deception, judgments of deceptive messages are generally not prompted during the interaction itself because receivers will stay within the truth-default state. The findings from research on motives for deceit provide the groundwork for the sixth proposition of TDT (Levine, Kim, & Blair, 2010).

Receivers of deception are not completely ignorant of the motives of deceivers, and the sixth proposition of TDT proposes that receivers of deception will be more suspicious and scrutinize information if they perceive the deceiver has a motive to

deceive. The work of Levine, Kim, and Blair (2010) is the foundation for the sixth proposition. They examined the motive for a false confession. False confessions are deceptive, yet their motive in most communication contexts is somewhat unclear (aside from extremely high stakes such as police questioning). For honest confessions, Levine, Kim, and Blair found that honest confessions to cheating in a trivia game were overwhelmingly believed more often than they are scrutinized. This overwhelming judgment of truth produces a wide gap between the accuracy of truthful confessions and false confessions. The gap was so wide that true confessions were virtually always judged correctly, while false confessions were virtually always judged incorrectly.

Confessions are relevant to the deception detection process as conceptualized by TDT, as Levine and his colleagues later considered confessions to be part of the operationalization of detected deception. Levine, Kim, and Blair contend that communicative motives which do not produce suspicion (e.g., confessing to an act like deception, which can be embarrassing or face-threatening) acts as a booster for the veracity effect because receivers are not perceiving a projected motive to deceive during a confession. These findings produced the projected motive model, which predicts that people generally know that deception is presumed to have a motive. If a motive is perceived by the receiver in the conversation, messages may be judged as deceptive. Absent an obvious motive to deceive, participants tend to believe the sender's message. In sum, motives play a large part in receivers' perceptions of deception because the awareness of a deceiver's motives is one of the first gateways to abandoning the truth-default. Ultimately, the truth-default is further dismantled by behaviors during and after the interaction.

The Abandonment of the Truth-Default

The third module, which contains propositions seven through ten, outlines the abandonment of the truth-default state and predicts the process by which people judge messages as deceptive. The abandonment of the truth-default is a complex process, one that individuals rarely go through in their communication. Clare and Levine (2019) recently documented the strength of the truth-default state. In two studies, they showed that when asked to give an open-ended response to a message, participants rarely evaluated the honesty of communication unless prompted to make a veracity judgment (i.e., were specifically instructed to judge the veracity of the message).

In Clare and Levine's study, it appears that it rarely occurred to people to consider the honesty of another's message unless they are explicitly asked to do so, supporting TDT's conceptualization of the truth-default, particularly in a laboratory setting where there is generally no relationship between the deceiver and the receiver. Of course, there are cases where the truth-default may be abandoned before an interaction begins such as talking with an unfaithful partner, a frequent deceiver, or a suspect in a crime. TDT predicts that, without a specific reason to abandon it, people generally begin interactions within the truth-default. Once prompted to actively consider the truthfulness of the message, participants in Clare and Levine's study described messages by the honesty, credibility, or sincerity of the sender significantly more than participants who were not prompted. These findings provide some evidence of the primacy of the truth-default; people generally assume communication is honest absent a motive for thinking otherwise.

Although the truth-default state seems to be fairly pervasive, it can be abandoned through a number of means. According to TDT, people abandon the truth-default because

of a series of triggers, which are identified in the seventh proposition of TDT. The seventh proposition of TDT proposes that the truth-default is maintained unless the receiver encounters a reason to abandon it, and the reasons include (but are not limited to): motives for deception, deceptive behavioral displays, and lack of coherence and correspondence in the communication (Levine, 2014). Once enough of these triggers are detected, the truth-default is abandoned – at least temporarily. Once the truth-default is abandoned, the efficiency of communication takes a backseat because the close analysis of information with an eye toward its veracity becomes paramount.

The triggers to abandoning the truth-default are not limited to those identified in the seventh proposition of TDT, but there are several triggers worth discussing when describing how the truth-default is abandoned (Levine, 2014, 2019). Behavioral displays that are associated with deception are one trigger. Assumptions regarding deceptive behavioral displays have some generalizability to the global population, as deception scholarship has found commonalities in deception and its detection among different cultures of the world. In 2006, the Global Deception Research Team conducted a study examining cultural myths about deceivers. What they found directly contributed to the conceptualization of how the truth-default is formed and abandoned (Levine, 2020).

In the Global Deception Research Team's study, 2,320 participants from 58 countries were asked how they can tell whether or not someone is lying. Over 60% of respondents indicated that the aversion of eye gaze was an indicator of deceit. More than one fifth of the sample also reported liars could be detected through nervousness, body movements (e.g., self-touching, shifting posture), facial expressions (i.e., the expression of emotion on the face), verbal incoherence, and verbal inconsistencies. The team

conducted a second similar study using the list of behaviors generated by the participants of the first study. In their second study, participants' assumptions about deceivers included that they used less eye contact, shifted posture more, used longer stories, stuttered more, and paused longer, among other behaviors (e.g., changes in speech rate, familiarity, fidgeting).

The findings from the Global Deception Research Team are important for understanding how unsuccessful people are at detecting deception. These common assumptions about how deceivers behave are cultural myths of deception, yet research has established that they have little value as reliable indicators of actual deception (Bond & DePaulo, 2006; DePaulo et al., 2003). As such, reliance on these myths of deception may at least partially explain why people are often unsuccessful at detecting deception – they are looking for cues or other indicators that are not reliably present when deceivers deceive. For TDT, the research conducted by the Global Deception Research Team is part of the reason why the truth-default is so powerful. Without a reason to scrutinize information (e.g., behavioral displays thought to be associated with deception), people will not exit the truth-default, therefore the veracity of communication is assumed. Additionally, the reasons associated with abandoning the truth-default are either not effective tools of deception detection or they are learned after the interaction is over.

Another way the research conducted by the Global Deception Research team has influenced TDT scholarship is through believability. Believability may in part be communicated by the lack of behavioral displays that are presumed to be deceptive, yet there is more to believability than these behavioral displays. According to TDT, believability influences perceptions of sender demeanor, which is an amalgam of

behaviors that contribute to how a person presents herself or himself and how they are in turn perceived by others. Believability is the ability to convey a perception of honesty regardless of objective honesty (Levine et al., 2011). Believability is powerful, mainly because it shapes how receivers perceive the veracity potential of deceivers in a few ways. First, believability can change the proportion of truthful judgments. Across American and Korean students, educators, and experts, communicators high in believability led to significantly higher judgments of truth by receivers, while communicators low in believability led to lie bias (i.e., when the amount of truthful judgments are fewer than actual honesty; Levine et al., 2011). Believability also significantly increased judgment accuracy when matched with message veracity, yet significantly lowered judgment accuracy when mismatched with message veracity in these same samples. Believability encompasses deceptive behavioral displays and plays an important part in judging the veracity of a deceiver.

In addition to consideration of the sender's demeanor in terms of deceptive behavioral displays and believability, TDT's seventh proposition includes the deceptive message's coherence and correspondence with known information as triggers to abandoning the truth-default. Essentially, if a deceiver's message contradicts itself or information already known by the receiver, the truth-default may be abandoned because the receiver perceives a decrease in the deceiver's message coherence. The coherence and correspondence criteria are foundational pieces to TDT's predictions about deceptive judgments and increasing deception detection (Blair et al., 2018). Historically, the coherence criterion has produced little improvements for deception detection accuracy but is still an important piece to abandoning the truth-default because according to TDT's

seventh proposition, it still contributes to the generation of suspicion through the abandonment of the truth-default (Levine, 2019).

Coherence involves the logical consistency of a deceiver's message (Blair et al., 2018). If, during an interaction, a deceiver contradicts her or his message, receivers may catch these inconsistencies and become suspicious. Blair and colleagues discuss how the coherence criterion has been widely used in deception detection and add TDT's perspective that what the receiver knows before the interaction may be more valuable than trying to catch a liar in a contradiction.

The overreliance on the coherence criterion in previous deception research led Blair and colleagues to consider how the possession of information pertinent to the deceptive interaction may increase detection accuracy more consistently. Objectively, the correspondence criterion was found to be proficient in separating deceivers from truth-tellers because deceivers gave fewer consistent details and more inconsistent details. These details were compared with an accurate depiction of a crime scene. Blair and colleagues then found that the possession of the accurate depictions of crime scenes significantly improved judgment accuracy. Participants who were equipped with accurate information were able to accurately compare the statements of deceivers and truth-tellers. Although the correspondence criterion appears to be a stronger tool for detection deception than coherence, TDT includes deceiver's message coherence as a trigger to the abandonment of the truth-default as well because coherence is still used as evidence to judge a sender as deceptive. According to TDT, both coherence and correspondence may be used as evidence to abandon the truth-default.

The eighth proposition of TDT predicts that receivers may temporarily abandon

the truth-default because of a combination of triggers which serve as evidence of deceit (Levine, 2020). Once the abandonment of the truth-default occurs, receivers are essentially in a state of increased suspicion; however, suspicion is not enough to conclude that a message is deceptive. In fact, there is evidence that higher levels of suspicion can actually impair the accuracy of deception detection, mainly because what once was an extremely high level of accuracy for truthful communication now varies more because receivers of truthful communication are engaging in more information-seeking and experiencing more doubts about the veracity of the communication (McCornack & Levine, 1990). Additionally, there is evidence that the abandonment of the truth-default does not necessarily lead to a judgment of deceit.

Suspicion has been shown to affect deception detection accuracy in several ways. Suspicion tends to cause people to overestimate the honesty of their acquaintances (i.e., familiarity with the deceiver may prevent a receiver from judging a message as deceptive although there is evidence of deceit) and underestimate the honesty of strangers (Burgoon et al., 1994). Suspicion also seems to be a hindrance to detection accuracy for experts of interrogation. One explanation for this is because of the interactive nature of suspicion. Deceivers often realize that receivers of deception are suspicious and will alter their behaviors to attempt to reduce receiver suspicion (Buller et al., 1991). Deceivers appear to realize a change when receivers abandon the truth-default and compensate accordingly to improve their deceptive performance. Finally, suspicion has led to increased truthful judgments by receivers when interacting with acquaintances. Showing suspicion during an interaction is accusatory, and if the deceiver perceives the suspicion in a receiver, he or she may adjust their communication to lead the receiver away from suspicious feelings

(Buller & Burgoon, 1996; Burgoon et al., 1994).

In its ninth proposition, TDT predicts that once out of the truth-default state, veracity judgments are made using the same triggers which led to abandoning the truth-default (Levine, 2014). These triggers can include the motives of the deceiver, the deceiver's demeanor, the coherence of the deceiver's message, information from the deceiver which does not correspond with relevant knowledge of the receiver, and third-party warnings of deceit.

Communicators who have abandoned the truth-default are in a heightened state of suspicion, and therefore uncertain regarding the honesty of the message received. Although the seventh and ninth propositions are similar, the key difference is the presence of the truth-default. When under the truth-default state, receivers are not actively assessing the veracity of information they encounter but can be triggered out of the truth-default state when "triggered" by information that hints at deceit (i.e., proposition 7). Once out of the truth-default state, communicators work to confirm or refute the suspicion generated by the abandonment of the truth-default, thus prompting assessments of veracity (i.e., proposition 8). These triggers may result in a reinstatement of the truth-default if there is evidence to believe the sender. However, if there is not enough evidence to believe the sender, the receiver makes a deception judgment.

TDT explains that the intensity of the triggers influences whether or not the receiver reverts back to the truth-default. For example, a deceiver may have a motive to deceive that is projected to the receiver, but if the deceiver has a sincere demeanor, presents a logically consistent message, and the receiver can confirm some of the deceiver's statement with relevant knowledge, an honesty judgment will likely be made

because the evidence for deception is not strong enough. In sum, TDT's ninth proposition explains the way in which deception judgment s occur, and the theory later uses this foundation to explain detection accuracy.

Expanding upon the practical use of triggers to deception detection, TDT's tenth proposition predicts that triggers to deception judgments do not have to present themselves during the interaction. Instead, TDT argues that many judgments of deceit occur after the interaction itself (Levine, 2014). Deception detection scholarship does not portray humans as skillful deception detectors. In fact, most of the logic surrounding the truth-default, truth bias, and the TDT would suggest people are susceptible to deception. One potential reason deception scholarship has shown such inability to detect deception is because the triggers which lead to deception judgments rarely occur during the interaction; rather, deception is usually detected after the fact (Park et al., 2002). Most of deception detection research using nonverbal cues suggests that deception detection is a skill which can be utilized during deceptive scenarios (e.g., criminal interviews), yet participants have reported finding out about another's deception through third-party information, physical evidence, or confession (Park et al., 2002). Third-party information included confessions to third parties or third parties giving contextual information which led to the discovery of deception.

Examples of physical evidence include smelling cigarette smoke on the breath of a claimed quitter or examining text messages of a significant other who claimed s/he was not cheating. In only around two percent of cases did participants report the nonverbal behavior of the deceiver as a cause for deception detection. Although TDT's tenth proposition proposes that most deception detection occurs after the fact, the next module

of TDT proposes how deception can indeed be detected during the interaction.

Detection Accuracy and Expert Questioning

The fourth and final module along with propositions eleven through fourteen are predictions and explanations regarding accurate (and inaccurate) deception detection. Taking into consideration the prevalence of deception, motives for deception, the truth-default, and its abandonment, TDT's eleventh proposition predicts that deception is not usually accurately detected through passive observation of the deceiver's demeanor during the interaction. Nonverbal cues of deception and honesty are not mutually exclusive to deception or honesty (Levine, 2014), so nonverbal cues often do not provide information with diagnostic utility, and therefore only produce slightly better than chance accuracy (Bond & DePaulo, 2006). Much of the research using TDT rests on the argument that humans are not encountering lies as much as originally thought, so the fact that participants consistently are slightly better than chance at detecting deception is somewhat confounding. How can individuals who rarely encounter deception have a detection accuracy above chance? According to TDT, the prevalence of truth bias allows for greater accuracy of judging truthful communication, but scholarship documenting *a few transparent liars* proposes that average detection accuracy can be attributed to a few individuals who are unskilled and unsuccessful deceivers (Levine, 2010; 2014).

TDT's prediction of a *few transparent liars* begins with the work of Bond and DePaulo (2008), who tested the individual differences between receivers of lies and truths and found negligible differences, so they in turn tested the individual differences between senders of lies and truths. They concluded that deceivers differed more in credibility than they did in detectability, indicating that senders differ not so much in the

transparency of their veracity (e.g., how effective a deceiver is a getting away with a lie) as they do in their appearance of honesty (e.g., the general believability of a deceiver). Given that there is no relationship between honest appearances and actual honesty, Levine (2010) hypothesized that this difference in liar detectability and credibility could be attributed to why deception detection accuracy is slightly better than chance, a hypothesis which worked its way into the modules and logic of TDT.

The basic idea of a few transparent liars is like a one hundred question true/false test (Levine, 2010; 2019). If ninety of the questions are extremely hard (representing most of the population in terms of sender credibility), test takers must guess, resulting in a 50% chance of getting the answer right; thus, half of the hard questions are answered correctly on average. The other ten questions are extremely easy (representing the small portion of the population who are transparent liars), where everyone answers them correctly. Combining the correct answers of these two groups results in 55 of the 100 true/false questions answered correctly, or 55%. This percentage is the same as the average deception detection accuracy rate as documented in Bond and DePaulo (2006), and Levine believes that the ten easy questions represent some population of transparent liars, a trend hinted at in Bond and DePaulo (2008). This is the underlying logic of the eleventh proposition of TDT, which predicts that – aside from these transparent liars – deception is not accurately detected during an interaction using cues (Levine, 2014). Proposition eleven explains that communication which appears honest or deceitful is independent of actual message veracity. Because of this, proposition eleven predicts that deception detection based on sender demeanor is only slightly better than chance because of transparent liars and the increased accuracy of truthful judgments from truth bias.

Levine et al. (2011) presented evidence from eight studies which provide the basis for the eleventh proposition of TDT. Results showed that the demeanor of the sender significantly influenced detection accuracy and believability of senders. Accuracy levels reached the 70-100% accuracy range through manipulating the sincerity of the communicator. Essentially, the believability of the communicator could predict the veracity judgment. Taken together, the results of these experiments align with TDT's eleventh proposition. Although nonverbal cues to deception exist, aside from a few transparent liars, sender demeanor and actual honesty remain unrelated. Also, consistent with Bond and DePaulo's (2008) findings, sender demeanor varies widely, and this variance is what contributes to the explanation of deception detection accuracy rather than the skillful interpretation of receivers. Levine et al. (2011) contend that cues-based deception detection has a hard cap on accuracy because cues to deception and honesty remain unrelated for the most part.

The final three propositions of TDT delve into the factors that are likely to improve deception detection accuracy. TDT's twelfth proposition explains that deception is most accurately discovered through the deceiver's confession or by using contextual information to compare with external knowledge. Previous scholarship supported the idea that people rarely reported detecting lies because of another's nonverbal behavior (Park et al., 2002). Instead, people primarily detected lies through confession, physical evidence, or third-party warnings. Evidence of this has been shown through deception scholarship which focused on providing receivers with contextual information about communication (Blair et al., 2010).

Even though traditional deception detection research has found slightly better than

chance accuracy, participants who were given contextual information about the deceptive interaction were consistently more accurate in deception detection. Levine (2020) makes the point that, in retrospect, using contextual information to detect lies seems at times impractical or may not be possible because there are so many cases where information pertinent to the deceptive interaction may not be available, but content in context has shown to be an invaluable asset in deception detection.

Blair et al. (2010) explain that although communicators will not *always* have relevant knowledge when targeted with deceit, communicators will have some relevant knowledge in *most* communication experiences, making content in context an important piece of the deception detection process. Blair et al. (2018) found that corresponding facts were readily perceived and used to make veracity judgments across several experiments. Deceivers – especially skilled ones – do not always contradict themselves, so the authors make the argument that the correspondence criterion is more useful in deception detection, as it relies on how informed the receiver of deceit is about the context of the communication.

TDT finishes its predictions with propositions about how receivers of deceit may go about gaining contextual knowledge using diagnostic utility. TDT is a theory of communication and interaction, and real-world deceptive encounters do not always end with the receiver of deception simply judging a message that they have received but to which they did not respond. The thirteenth proposition of TDT proposes that questioning is the most effective way to elicit either confessions or information with diagnostic utility; however, the questioning must be context-sensitive (i.e., questions must prompt responses which contribute to the receiver's formation of an inference).

Consistent with the eleventh proposition, questioning which manipulates the believability of the deceiver is most effective at producing higher accuracies (Levine, Shaw, & Shulman, 2010). Results indicate that when interviewers ask questions which prompt the deceiver to disclose information the interviewer possesses relevant knowledge of *or* confess to deceiving (e.g., "Are you telling the truth?" or "What would your partner say if I asked them the same question?"), judgment accuracy increases substantially, even under truth bias. The expert questioning research eventually led Levine, Blair, and Clare (2014) to develop the idea of diagnostic utility, a variable which would be a foundational part of how TDT predicts an increase in detection accuracy (Levine, 2014).

Diagnostic utility is essentially a continuum of how likely it is that information can be used to make a correct inference, specifically about the veracity of a sender's message (Levine, Blair, & Clare, 2014). The continuum of diagnostic utility is anchored on one end by negative utility. According to TDT, questions with negative utility make truth-tellers appear more deceptive than deceivers appear. Questions which provoke negative utility are more difficult to respond to for truth-tellers than deceivers (e.g., "Why should I believe you?"). One explanation for why questions like these have negative utility is because those who tell the truth are not expecting a receiver to become suspicious or skeptical, whereas deceivers can prepare for the suspicious reactions of a receiver (Levine, 2020).

Questions can also produce no utility (e.g., background questions about a deceiver), which relates to the slightly better than chance deception detection accuracy primarily because questions with no utility often attempt to produce cues. Cues are not reliable tools for deception detection (e.g., focusing on the wrong cues, believability's

lack of relationship with actual honesty), so questions aimed at provoking deceptive cues will not produce enough relevant information to correctly form an inference. Finally, questions can prompt diagnostic utility (e.g., prompting cheaters in a trivia game to explain how they knew the answers to the difficult trivia questions), which greatly increases detection accuracy. Levine, Blair, and Clare (2014) argue that diagnostic utility equips receivers of deceit with the accurate contextual information which is most often used to detect deception.

The fourteenth and final proposition of TDT predicts that expertise in deception detection requires strategically prompting diagnostically useful information through questioning deceivers (Levine, 2014). Across the several studies conducted by Levine and his colleagues on diagnostic utility, there are several points worth noting regarding expertise in deception detection. Levine, Blair, and Clare (2014) were able to show incremental increases in detection accuracy between background questioning of a deceiver (i.e., questions which inquired about a deceiver's previous experiences regarding the context of the laboratory experiment) and context specific questioning about the deceiver's performance in a trivia game. The questions which produced the greatest detection accuracy focused on prompting the deceiver to explain why s/he did well in the trivia game, what the deceiver did during the opportunity to cheat (i.e., when the experimenter left the room with the answers still in front of the participant), whether the deceiver discussed cheating with her or his partner (a confederate in the study), and whether the deceiver thought a cheater should be punished.

Levine, Clare, et al. (2014) applied the concept of expert questioning quite literally and invited an expert interrogator of the Reid technique to interview thirty-three

cheaters and non-cheaters of the trivia game. The questions used for each interview were also constructed through the Reid technique, so their questions differed from those in Levine, Blair, and Clare (2014). Some of the major differences include that the expert began by asking about background information of the participant (e.g., academic major, hometown), indicating that expert questioning is an interactional, interpersonal process which may organically start with background questions before proceeding to the strategic use of context-sensitive questions.

The expert also asked the participants to recount which questions they got right or wrong, then followed up by asking participants to explain *how* they had the answer on the correct ones. The questions from Levine, Clare, et al. also contained a bait question in which the interviewer left the room after asking what the participant thought their trivia partner would say when asked if they cheated. Once the expert returned, he claimed he knew that the participant cheated and just wanted the participant to be "up front with them" (Levine, 2019). The largest difference between the two question sets of Levine, Blair, and Clare and the expert's questions occurs when the interviewer returned to the room.

Using strategy from the Reid technique to induce confessions, the expert accuses the participant of cheating and pressures them to explain why they cheated. For the purpose of discovering the cheaters from the trivia game, the questions used by the expert interrogator are certainly effective at eliciting confessions, as all four cheaters of the trivia game subsequently confessed in addition to the expert correctly classifying the non-cheaters. In addition to the expert judging the veracity of trivia participants, the recordings of the expert interviews were shown to undergraduate students, where

detection accuracy was almost 80% by passively observing the interviews.

Levine (2019) details two experiments funded by the FBI with expert interrogators from a government law enforcement agency using the trivia game. In these experiments, there were 89 participants in the trivia game, and five experts were able to accurately classify 98% of the participants overall as deceivers or truth-tellers. Using recordings of these five expert interviews, around thirty undergraduate students in a deception course were able to accurately classify 94% of the cheaters and non-cheaters. Levine discusses common themes surrounding the questioning used by the experts in these two experiments. Similarly to the expert in Levine, Clare, et al. (2014), experts here started with basic background questioning. Experts also checked the coherence of participants' statements by asking questions with a specific focus on inconsistencies in participant responses. Levine describes how the experts would focus on inconsistencies, yet they would not presume deception occurred until the participant was given an opportunity to explain discrepancies. Levine points out how these focused questions gave experts more information to form holistic inferences about the veracity of the participant's responses. Levine also recounts how experts used questions with relevant themes to provoke confessions from the participant. For example, if a participant considered themselves an honest person, the expert would use that theme throughout the interview to try and hold the participant accountable to that identity.

A Test of the Truth-Default and Veracity Judgments

The goal of this study is to test the predictions of two of the modules of TDT. As outlined above, the modules and propositions of TDT are grouped into four major sections which describe the deception process: how the prevalence of deception shapes

the truth-default, deceptive motives, the abandonment of the truth-default, and expert questioning through diagnostic utility. For the proposed study, TDT will first be tested by examining predictions regarding the abandonment of the truth-default. TDT proposes that the truth-default is a state of suspended bias toward believing the communication of others. According to the TDT, individual perceptions of the triggers to the abandonment of the truth-default should impact whether or not observers describe sender messages with mentions of veracity or deceit. If individuals spontaneously mention the veracity or deceptiveness of the messages without being prompted to make any judgments regarding honesty, then according to the theory, the truth-default state has been abandoned. Informed by the logic of the seventh proposition of TDT, the first hypothesis is proposed.

> H1: (a) Decreased sender believability and (b) decreased message coherence increase the likelihood of observers describing sender messages with mentions of veracity or deceit, which is indicative of having abandoned the truth-default state.

TDT's predictions regarding the truth-default have been supported by previous research. Clare and Levine (2019) showed how participants rarely reported any thoughts related to veracity when asked to write reactions to plausible and implausible messages, indicating that the truth-default functioned to prevent participants from even considering the veracity of the message. This study seeks to extend previous work on the abandonment of the truth-default by assessing two specific triggers predicted by TDT: message coherence and sender believability.

Based on available evidence, TDT proposes in its seventh proposition that there are various triggers which may result in abandonment of the truth-default. Two triggers that are fundamental to deceptive judgments include perceptions of the logical

consistency of the deceiver's message (Blair et al., 2018) and the believability of the deceiver (Levine et al., 2011). TDT predicts that the logical consistency, or coherence, of communication is a trigger of abandoning the truth-default (Levine, 2014). The logical consistency of a deceiver's message is a pivotal part of seeming believable. The coherence criterion has been tested most often with repeated statements by criminals which law enforcement officials use to compare stories (Granhag & Stromwall, 2000; Steller & Koehnken, 1989; Stromwall et al., 2003), and TDT predicts that logically inconsistent statements can trigger the abandonment of the truth-default. Blair et al. (2018) contend that the coherence criterion can produce suspicion in an individual, resulting in abandoning the truth-default.

Previous research on sender believability under the perspective of TDT has shown that although the honesty of a message is independent of the believability of the sender, honesty *judgments* are significantly related to sender believability (Bond & DePaulo, 2008; Levine, 2010; Levine, 2016). Specifically, judgments of message veracity can be affected by manipulating sender believability and transparency. Senders who are able to enact cues related to credibility and believability are often judged as honest as a result of those cues, even if their message is deceptive. Additionally, there are transparent liars (Levine, 2010) whose messages are easier to accurately judge. Senders low in believability are more likely to trigger the observer's abandonment of the truth-default.

Another goal of this study is to test the ninth proposition of TDT regarding deception judgments. The ninth proposition of TDT regarding the abandonment of the truth-default proposes that once in a state of suspicion, observers of deceit use the same triggers which caused them to abandon the truth-default as evidence to judge message

veracity. Under the logic of TDT, prompting participants to judge the veracity of a person's message effectively forces them to abandon the truth-default, subsequently increasing suspicion (Clare & Levine, 2019). The logic of TDT shifts after the abandonment of the truth-default to focus on the judgment of deception.

TDT is clear that the judgment of deceit is preceded by the triggers which are perceived, and although TDT proposes that the same triggers to abandon the truth-default are used in veracity judgments, this does not mean the triggers to veracity judgment are *accurate.* Rather, TDT explains a causal process of how individuals actively judge another as deceptive. The ninth proposition of TDT proposes that, using a variety of information sources which include the triggers listed so far (i.e., message coherence and sender believability), a message may be judged as deceptive. TDT explains that once out of the truth-default, individuals may revert back to the truth-default state either because of evidence of honesty (e.g., clarifying previous messages, displaying higher believability) or not enough evidence to make a deception judgment. Primarily, the ninth proposition provides a foundation for a process by which deception judgments are formed, and based on the logic of the ninth proposition of TDT, the next hypothesis is proposed.

> H2: When observers are equipped with contextualized communication content, exposure to deceptive messages (as compared to honest messages) will cause them to perceive senders as (a) less believable and (b) less coherent.

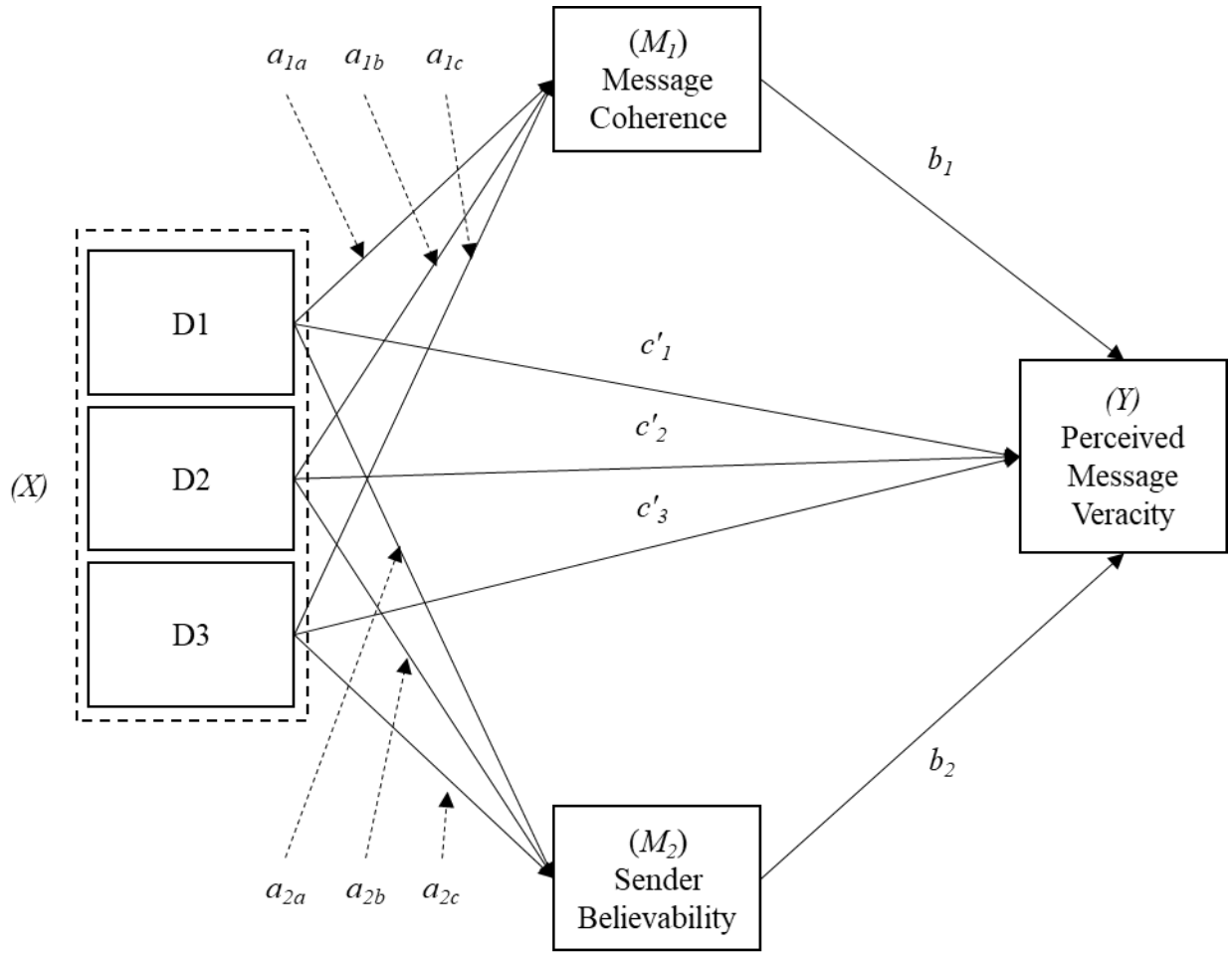
There is potential evidence of sender believability and message coherence acting as mediators between the relationship of message honesty and veracity judgments according to the ninth proposition of TDT, although no study to date has actually tested

these hypothesized relationships. Levine et al. (2011) found that when sender believability was consistent with actual honesty, judgment accuracy significantly increased for observers. Mismatched believability and honesty, however (e.g., when sender believability was high but the message was actually deceptive), depleted judgment accuracy significantly below chance. This indicated that higher sender believability was being judged as honest while lower sender believability was being judged as deceptive, regardless of the actual honesty of the sender's message. Further, message coherence has been the foundation for the use of repeated statements in police investigations (Granhag & Stromwall, 2000) and interrogation techniques like the BAI (Inbau et al., 2001), which strategically attempt to cause the suspect to communicate contradictions in order to procure a confession. According to TDT, messages will elicit a decrease in message veracity judgments through the perception of less sender believability and message coherence when the observer possesses contextualized communication content. Thus, using the logic of the ninth proposition, the third hypothesis is proposed.

> H3: When observers are equipped with contextualized communication content, exposure to deceptive messages (in comparison with honest messages) will result in lower perceptions of (a) sender believability and (b) message coherence, which, in turn, negatively predict perceptions of message veracity.

Figure 1

Conceptual model of hypotheses 2 and 3



Note: Indicator coding represents comparisons between conditions to the referent group (honest non-cheater/background questioning). D1 = deceptive cheater/background questioning condition, D2 = honest non-cheater/diagnostic utility questioning condition, D3 = deceptive cheater/diagnostic utility questioning condition. Any covariates that emerge during preliminary analyses as significantly impacting message veracity (e.g., sex, lie acceptability, general communicative suspicion) will be included in final model.

The third and final goal of this study is to test the twelfth proposition of TDT which predicts how to best improve deception detection accuracy. Although TDT predicts that sender believability and the logical consistencies of communication trigger the abandonment of the truth-default, ultimately TDT claims that deception is not accurately judged by reliance on sender believability. Rather, observers must rely on

confessions or contextualized communication content to maximize accuracy (Levine, 2020). TDT considers information which yields diagnostic utility as the best tool for improving accurate deception detection. TDT scholarship has found that obtaining information with diagnostic utility is best accomplished through expert questioning of cheaters and non-cheaters in a trivia experiment (Levine, Clare, et al., 2014). Even when interviews were observed by undergraduate students, observers were able to accurately judge messages at a much higher rate than the 54% average documented in Bond and DePaulo (2006). Given the current evidence of diagnostic utility's value in increasing judgment accuracy, this study seeks to replicate previous research by testing differences in third-party observation of background questioning versus diagnostic utility questioning.

In addition to diagnostic utility, sender believability is also likely to increase judgment accuracy, but this relationship should not be much more than chance (Bond & DePaulo, 2006). Additionally, the fact that sender believability has been shown to have no relationship with objective honesty may impair observers in their ability to accurately classify messages because most nonverbal cues to deception are weak tools for meaningfully increasing classification accuracy (Bond & DePaulo, 2006; Levine, 2010; Levine, 2019). Additionally, although message coherence is a tool for judging a message as deceptive, it is likely to increase judgment accuracy only slightly because of a few senders who are not effective deceivers (i.e., transparent liars) (Blair et al., 2018).

According to TDT, the use of diagnostic utility should increase the odds of a person accurately judging the veracity of a message above and beyond sender believability and message coherence. Specifically, proposition twelve of TDT proposes

that deception is most accurately detected through comparing contextualized communication content with the information provided by the deceiver (Levine, 2020), and Levine, Blair, and Clare (2014) have shown that diagnostic utility can improve the accuracy of third-party observers as well. In their study, interviews of cheaters and non-cheaters of a trivia experiment were shown to a class of undergraduate students. By observing interviewers who used questions which prompted deceivers in the trivia experiment to explain how they knew the difficult answers, student observers were able to achieve detection accuracies over 70%. Similarly, Levine, Clare, et al. (2014) found that third-party observation of expert questioning would result in higher accuracies for non-experts. In their study, undergraduate students watched recorded interviews of honest and deceitful trivia experiment participants that were conducted by an expert interrogator. Through passive observation of the expert questioning, the students were able to achieve an average accuracy of just under 80%.

Diagnostic utility should increase the odds of accurately judging a message even more than the other triggers which relate to the abandonment of the truth-default, even when the diagnostically useful information is prompted by someone else. Using the logic of TDT's twelfth proposition, a fourth hypothesis predicts that when evaluators are equipped with contextualized communication content, the passive observation of prompting information with diagnostic utility will increase accuracy above and beyond message coherence or sender believability.

> H4: When observers are equipped with contextualized communication content, exposure to interviewing which prompts diagnostic utility (in comparison with exposure to interviewing comprised of background questioning) will predict

higher judgment accuracy after controlling for sender believability and message coherence.

Summary

In this chapter, a case was made to test modules three and four of truth-default theory. The chapter first reviewed a brief history of deception detection research which included a discussion of nonverbal cues, the weakness of sender believability as a tool for deception detection, and an avenue to improving deception detection through the use of strategic questioning for diagnostic utility. Next, a full review of TDT's four modules and fourteen propositions was provided. Finally, a rationale for testing propositions seven, nine, and twelve of TDT was presented, culminating with four distinct hypotheses. These hypotheses include predictions regarding how individuals abandon the truth-default when triggers such as sender believability and message coherence are present. hypotheses two and three predict how observers of deceptive messages who are provided with relevant, contextual information will have lower perceptions of sender believability and message coherence than observers of truthful messages, therefore resulting in lower perceptions of message veracity. The final hypothesis predicts that after controlling for sender believability and message coherence, observers will have a higher judgment accuracy when exposed to questioning which uses diagnostic utility than when exposed to questioning which only uses background information.

CHAPTER II: METHODOLOGY

Participants and Recruitment

Participants for the study were recruited online using Amazon's Mechanical Turk (MTurk). Using MTurk has several advantages for the purpose of the current research, and the choice to use MTurk for participant recruitment was made with theoretical contributions top of mind as the propositions of TDT include generalizations about most people in most contexts (Levine, 2020). Hara et al. (2018) found that MTurk had over 2,600 unique participants completing research surveys. Given Hara and colleagues' history of securing diverse samples, MTurk appears to be a strong sampling arena for generalizable findings.

MTurk offers several methodological advantages as well, including improved access and cost for conducting social science research for online recruitment (Mason & Suri, 2011). For access, MTurk has a pre-existing group of participants from whom data may be collected. In addition, MTurk is recognized as providing socioeconomic and ethnically diverse samples (Casler, Bickel, & Hackett, 2013). Further, MTurk participants also have a diverse range of ages from 18 to 80 years old, with Mason and Suri (2011) reporting a median age of 30 (M_{age} = 32).

The cost of recruitment is also lower for researchers than when conducting laboratory experiments (Mason & Suri, 2011). Although compensating participants usually costs around minimum wage, compensating MTurk users costs significantly less because of ease of access and completion for the participant in comparison to the scheduling process of laboratory experiments (Mason & Suri, 2011). For this study, participant recruitment was expected to cost somewhere between 25 cents and 3 dollars

per participant (Follmer, Sperling, & Suen, 2017), and payment was set at $1.75 to recruit a maximum of 400 participants.

The only inclusion criteria for participation in this study was that participants are 18 years of age or older. Participants were recruited to the study via a short statement describing the general study procedures without mentioning that the study is about deceptive messaging or deception detection (i.e., watching two very brief videos about trivia participants and completing questionnaires after each). The statement provided the estimated time for completion and pay rate for completion. An anonymous Qualtrics link was used for participants to access the study, which provided a consent form detailing the study without specifying that the study is about deception so as not to prematurely prompt participants out of the truth-default state.

Participants

A total of 361 participants completed the study. The reported age of participants ranged from 19 to 70 years old (M_{age} = 36.00). The majority of participants (64.3%) were male and 35.2% of participants were female, and one participant identified as transgender. A majority of participants identified as white (57.6%), with 18.3% identifying as Asian, 16.6% Black, 4.4% Hispanic, and 1.9% Middle Eastern.

The majority of participants reported originating from the United States (81.9%), with the next highest national origin as India (9.5%). Other countries of origin for participants include Canada, UK, Philippines, Italy, South Korea, Nigeria, Cuba, Nepal, Egypt, the EUA, and Bangladesh. The vast majority of participants (83.7%) identified as US Citizens, while just under 9% identified as Indian citizens. Overall, 89% of participants resided within the United States.

All participants had some form of formal education. The majority of participants reported a bachelor's degree as their highest education level (62.0%), while 26.5% of participants reported earning a Master's degree, 5.8% of participants reported a high school diploma as their highest degree earned, 3.3% reported an Associate's degree and 0.8% reported earning a Doctorate.

Stimuli and Study Procedures

Participants in this study watched two different videos from Levine's (2007-2011) NSF-funded trivia experiment interviews, which constituted the study stimuli. In these experiments, individuals were recruited to play a highly difficult trivia game for a cash reward. The difficulty of the questions in the game was such that it was extremely unlikely that an individual would accurately answer all the questions because they ranged across a diverse pool of subjects (e.g., biology, chemistry, etc.). During the trivia game, there was an administrator who gave the quiz as well as a confederate who posed as another naïve trivia contestant. In the middle of the trivia game, the administrator leaves the room, intentionally leaving behind the answers to the trivia game on a table in front of the contestants. At this point, the confederate ensured that the contestant was aware that the answers to the trivia game were on the table in order to provide a potential way to cheat the trivia game.

After completing the trivia game, contestants were interviewed on camera by another research project associate who was not the administrator of the game. During the interviews, cheaters (i.e., participants who looked at the answers left on the table) and non-cheaters (i.e., participants who were made aware of the answers left on the table but refused to look at them) were asked if they cheated during the game when the

administrator left the room. Levine (2019) describes participants who cheat in the trivia game and subsequently lie about cheating as unsanctioned liars because at no point are the interviewees instructed to cheat during the game, only made aware of the option to do so by the confederate. Each of these recorded interviews contain one of two question sets (i.e., background questioning or targeted diagnostic utility questioning). Based on which questioning set was employed, the duration of the interviews averaged 77.75 seconds for background questioning and 147.50 seconds for diagnostic utility questioning.

This study proceeded in two phases in order to test the theoretical predictions of TDT in keeping with the proposed flow of the process of deception detection judgments. The first goal of this study was to test the predictions of TDT regarding the abandonment of the truth-default, while the subsequent goal of this study was to test how TDT explains the process of deceptive judgment. Therefore, after obtaining informed consent, participants were shown two videos. The first video and the questionnaire that followed it were intended to test hypothesis 1, and constituted the first phase of the study. After this phase, participants were given contextual information about the trivia game, including the opportunity participants had to cheat, and informed that trivia participants were possibly lying about cheating. Providing this information ensured that participants in this study abandoned the truth-default state and equipped them with the relevant contextual information that is necessary to effectively test the remainder of the study's hypotheses (hypotheses 2-4).

Phase I

To test the first hypothesis of this study, participants were randomly assigned to a video of either a truthful non-cheater or a deceptive cheater (given the hypothesis, the

interviewer's questioning type is not relevant in Phase I of the study but will come into play during Phase II, described below). For Phase I, the interviewee in both videos was female. Prior to watching the first video, participants were given no information regarding the trivia experiment or the interviewee in the video, nor were they given any warning of potential deceit. Instead, participants were informed that they would watch a trivia game participant answering questions about their trivia experience. The interviewer's questions (i.e., background questioning or diagnostic utility questioning, as described previously) were cut from the video so as not to prime judgments of veracity or deceit (e.g., questions such as "why should I believe you?"). After watching the first video, participants were instructed to provide their general reactions to the video to assess the presence or the absence of the truth-default state. The instructions were adapted from Clare and Levine's (2019) study on documenting the truth-default which asked participants to give their general thoughts on the interviewee's responses as a whole instead of responses to specific questions, and appear as follows:

> Now that you have watched the interview, what are your thoughts about the interviewee's answers? Please write down anything that comes to mind as you consider the answer(s) that the interviewee gave. Your thoughts don't need to necessarily be coherent, cogent, or complete. Your thoughts don't even need to be relevant to the interviewee's actual answer. Simply write down what you were thinking. Just write down your thoughts as they come to you.

The average word count per participant was 12.68 words. After writing down their thoughts regarding the first video, participants completed measures of their perceptions of the interviewee's believability (referred to as sender believability, below) and message

coherence, described below, and then proceeded to Phase II of the study.

Phase II

The second phase of the experiment began by briefing participants on pertinent contextual information about the trivia experiment. Providing the participants with details of the trivia experiment ensures that they have access to relevant, context-specific knowledge regarding the second interview they watched without prompting any triggers to potential deceit or cheating, which is a prerequisite for testing hypotheses two through four of this study. As part of the briefing, participants were told that the interviewee was a contestant in a federally funded experiment, the interviewee could have earned money based on how well they performed in the trivia game, the questions in the trivia game were extremely difficult to answer, the administrator of the trivia experiment left the room in the middle of the trivia game, and the "partner" the interviewee was working with was really a research team member who was posing as another naïve contestant in the federally funded experiment.

After this briefing to provide relevant contextual knowledge, participants were randomly assigned in a 2 (honest non-cheater/deceptive cheater) by 2 (diagnostic utility questioning/background questioning) experimental design. The second video participants viewed represented one of these four conditions. One group viewed an interview that features background questioning of an honest non-cheater, another group viewed an interview that shows diagnostic utility questioning of an honest non-cheater, a third group viewed an interview that displays background questioning of a deceptive cheater, and a final group viewed an interview that features diagnostic utility questioning of a deceptive cheater. After viewing the second video representing one of these conditions, participants

were instructed to complete a questionnaire which includes assessments of their perceptions of the interviewee's believability (i.e., sender believability), message coherence, and both a dichotomous and continuous assessment of the interviewee's message veracity.

After completing the measures of sender believability, message coherence, and message veracity, participants responded to manipulation checks of the trivia game briefing (i.e., to ensure that the relevant contextual knowledge needed to prompt participants out of the truth-default state in order to test hypotheses two through four was successfully imparted) and questioning type (i.e., to ensure that participants distinguished between the two questioning type conditions: background versus diagnostic utility). No manipulation check was employed for the honest non-cheater versus deceptive cheater conditions in this 2 by 2 design, as those are objectively established in the study stimuli. In addition, participants were asked to respond to assessments of relevant control variables (i.e., lie acceptability and general communicative suspicion). The questionnaire concludes with questions regarding the participants' demographic information such as age, sex, ethnicity, and nationality.

At the close of the study survey, after the demographic items were completed, participants were debriefed regarding the purpose of the study. They were informed that the goals of the study were to understand how individuals judge deceptive messages when there is no forewarning of deceit. The survey concluded by asking participants if they still agreed to have their data included in the final data analyses after they were informed of the true purpose of the study. No participants withdrew their data after debriefing. After responding to this question, participants were finished with the

questionnaire and compensated through MTurk.

Instrumentation

In this section, the instrumentation used for this dissertation study is discussed, including reliability estimates using coefficient omega (ω; Hancock & An, 2020). Although traditional social science scholarship has used Cronbach's alpha (α), there are noteworthy limitations and strict assumptions to α that are not met by the data from this dissertation. Goodboy and Martin (2020) outline how omega's use of confirmatory factor analysis (CFA) to calculate the statistic provides a more accurate test of instrument reliability because ω does not assume essential-tau equivalence, factor loadings and residual variances are free to vary through CFA, and the resulting statistic may be interpreted as a true score variance. The advantages of ω are apparent, thus this study implemented its calculation for reliability estimation. Omega reliability coefficients are reported for each of this study's variables along with 95% confidence intervals from 5,000 bootstrap samples, calculated with Hayes and Coutts' (2020) OMEGA macro for SPSS.

Manipulation Checks

Briefing

A short quiz was used to check whether the briefing information provided to all participants at the end of Phase I was retained, as this knowledge is necessary to meet TDT's criteria that accurate contextual information is needed in order to increase deception detection accuracy through diagnostic utility questioning [see Appendix A for the quiz]. The most crucial pieces of information that would accurately inform participants and prompt them out of the truth-default state include: 1) interviewees

earned cash for every correct answer of the trivia game (i.e., there was a motive to cheat), 2) interviewees had a clear opportunity to cheat (i.e., so it is possible that they would have cheated and may be dishonest about that during the interview), and 3) it was solely up to the interviewee to decide whether or not to cheat (i.e., the confederate only provided an opportunity to cheat without persuading the interviewee to cheat). Bearing these issues in mind, the quiz asked participants what interviewees won with each correct answer, whether someone left the room during the trivia game, and what the unique detail was about the partner in the trivia game. These pieces of knowledge serve as a manipulation check as they should indicate that the briefing successfully prompted participants out of the truth-default state and that they were indeed informed with relevant, context-sensitive information.

Diagnostic utility. A manipulation check is also necessary to establish whether the diagnostic utility questioning type (compared to the background questioning type) provoked more relevant information from the interviewee for the participant to use in judging the interviewee's veracity (see Appendix B). To check whether the two types of questions (see Appendix C for the two question sets) in the Phase II videos were different in the degree to which they provoked relevant information from the interviewees that could be compared against the basic background information about the trivia experiment provided in the briefing, participants completed a four-item, seven-point semantic differential scale assessing their perception of the relevance of the interviewee's answers to the briefing materials. The four items were originally used by McCornack et al. (1992) to assess relevance of a message. In this study, participants provided their assessment of the relevance of the questions (i.e., background questioning or diagnostic utility

questioning) to the information they were given in the Phase I briefing. Scale items are anchored using the following descriptors: irrelevant/relevant, inappropriate/appropriate, nonapplicable/applicable, and not pertinent/pertinent. Higher scores on this assessment indicate the answers were perceived by participants as more relevant to the briefing information they received, and thus, that the specific questions they heard on the interview were prompting them to consider the contextual knowledge provided to them prior to viewing the interaction. If the questions from the video truly contained diagnostic utility, relevance scores should be higher for participants in that group because the questions would be perceived as more relevant to their judgments of veracity.

Dependent Variables

The Truth-Default State

In Phase I, responses to the open-ended question after the first video (where participants were randomly assigned to observe either an honest non-cheater or a deceptive cheater) were used to operationalize the presence or absence of the truth-default state. Responses were coded for any mentions of veracity or deceit, following previous studies documenting the truth-default (Clare & Levine, 2019; Levine, Punyanunt-Carter, & Moore, 2020). Example mentions of veracity include, but are not limited to, descriptors such as deceptive, honest, sincere, insincere, truth-telling, and lying. If veracity was mentioned at all in the open-ended response, that response was coded as a 1. Responses that made no mention of veracity were coded as a 0. The subsequent analyses included 0 as indicative of being in the truth-default state and a 1 as indicative of having been prompted out of the truth-default state.

Veracity Judgments

To test hypothesis 3 in Phase II of the study, perceptions of message veracity were measured by participants' assessment of the truthfulness of the interviewee's response to each question on a seven-point Likert-type scale (1 = *Not truthful at all*, 7 = *Completely truthful*). Veracity judgments were averaged across all questions for a composite veracity judgment of the sender's responses, where lower scores indicate lower perceptions of honesty (see Appendix C). This operationalization of veracity was used in Burgoon et al. (1995), where individuals rated the veracity of an honest or deceptive sender after each response to an interview question. An example item for this measure regarding the background questions includes "How truthful was the interviewee's answer when they were asked 'How would you explain your success?'" An example item for this measure for the diagnostic utility questions includes "How truthful was the interviewee's answer when they were asked 'Did any cheating occur?'"

The random assignment to one of two different question sets inevitably leads to the use of two different scales to assess veracity in studies like this one, even though the questions are structured in the same manner. Participants assigned to the background questioning condition received six questions, while participants assigned to the diagnostic utility condition received nine questions. This variance in number of scale items prompted the calculation of a reliability estimate for each of the conditions. The message veracity scale reliability estimate in the background questioning condition with an honest non-cheater interviewee resulted in an omega reliability coefficient of .89 [95% CI: .82, .93]. The message veracity scale reliability estimate in the diagnostic utility condition with an honest non-cheater interviewee resulted in an omega reliability coefficient of .93

[95% CI: .90, .96]. The message veracity scale reliability estimate in the background questioning condition with a deceptive cheater interviewee resulted in an omega reliability coefficient of .86 [95% CI: .76, .91]. Finally, the message veracity scale reliability estimate in the diagnostic utility condition with a deceptive cheater interviewee resulted in an omega reliability coefficient of .90 [95% CI: .83, .93].

In order to test hypothesis 4 in Phase II of the study, message veracity was also measured using a dichotomous choice (see Appendix C). In this way, judgments of actual veracity (as well as judgments of actual dishonesty when referring to participants who judge a message as deceptive) could be assessed, as opposed to assessing *perceptions* of a message's veracity. The fourth hypothesis is distinct from the third hypothesis in that the outcome measure is objective detection judgment accuracy rather than subjective perceived message veracity, thus it was necessary to assess message veracity in this more objective way as well. Further, dichotomous assessments of objective veracity permit the ability to examine the judgement accuracy, or correctness, of the participants' veracity judgments. When measuring the accuracy of deception judgments, there is typically too much variance between "correct" judgments (e.g., two participants who marked a 5 and a 7 on veracity, respectively, for an honest non-cheater's message on 7-point Likert scale would both be considered correct in their judgment); therefore, actual detection judgment accuracy was measured dichotomously for hypothesis four so that every participant may be equally correct or incorrect in their judgment of the interviewee's veracity. Participants were asked to decide whether the interviewee was being truthful (coded as 0) or deceptive (coded as a 1) in the second video they viewed in Phase II of the study.

Mediators

Message Coherence

The perceived logical consistency or coherence of the interviewee's responses was measured using a five-item, seven-point semantic differential scale with descriptors of logical consistency (see Appendix D). The operationalization of message coherence in this study borrows from McCornack and colleagues (1992), who created semantic differential scales as their operationalizations of different types of lies. More specifically, participants were asked to report their perceptions of a message using synonyms and antonyms of the type of lie in question (e.g., vague/precise was used to measure perceptions after reading a deceptive message that was intentionally ambiguous). Following McCornack and others' logic, items for the proposed study were anchored by synonyms and antonyms of TDT's definition of message coherence. TDT defines coherence as the logical consistency of a message (Levine, 2019), so items for this study's operationalization of message coherence include the following endpoints: illogical/logical, incoherent/coherent, irrational/rational, inconsistent/consistent, and incompatible/compatible. Higher scores indicate greater interviewee/sender message coherence, as perceived by participants. The omega reliability coefficient for this measure was .89 [95% CI: .85, .92] in Phase I and .90 [95% CI: .87, .92] in Phase II.

Sender Believability

The interviewee's perceived believability in the video was measured using the honest demeanor index, or believability quotient (BQ; Levine et al., 2011). The BQ is a list of seven deceptive behaviors and four honest behaviors which contribute to perceptions of overall sender demeanor (see Appendix E). The BQ is an index computed

by subtracting the average seven-point Likert-type scale (1 = *strongly disagree*, 7 = *strongly agree*) ratings of the seven deceptive behaviors from the average 7-point Likert-type scale ratings of the four honest behaviors. The resulting index could range from -6 to 6 with positive values indicating a perception of greater believability whereas negative values indicate a perception of deceptiveness. The actual range of values for this study was -4.29 to 6 for Phase I (M = .52, SD = 1.45) and -3.89 to 5.50 for Phase II (M = .13, SD = 1.26). Past uses of the BQ have implemented third-party coders in order to establish a perception of believability, yet Levine et al. (2011) indicate the items may be used either by coders or as survey questions. In this study, participants responded to the items of the BQ as a Likert-type scale. Examples of honest demeanor questions include "The interviewee was confident and composed" as well as "The interviewee has an engaged and involved interaction style." Examples of deceptive demeanor questions include "The interviewee avoids eye contact" as well as "The interviewee appears tense, nervous, and anxious." The omega reliability coefficient for this measure was .86 [95% CI: .81, .89] in Phase I and .85 [95% CI: .80, .88] in Phase II.

Covariates

The literature on deception detection reveals two important variables which may impact some of the predictions of TDT. TDT's predictions are designed to be generalizable, even to high stakes deception interactions (e.g., police interviews); however, individual differences in perceptions related to deception may change results. Lie acceptability and general communicative suspicion are two such individual difference variables that are likely to impact judgments of deception across a variety of interactions. Lie acceptability is a general measure of how individuals view the act of lying with

regard to its appropriateness and suitability. General communicative suspicion (GCS) is a measure of an individual's general uncertainty regarding others' veracity. Oliveira and Levine (2008) found that those higher in lie acceptability were more likely to use deceptive omission and outright lies themselves, while Bond and DePaulo (2008) found that individuals higher in GCS were much more likely to judge others' messages as lies. It is important to examine the effects of both lie acceptability and GCS on perceptions of message veracity in the event that these variables need to be controlled for during hypothesis testing.

Lie Acceptability

The extent to which deceit is considered by participants to be a satisfactory method of achieving personal ambitions was measured using Oliveira and Levine's (2008) Lie Acceptability Scale as a possible covariate for veracity judgments. The scale (see Appendix F) consists of eight items assessed on a 7-point Likert-type scale (1 = *strongly disagree* to 7 = *strongly agree*). The items are structured so that higher scores indicate more lie acceptability. Example items include statements such as "It is often better to lie than to hurt someone's feelings" and "Lying is no big deal." The omega reliability coefficient for this measure was .75 [95% CI: .66, .81].

General Communicative Suspicion

The general suspicious attitudes of participants toward other communicators was assessed using the general communicative suspicion measure (GCS; Levine & McCornack, 1991). The GCS (see Appendix G) is an eleven item, seven-point (1 = *strongly disagree*, 7 = *strongly agree*) self-report scale which evaluates the attitude an individual has about the honesty of those with whom they interact. The items are

structured such that higher scores indicate more general communicative suspicion. Example items include “I often feel as if people aren’t being completely truthful with me” and “Dishonesty is a part of human nature.” The omega reliability coefficient for this measure was .86 [95% CI: .81, .89].

Data Analysis

Data analysis began with descriptive statistical analysis of demographic variables. The range and average age were computed as well as percentages of membership in groups defined by sex, race/ethnicity, nationality, and education level. Next, possible covariates (i.e., lie acceptability, GCS, sex) were examined. Specifically, Pearson correlation coefficients were used to examine continuous covariates such as lie acceptability and GCS with continuous study variables such as message veracity, sender believability, and message coherence.

Self-reported sex was examined as a potential covariate in this study because it has often related to differences in perceptions of lie acceptability and the self-reported telling of lies in past research, such that men are more likely to condone lying while women are historically more accurate in deception detection (Burgoon et al., 2006; DePaulo et al., 1996; Oliveira & Levine, 2008). Therefore, an independent samples *t*-test between males and females on perceptions of message veracity was used to assess whether or not sex should be included as a covariate in substantive analyses. Any significant relationships or differences that emerge from the preliminary analyses resulted in controlling for the relevant variable(s) during hypothesis testing.

To ascertain how well participants retained the briefing information in Phase I, and thereby to assess whether basic background knowledge regarding the trivia game has

been established regarding the proposed manipulation for Phase II of the study, the average quiz score was computed. The pieces of information contained within the quiz questions serve as indicators of the information prompted by the diagnostic utility question set. The questioning type (background versus diagnostic utility) manipulation check in Phase II was tested with an independent samples *t*-test comparing the relevance scores of participants in each questioning type group, where a significantly higher relevance score for the diagnostic utility group indicates a successful manipulation, as it suggests that these participants were effectively prompted out of the truth-default state for Phase II of the study.

Hypothesis One

The first hypothesis was tested using a binomial logistic regression. Sender believability and message coherence served as the predictors, while retaining (0) or abandoning (1) the truth-default state was the outcome variable. Nagelkerke R2 and Cox and Snell R2 were assessed for model fit. Classification rate before and after adding predictors to the equation was used to test significance of the overall model. Sender believability and message coherence were assessed as predictors through the unstandardized betas (B), Wald Chi Squares, Odds Ratios, p values, and 95% confidence intervals.

Hypotheses Two and Three

The second and third hypotheses was tested using path analysis with Hayes' (2018) PROCESS version 3.5 macro for SPSS Version 26, and Hayes and Preacher's (2014) method for modeling linear mediation using multi-categorical independent variables. In the proposed model, sender believability and message coherence are parallel

mediators of the effect of veracity (honest non-cheater/deceptive cheater) and questioning type (background/diagnostic utility) on perceived message veracity (see Figure 1). Conducting a parallel mediation allows for the analysis to control for each mediator's effect on the model simultaneously, thus providing a superior analysis for this study over two separate simple mediation analyses. Any variable revealed by preliminary analyses to significantly impact message veracity was included in the model as a covariate (e.g., lie acceptability, GCS, sex). Indirect effects will be calculated using 5,000 bootstrapped samples and 95% bias-corrected confidence intervals.

In order to allow for reparamaterization of the linear model when the independent variable is multi-categorical (rather than continuous or dichotomous, for example), the Hayes and Preacher (2014) procedure calls for the experimental conditions to be dummy coded for use in this analysis. Thus, the honest non-cheater/background questioning condition was designated as the referent group to which the other three conditions were compared. Hypothesis testing for the second and third hypotheses depended on the specific and total relative indirect effects of the mediation model. Specific relative indirect effect is the product of the *a* and *b* path coefficients for the specific mediator (i.e., sender believability and message coherence), while total relative indirect effect is the sum of the products of the path coefficients in the model (Hayes, 2018). If the bootstrapped confidence intervals of the specific and total indirect effects exclude zero, there is evidence of parallel mediation.

Hypothesis Four

Lastly, the fourth hypothesis was tested using a binomial logistic regression. Predictors for the regression equation included type of questioning (i.e., background

questioning = 0, diagnostic utility = 1), sender believability, and message coherence, which were entered to predict whether the participant correctly judged the objective veracity of the interviewee. As with the first hypothesis, Nagelkerke R^2 and Cox and Snell R^2 were assessed for model fit. Classification rate before and after adding diagnostic utility, sender believability, and message coherence to the equation was used to test significance of the overall model. Diagnostic utility, believability, and message coherence were assessed as predictors through the unstandardized betas (*B*), Wald Chi Squares, Odds Ratios, *p* values, and 95% confidence intervals.

Summary

This chapter details the methodological procedures, instrumentation, and data analysis for this dissertation, which experimentally tests several of TDT's propositions. The theoretically based predictions of this study include how the truth-default is abandoned, how messages are judged as deceptive, and how the accuracy of veracity judgments is increased (or decreased) by perceptions of message coherence, sender believability, and questioning effects. This chapter explains that study participants (recruited via MTurk) were randomly assigned to watch a series of two videos. In Phase I of the study, participants watched a brief video of either a truthful or deceptive trivia game interviewee. After the first video, participants completed a questionnaire involving an open-ended question checking for the truth-default and assessments of perceptions of message coherence and sender believability. Participants then were briefed with some basic information about the trivia experiment in order to provide them with rudimentary background information so as to prompt them out of the truth-default state before Phase II of the study. In Phase II, participants watched a second brief video representing one of

four conditions: honest non-cheater or deceptive cheater responding to background questioning; honest non-cheater or deceptive cheater responding to diagnostic utility questioning. After watching the second video, participants completed a questionnaire assessing perceptions of message coherence, sender believability, and message veracity. Finally, this chapter details including two manipulation checks, a logistic regression for both the first and fourth hypotheses, and linear mediation using multi-categorical independent variables (i.e., the four experimental conditions) with two parallel mediators for the second and third hypotheses.

CHAPTER III: RESULTS

In this chapter, the results of preliminary analyses are presented, followed by results of binomial logistic regression (used to test hypothesis 1). Then, the results of the study's manipulation checks are presented, followed by the results of mediation analyses (to test hypotheses 2 and 3) and binomial logistic regression (to test hypothesis 4). Hypothesis one was tested with Phase I data, while hypotheses two, three, and four were tested with data from Phase II of the study. Intercorrelations among study variables are presented in Table 1, along with composite means, standard deviations, and omega reliability coefficients with 95% confidence intervals from 5,000 bootstrap samples, calculated with Hayes and Coutts' (2020) OMEGA macro for SPSS.

Preliminary Analyses

In accordance with this study's data analysis plan, several tests were conducted to assess which known confounding variables needed to be controlled for in hypothesis testing. First, an independent samples *t*-test was conducted, which revealed significant differences between men and women in their perception of the message veracity of the interviewee/sender, t (213.97) = 2.71, $p < .01$, Cohen's $d = .31$. Men ($M = 5.37$, $SD = .90$) reported higher perceived message veracity than women ($M = 5.05$, $SD = 1.13$), indicating that men thought the interviewee was more honest on average than did women. The presence of sex's effect on perceptions of message veracity (also found in prior research on veracity perceptions; Burgoon et al., 2006; DePaulo et al., 1996; Oliveira & Levine, 2008) prompted its use as a covariate for tests of hypotheses 2 and 3.

Next was to test the two deception-specific covariates assessed for this study. Lie acceptability was tested with each of this study's outcome variables, and a significant

relationship emerged with perceived message veracity (r [359] = .12, $p < .05$), the absence of the truth-default state (t [40.34] = 2.61, $p < .01$, Cohen's d = .64), and lie judgments (t (76.31) = 2.96, $p < .01$, Cohen's d = .51.). Lie acceptability did not have any confounding relationship with judgment accuracy, t (230.36) = -.60, $p > .05$, Cohen's d = -.08. The results of these preliminary analyses indicated the inclusion of lie acceptability as a covariate in every hypothesis test moving forward.

Lastly, the effect of general communicative suspicion (GCS) on this study's dependent variables was probed. Analyses revealed one significant relationship with perceived message veracity, r (359) = .15, $p < .01$. GCS did not have a significant relationship with judgment accuracy (t [221.94] = 1.27, $p > .05$, Cohen's d = .16), the absence of the truth-default (t [40.68] = -1.32, $p > .05$, Cohen's d = -.32), or lie judgments (t [80.20] = 1.44, $p > .05$, Cohen's d = .24). The results of these preliminary analyses prompted GCS to be used as a covariate when testing hypotheses 2 and 3.

Table 1

Correlations of all study variables presented with means, standard deviations, and scale reliability estimates

Variable	2	3	4	5	6	7	8	9	10	M	SD	ω
1. Phase I Believability	.45***	.20**	.33***	.07	.01	.15*	-.04	-.42***	-.21***	.68	1.63	.86 [.81, .89]
2. Phase I Message Coherence	-	.02	.20**	.49***	.42***	-.09	.01	-.05	.11	5.59	1.10	.89 [.85, .92]
3. Absence of the Truth-Default		-	.04	-.07	-.20**	.22***	-.14*	-.23***	.11	.15	.36	-
4. Phase II Believability			-	.54***	.42***	-.39***	-.03	-.27***	-.26***	.18	1.46	.85 [.80, .88]
5. Phase II Message Coherence				-	.76***	-.55***	-.01	.03	.08	5.32	1.19	.90 [.87, .92]
6. Phase II Continuous Perceived Veracity					-	-.61***	.07	.08	.07	5.13	1.10	****
7. Phase II Lie Judgment						-	-.08	-.21**	-.10	.23	.42	-
8. Judgment Accuracy							-	.04	-.08	.50	.50	-
9. Lie Acceptability								-	.33***	3.56	.86	.75 [.66, .81]
10. General Communicative Suspicion									-	4.16	.77	.86 [.81, .89]

Note: * = $p < .05$; ** = $p < .01$; *** = $p < .001$. **** = Phase II Perceived Veracity is broken into four different scores for the four groups because the scale was based on the question set. The Honest Non-Cheater with Background Questioning group veracity reliability score was .89 [.82, .93]. The Deceptive Cheater with Background Questioning group reliability score was .86 [.76, .91]. The Honest Non-Cheater with Diagnostic Utility Questioning group reliability score was .93 [.90, .96]. The Deceptive Cheater with Diagnostic Utility Questioning group reliability score was .90 [.83, .93].

Phase I Results

The first hypothesis tested TDT's module regarding the abandonment of the truth-default state by predicting that decreases in sender believability (H1a) and sender message coherence (H1b) would result in a greater likelihood of abandoning the truth-default (as evidenced by participants' use of veracity- and/or deceit-related descriptors when listing their thoughts about the interviewee they observed). Lie acceptability was included as a covariate in this analysis because of its significant relationship with the absence of the truth-default state. Results of a binomial logistic regression were statistically significant, $\chi^2(3) = 13.50$, $p < .01$; and the Hosmer and Lemeshow test indicated good fit, $\chi^2(8) = 2.31$, $p = .97$. The variance explained by the model ranged from .06 (Cox & Snell R^2) to .10 (Nagelkerke R^2). A total of 83.7% of cases were correctly classified after adding the predictors to the model in contrast to the 84.5% correctly classified cases prior to variable entry. Of the three predictors, lie acceptability ($B = -.42$, Wald $\chi^2 = 3.90$, $p < .05$) was the only significant contributor, revealing that increases in lie acceptability decreased the chance of abandoning the truth-default state (Exp[B] = .66, Exp[B] 95% CI: .43, 1.00). Conversely, neither message coherence ($B = -.18$, Wald $\chi^2 = .84$, Exp[B] = .84, Exp[B] 95% CI: .68, 1.09, $p = .36$) nor sender believability ($B = .23$, Wald $\chi^2 = 3.20$, Exp[B] = 1.26, Exp[B] 95% CI: .98, 1.63, $p = .07$) were significant predictors of the abandonment of the truth-default state. Thus, neither H1a nor H1b were supported.

Phase II Results

Phase II of this study begins with an overview of the manipulation checks that were conducted regarding participants' retention of the briefing information (introduced

after Phase I in order to provide participants with relevant contextual information intended to prompt them out of the truth-default state) and participants' awareness of the type of questioning set to which they were exposed (which constituted the study's experimental manipulation of background questioning versus diagnostic utility questioning).

Manipulation Checks

Briefing Check

Two manipulation checks were necessary in this experimental study. The first check assessed the briefing knowledge participants retained prior to observing the Phase II video. Participants responded to three quiz questions pertaining to the background information from the trivia experiment. The first question asked if the participant remembered what the reward was that each trivia contestant earned for answering questions correctly, with cash as the correct answer. Only 27.4 % of participants answered the first question correctly. The second question asked if the participant remembered if the interviewee had an opportunity to cheat during the trivia experiment. Only 39.6% of participants answered the second question correctly. Finally, the third question asked if the participant remembered that the partner of the interviewee was a confederate of the study. Participants performed best on this third question, with slightly more than half (53.5%) responding correctly. Taken together, the average participant accuracy rate on the briefing manipulation check was just over 40%. Further, out of all participants, 76 (21.1%) answered all three quiz questions correctly, 49 (13.6%) answered two of the three quiz questions correctly, 109 (30.2%) answered one of the three quiz questions correctly, while 127 (35.2%) provided incorrect responses to all

three quiz questions.

If participants who did not answer all three quiz questions correctly were removed from further consideration, then only 21.1% of the sample (76 participants) would have remained for analysis. Because of this, the decision was made to retain for analyses all participants who responded correctly to at least one of the three quiz questions identified previously (n = 234). Any participant who answered none of the briefing questions correctly (n = 127), however, was removed from the study for further analysis (i.e., these individuals were not included in tests of hypotheses 2 through 4). Although the amount of questions answered correctly varied among the participants who were retained for analyses, as explained previously, the participants who were retained demonstrated evidence (in the form of correctly answering at least one quiz question) that they knew *some* amount of contextual information regarding the video they watched in Phase II. The total number of participants for Phase I (H1 testing) was 361, while the total number of participants for Phase II (H2-H4 testing) was 234.

Questioning Check

Data analysis continued with a manipulation check of the two question sets (i.e., background questioning versus diagnostic utility questioning). This manipulation check was intended to assess the effectiveness of this study's experimental manipulation founded on TDT's claim that questions which provoke information with diagnostic utility are superior to simple background questions in improving deception detection accuracy. A four-item relevance scale was adapted from McCornack et al. (1992) and used as a dependent variable in an independent samples *t*-test between the two questioning set types (background versus diagnostic utility). The independent samples *t*-test was

nonsignificant, $t(219.52) = -.32$, $p = .38$, Cohen's $d = -.04$, indicating that participants who were exposed to the video that featured diagnostic utility questioning ($M = 5.31$, $SD = 1.10$) did not perceive that question set to be any more useful and relevant than the group of participants who were exposed to the video that exhibited general background questioning ($M = 5.27$, $SD = 1.33$).

Additionally, to explore further detailed differences between the Phase II conditions, a one-way analysis of variance (ANOVA) was conducted with the four-item relevance scale as a dependent variable by the four conditions (type of interviewee: honest non-cheater versus deceptive cheater by type of questioning set: background questioning versus diagnostic utility). The ANOVA allowed for further explanation of the effect of objective message veracity in addition to the two question sets. Levene's test of homogeneity did not indicate significantly different variance, $F(3, 357) = .50$, $p = .68$; however, the results of the ANOVA will report Welch's F in order to provide more conservative estimates of the results related to the experimental manipulation. The ANOVA was nonsignificant, $F(3, 197.065) = .15$, $p = .93$, partial $\eta^2 = .001$, providing further evidence that participants who received the diagnostic utility question set (deceptive cheater: $M = 5.38$, $SD = 1.05$; honest non-cheater: $M = 5.35$, $SD = 1.04$) did not perceive their questions to be any more relevant than participants who received the general background question set (deceptive cheater: $M = 5.34$, $SD = 1.23$; honest non-cheater: $M = 5.45$, $SD = 1.16$), regardless of the objective veracity of the message they viewed.

Taken together, the results from the preliminary analyses and manipulation checks necessitate changes to this study's original data analysis plan for Phase II. The

manipulation check of the two question sets (background questioning versus diagnostic utility) failed to show any meaningful difference in the two question sets providing relevant information to participants, therefore prompting a collapse of the experimental groups from four conditions to two conditions: those who viewed the interview of a deceptive cheater, and those who viewed the interview of an honest non-cheater (as objectively determined by the trivia participant in the video participants watched, who either lied or did not lie). For hypotheses 2 and 3, this resulted in testing a different model than originally proposed for this study. Rather than comparing the honest non-cheater interview with background questioning group as the referent for the rest of the conditions, the analysis compared those who viewed an honest non-cheater with those who viewed a deceptive cheater, dummy coded 0 (honest non-cheater; $n = 124$) and 1 (deceptive cheater; $n = 110$).

In terms of covariates, the parallel mediation analysis used to assess hypotheses 2 and 3 employed participant sex, lie acceptability, and general communicative suspicion as covariates because of their significant relationships with perceived message veracity. The number of correctly answered briefing check questions was also entered as a covariate to account for potential differences based on amount of contextual knowledge participants retained. In tests of hypothesis 4, lie acceptability was included as a covariate because of its relationship with judgement accuracy.

Phase II Hypothesis Testing

Hypotheses Two and Three

Using the ninth proposition of TDT as a theoretical foundation, the second and third hypotheses predicted that when provided with contextual knowledge, viewing a

deceptive (versus honest) message would cause participants to perceive decreased sender believability and message coherence (H2) which would, in turn, relate to lower perceptions of message veracity (H3). Hypotheses 2 and 3 were tested using parallel mediation analysis with Hayes' (2018) PROCESS version 3.5 macro for SPSS Version 26 by way of Hayes and Preacher's (2014) method for modeling linear mediation using a dichotomous independent variable (i.e., Model 4). As the experimental stimuli failed to provoke significant differences in participants' relevance perceptions regarding the type of question set (diagnostic utility versus background questioning), the experimental groups were collapsed from four to two conditions (honest non-cheater or deceptive cheater) as described previously and entered into the model as a dichotomous independent variable. Message coherence and sender believability were entered as the parallel mediators, with perceived message veracity entered as the outcome. Participant sex, lie acceptability, and general communicative suspicion were entered as covariates for the analysis. Additionally, the number of correctly answered briefing questions were entered as a covariate in this analysis to account for possible differences due to amount of contextual knowledge retained by each participant.

Results did not support the second hypothesis, which predicted that exposure to a deceptive versus honest message would result in decreased perceptions of sender believability and message coherence, $F(5, 226) = 4.59$, $p < .001$, $R^2 = .09$ (see Table 2 for unstandardized model estimates related to both hypotheses 2 and 3, including covariates). Differences between participants who viewed a deceptive message or an honest message were non-significant. Viewers of deceptive messages did not report perceiving lower message coherence ($a_1 = .03$, $p = .85$, CI: -.28, .34) or decreased sender believability ($a_2 =$

.06, p = .77, CI: -.31, .42) controlling for sex, lie acceptability, GCS, and the number of briefing questions correctly answered, as predicted. Instead, the coefficients for both mediators (message coherence and sender believability) were non-significant.

The third hypothesis was tested using the same process as described for H2. The third hypothesis predicted that lower message coherence and lower sender believability would mediate the relationship between actual message veracity and perceived message veracity. The third hypothesis was also not supported. The final empirical model may be found in Figure 2. When controlling for participant sex, lie acceptability, GCS, and the number of briefing check question answered correctly, only message coherence (b_1 = .63, p < .001, CI: .53, .73) significantly predicted perceptions of message veracity. Neither objective veracity (c' = .10, p = .30, CI: -.09, .28) nor sender believability (b_2 = .06, p = .19, CI: -.03, .14) significantly predicted perceived message veracity. There was no evidence of a mediated relationship between actual and perceived veracity either through message coherence (a_1b_1 = .02, CI: -.19, .22; a_1b_{1ps} = .02, CI: -.18, .19) or sender believability (a_2b_2 = .00, CI: -.03, .04; a_2b_{2ps} = .00, CI: -.02, .03) while controlling for each other.

Table 2

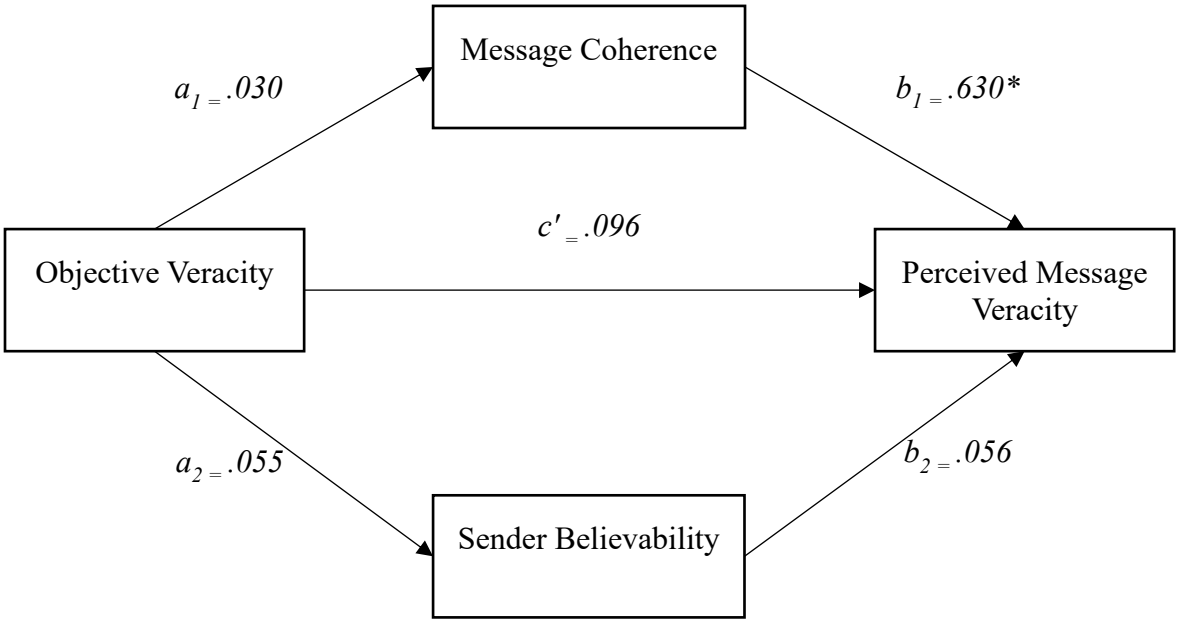
Unstandardized model estimates for hypotheses 2 and 3 with the inclusion of covariates

Hypothesis 2 and 3 *a1b1* = 0.02, CI: [-0.18, 0.22] *a2b2* = .00, CI: [-0.03, 0.03] *c′* = 0.10, *p* = 0.30			
	$F(5, 226) = 5.35$, $p < .001$, $R^2 = .11$		
	Believability		
	Estimate	SE	*p*
Objective Message Veracity	0.05	0.19	0.77
Believability	-	-	-
Message Coherence	-	-	-
Sex	0.03	0.20	0.90
Lie Acceptability	**-0.36**	**0.12**	**< 0.01**
GCS	**-0.36**	**0.13**	**< 0.01**
Briefing Questions Correct	**-0.22**	**0.09**	**0.22**
	$F(5, 226) = 2.15$, $p = .06$, $R^2 = .05$		
	Message Coherence		
	Estimate	SE	*p*
Objective Message Veracity	0.03	0.16	0.85
Believability	-	-	-
Message Coherence	-	-	-
Sex	-0.30	0.17	0.07
Lie Acceptability	-0.08	0.10	0.44
GCS	0.07	0.11	0.50
Briefing Questions Correct	**-0.22**	**0.09**	**0.22**
	$F(7, 224) = 49.10$, $p < .001$, $R^2 = .61$		
	Perceived Message Veracity		
	Estimate	SE	*p*
Objective Message Veracity	0.10	0.09	0.30
Believability	0.06	0.04	0.19
Message Coherence	**0.63**	**0.05**	**< .001**
Sex	-0.17	0.10	0.10
Lie Acceptability	0.03	0.06	0.57
GCS	-0.01	0.07	0.87
Briefing Questions Correct	**-0.16**	**0.06**	**< .01**

Note. Bold text indicates significant unstandardized estimates.

Figure 2

Parallel mediation analysis for hypotheses 2 and 3



Note. Indirect effects did not provide any evidence of parallel mediation for message coherence (a_1b_1 = .019, CI: -.190, .219) or sender believability (a_2b_2 = .003, CI: -.026, .035).

Hypothesis 4

Whereas hypothesis three was concerned with perceived message veracity, the final hypothesis was based on TDT's prediction that those who viewed an interviewee/sender who was questioned with diagnostic utility would have higher objective judgement accuracy than those who watched an interviewee who was questioned with simple background questioning. Because of the failure in experimental manipulation between the two question sets (background questioning versus diagnostic utility), the final analysis reported here used objective veracity (honest non-cheater versus deceptive cheater) only as a code between groups. The final hypothesis predicted that,

when equipped with contextual knowledge, exposure to interviewing with diagnostic utility (as compared to general background questioning) would result in higher objective judgment accuracy when controlling for sender believability and message coherence. To test this hypothesis, a binomial logistic regression was conducted using objective veracity as a predictor of the accuracy of participants' dichotomous lie judgments. As was the case regarding the analyses of the second and third hypotheses, because experimental manipulation of the questioning types failed, objective veracity was used as a grouping mechanism. Lie acceptability, sender believability, message coherence, and amount of briefing questions answered correctly were all entered into the model as covariates.

Results of the logistic regression were significant, $\chi^2(5) = 72.88$, $p < .001$; the Hosmer and Lemeshow test indicated good fit, $\chi^2(8) = 3.86$, $p = .87$. The variance explained by the model ranged from .27 (Cox & Snell R^2) to .36 (Nagelkerke R^2). A total of 76.9% of cases were correctly classified after entering the predictors into the model in contrast to 50.4% of correctly classified cases with no predictors. Objective veracity (B = -2.41, Wald $\chi^2 = 58.68$, $p < .001$) was the only significant predictor of judgment accuracy when controlling for lie acceptability, sender believability, perceived message coherence, and the amount of briefing questions answered correctly. Observers of deceptive messages had a significantly lower chance of accurately judging the veracity of the message (Exp[B] = .09, Exp[B] 95% CI: .05, .17). Using the constant and unstandardized Beta for objective message veracity (B = -2.41), the logistic regression results may be used to calculate the probability of a participant accurately judging the message's veracity given the condition the participant was in (i.e., whether they observed the interview of an honest non-cheater or a deceptive cheater). In this study, observers of

deceptive messages showed a 9.21% chance of judging the message correctly (i.e., as a deceptive – rather than an honest – message). Inversely, observers of an honest message showed a 53.12% of judging the message correctly (i.e., as an honest – rather than a deceptive – message). Lie acceptability (B = .09, Wald χ^2 = .17, Exp[B] = 1.09, Exp[B] 95% CI: .73, 1.62, p = .68), message coherence (B = .08, Wald χ^2 = .27, Exp[B] = 1.09, Exp[B] 95% CI: .79, 1.50, p = .61), sender believability (B = -.06, Wald χ^2 = .21, Exp[B] = .94, Exp[B] 95% CI: .71, 1.23, p = .64), and number of briefing questions answered correctly (B = .12, Wald χ^2 = .41, Exp[B] = 1.13, Exp[B] 95% CI: .78, 1.65, p = .52) were all non-significant predictors of deceptive judgment accuracy.

Summary

This chapter details the results of preliminary analyses, two manipulation checks, and the study results (i.e., the results of hypotheses testing for Phase I and Phase II). This study used TDT as a theoretical backbone to posit and test four hypotheses. Over 350 participants from a diverse range of backgrounds were surveyed for analysis in Phase I. During Phase I, the first hypothesis was tested using a binomial logistic regression, wherein only lie acceptability was found to be a significant, negative predictor of the abandonment of the truth-default state. Fewer than 250 participants remained after a significant number of participants failed to retain any of the briefing information provided following Phase I to establish contextual knowledge necessary for Phase II. Further, as participants failed to perceive a significant difference between the two question sets, those conditions were collapsed from four to two: an honest non-cheater condition versus a deceptive cheater condition. The second and third hypotheses were tested with participants who did retain some of the briefing information using a parallel

mediation model, but there was no evidence of mediation either through message coherence or sender believability when controlling for participant sex, lie acceptability, general communicative suspicion, or the number of briefing questions answered correctly. The fourth hypothesis was tested using binomial logistic regression, and only the objective veracity of the message was a significant, negative predictor of judgment accuracy after controlling for lie acceptability, message coherence, and sender believability. None of the four study hypotheses were supported.

CHAPTER IV: DISCUSSION

This dissertation represents an experimental examination of TDT (Levine, 2014, 2019) with the general goal of gaining a greater understanding of how the truth-default impacts observers' perceptions of communicators' messages. Across two phases, participants were tested on their reactions to truthful or deceptive communication with and without prompting them that deceit was possible. For the first phase, TDT's seventh proposition was put to the test, where the presence or absence of the truth-default was examined by having participants assess the message coherence and believability of the interviewee (who was either a deceptive cheater or an honest non-cheater in a trivia game) they witnessed. Only a control variable, lie acceptability, was revealed as a significant predictor of the presence of the truth-default state. After the first phase of the study, participants were briefed on the context of the interviews they observed in order to attempt to provoke them out of the truth-default state by providing them with contextually relevant information, then randomly assigned to one of four separate groups to observe either truthful or deceptive interviewees who were questioned with either basic background questioning or diagnostic utility questioning. In this dissertation, the manipulation between question types failed, leaving objective message veracity (an objectively truthful versus deceptive interviewee) as the distinguishing factor between groups in the second phase of the study.

Phase II explored how observers process truthful or deceptive communication based on the predictions of TDT's ninth and twelfth propositions, which discuss how messages are judged as deceptive through the same triggers that prompt the abandonment of the truth-default state (proposition 9) and that questioning which produces diagnostic

utility should lead to higher judgment accuracy as compared with relying on perceptions of sender believability or message coherence (proposition 12). Participants were again shown an interview of a deceptive cheater or an honest non-cheater in a trivia game and asked to assess the veracity of the interviewee as they responded to an interviewer's background questioning or diagnostic utility questioning. No evidence of mediation through message coherence or sender believability was discovered, as only message coherence acted as a significant predictor of perceived message veracity in the mediation model. Additionally, the accuracy of participants' perceptions of message veracity did not have any significant relationships with this study's primary predictor variables (i.e., message coherence and sender believability).

Foundational Findings

Although the theoretically driven hypotheses of this study were unsupported, some findings from this investigation nevertheless help to refine and add to extant deception scholarship. These are findings which either augment with additional evidence some of what has already been established in deception research, or showcase a need for further explanation or application in future deception research. The findings discussed involve the prevalence of the truth-default state in Phase I, the truth bias found in Phase II (specifically related to hypothesis 4), and the weakness of sender believability as a predictor of perceived message veracity.

The Prevalence of the Truth-Default and Truth Bias

The prevalence of the truth-default in Phase I, where 87.5% of participants were operationally in the truth-default state, is consistent with what might be expected based on relevant deception literatures. The mechanisms of the truth-default appear to be

operative in a variety of different environmental settings, including experimental settings like that of the present study, all of which lead observers of messages to default to a lack of any veracity evaluation whatsoever in most circumstances. One longstanding foundational principle of the truth-default is the prevalence of lying in everyday communication. Over decades, deception scholarship has consistently found that an accepted average of one to two lies told per day is explained by a log function, meaning most people tell less than one lie per day but there are a few prolific liars who skew the average steeply upward to almost two lies per day (DePaulo et al., 1996; Serota et al., 2010). This knowledge regarding the presence of prolific liars indicates that most communicators do not encounter lying regularly, in all likelihood; rather, most of the communication we experience is actually truthful – this makes observers more susceptible to occasional deception as we "get comfortable" in the knowledge that most people tell us the truth most of the time.

In this dissertation, participants were recruited into an online experimental setting using MTurk, and it is reasonable to think that the amount of studies which expose participants to outright lies would be rare, perhaps only for occasions when researchers need to hide information for reasons of experimental control. Even if the rate of outright lying or other (arguably) milder forms of deception is potentially higher in scientific research, TDT's underlying assumptions contend that the prevalence of lying in everyday life has conditioned participants along with laypeople more generally to default to not evaluating veracity (Levine, 2019). Deception scholarship has coined this as the veracity effect (Levine et al., 1999), and social scientific research may represent a context that exacerbates the veracity effect because of the trust that participants place in researchers

and/or research institutions when they agree to participate in studies (Guillemin, Barnard, Allen, Stewart, Walker, Rosenthal, & Gillam, 2018). Clearly, even when participating in survey after survey such as MTurk participants often do, individuals are reluctant to abandon the truth-default state. The results of this study support and extend the strength and generalizability of the veracity effect.

One reason the truth-default might continue to be high amongst the population in this study is that this sample of participants is likely to be observing messages, videos, and questionnaires in the most efficient manner possible. MTurk participants typically take several surveys over a short period of time in order to earn extra cash (Schmidt, 2015), so they may be sacrificing veracity evaluation in order to conserve their mental capacities for speed and efficiency on necessary or otherwise required tasks (Fiske & Taylor, 1991). Recent scholarship related to the truth-default shows that unless specifically prompted to evaluate the veracity of a message, such as with veracity judgement surveys, participants rarely reference truthfulness or deceptiveness when describing the message of a sender (Clare & Levine, 2019). In sum, this dissertation provides another line of evidence detailing the overwhelming prevalence of the truth-default in observers of messages across all kinds of different contexts and relationship types (strangers, close others, etc.; Bond & DePaulo, 2006; Levine et al., 1999; Zuckerman et al., 1981).

Another finding from this study which strengthens and perhaps extends foundational deception-related knowledge is the level of truth bias that appeared to be operative in Phase II, where 83.4% of participants across both conditions judged the message they saw as truthful. The truth bias has been at the forefront of TDT's

assumptions and propositions. As with the prevalence of the truth-default state, the prevalence of lying also contributes to why observers are traditionally truth biased. With regard to the findings from this particular study, however, a difference emerged in that truth bias remained even after explicit efforts were made to prompt participants out of the truth-default state. This is not completely uncommon, as meta-analyses on deception detection explain a slightly better than chance deception detection accuracy rate across the board through truth bias (Bond & DePaulo, 2006; Levine, 2019); commentary on these findings has indicated that the reason average accuracies are even slightly better than chance is because of the high accuracy of those who are randomly selected into judging honest messages (e.g., people who are in the position of being asked to judge if a message is honest are going to be correct most of the time, in part because of the strength of the truth bias; Levine, 2019). In a sense, TDT suggests that truth bias is an adaptation of human communication which helps observers process communication efficiently, even though it opens them up to the possibility of being duped by occasional deceit (Levine, 2019). As with the prevalence of the truth-default state, this is even more pronounced for instances where participants are actively judging the veracity of a message because truth bias still persists.

Sender Believability's Underperformance

Finally, the findings from this study relate to those in the foundational deception literature regarding sender believability. Historically, sender believability has demonstrated utility as an index and predictor of perceived message veracity (Levine, 2010; Levine et al., 2011). Yet, across both phases of the current study, perceptions of sender believability did not contribute in meaningful ways to judgments of perceived

message veracity regardless of objective veracity. Sender demeanor is a core construct in the list of triggers which TDT predicts will lead to the abandonment of the truth-default state (Levine, 2014, 2019), and sender believability serves as a typical operationalization of sender demeanor (Levine et al., 2011; Levine, 2014, 2019). However, the weaknesses of sender believability as an important predictor were present for both phases of the current study, regardless of the abandonment of the truth-default, the process of message veracity perception, or objective message veracity judgment accuracy.

According to the models tested as part of the present study, sender believability simply did not perform as theoretical and empirical evidence tells us it should have. In hypothesis 1, sender believability had no predictive utility regarding the abandonment of the truth-default state. For hypotheses two and three, neither the number of briefing questions known (a proxy for the amount of contextual knowledge participants retained) nor the objective veracity of the interviewee in the video had any effect on participants' perceptions of the believability of the interviewee. In turn, sender believability had no effect on perceptions of message veracity when controlling for objective message veracity, message coherence, and number of briefing questions known. Finally, and in opposition to deception literature in general (Levine et al., 2011) and TDT's propositions in specific (Levine, 2014, 2019), sender believability had no effect on the accuracy of a given participant's objective veracity judgment.

The poor performance of sender believability as a predictor in the current study is extremely disappointing considering its place of privilege in operationalizing sender demeanor. Even TDT positions sender believability as in flux (Levine, 2014), where those in the truth-default state are not closely scrutinizing sender believability, yet it is a

key piece of the theoretical process of abandoning the truth-default state and evaluating message veracity (Levine, 2019). Based on the findings of this dissertation, it appears sender believability underperformed as a predictor largely because of the overpowering mechanisms within truth bias. Following the logic of TDT, sender believability's ineffectiveness in this study can be traced through the propositions of TDT and how the theory sets up the process for message veracity evaluation. In regard to the prevalence of the truth-default state, there are not enough instances where MTurk participants are expecting to be deceived, they are trying to move quickly through each survey for better efficiency of time spent to money earned, and therefore are not dedicating key cognitive resources to examining, scrutinizing, or vetting the information in front of them. Sender believability as a function of truth bias is likely one of the implicit tools that allows typical MTurk participants to move through the studies they complete without rigorous scrutiny, which may explain the ineffectual nature of sender believability in this particular context.

In regards to truth bias, TDT explains that even those prompted out of the truth-default state are still biased into believing that communicators are truthful or may even slip back into the truth-default state at times (Levine, 2014, 2019). Although we did our best to effectively prompt participants out of the truth-default state, the impact of sender believability was likely subordinate to the truth bias in the sample and the failure of effectively prompting participants out of the truth-default state. There are several pieces of evidence which suggest that this study failed to successfully prompt participants out of the truth-default state, such as the failure of the briefing check and possibly the failure of participants to perceive a difference in the questioning sets as well. The complete lack of

success at prompting participants to remember very general yet key details of the trivia experiment is evidence of an unsuccessful prompt out of the truth-default state. Without knowing key details of the trivia experiment, participants would have no reason to scrutinize sender demeanor closely, certainly not enough to use sender believability as a crucial means to judging the veracity of the message.

Theoretical and Practical Implications

Underlying Assumptions of TDT

Though disappointing, the results of this study do support several underlying assumptions of TDT. One assumption of TDT posits that the complexity of the abandonment of the truth-default state results in communicators rarely going through the process of abandonment (Levine, 2014, 2019). Clare and Levine (2019) found empirical evidence of this assumption, and it is worthwhile to reiterate that the same operationalization of the truth-default state was used in the current study. Mentions of veracity, honesty, or deception were few and far between in the open-ended responses to the first video shown in Phase I of this study. To be fair for participants, any and all possible triggers to the abandonment of the truth-default in the Phase I video participants viewed were accounted for and removed, which resulted in only showing participants the responses of the interviewee, providing no indication that the interviewee might be deceptive, and not providing the full context of the trivia game to our participants. In order to truly provide a controlled environment for examination of the abandonment process, there were not many theoretically-identified triggers available to the participant in the Phase I videos. As Clare and Levine showed in their study, the lab environment is an ideal place for the truth-default state to hold up because there is little to no

interpersonal engagement or connection between the sender and the receiver (or sender and observer in this case). In the laboratory environment, scientists' participants typically are not actually suspects in a crime, accused of cheating on their partner, or provided with an opportunity to cheat on a test.

The rare occasion of abandoning the truth-default state leads to the second assumption of TDT that held up in this study, which is that observers of deceptive messages are overwhelmingly truth biased (McCornack & Parks, 1986; Levine, 2014, 2019). Even when direct efforts are undertaken to prompt people out of the truth-default state, observers are still much more likely to fall back into the truth-default or judge a message as truthful than they are to judge a message as deceptive. Phase II of this study was a shining example of the pervasiveness of the truth bias, even in an observational setting (versus an interaction). Across conditions in this study, participants judged the message they viewed as truthful much more often than deceptive. Levine (2019) presents truth bias as an underlying mechanism to the slightly better than chance accuracy average across deception detection research. Most studies split their participants randomly into either honest or deceptive message conditions, so when many more participants are concluding that a message is honest rather than deceptive, those who are randomly assigned to honest message conditions in an experimental setting are extremely accurate. This foundational claim of TDT was supported in this dissertation, and adds yet another line of research supporting the notion that the truth bias and the truth-default state will be much more common in the general population than are skepticism and disbelief.

Understanding the Truth-Default and Truth Bias in Context

The findings from this dissertation lend several implications to deception

detection in the observation of communication. The first is the overwhelming presence of the truth bias among this group of participants. The majority of participants (87.5%) in Phase I made no mention of the veracity of the interviewee and 83.4% of participants in Phase II judged their interviewee as honest, even though around 53% of them were viewing a deceptive message. TDT makes several assumptions regarding the reason communicators are susceptible to deception, and truth bias is one of the greater foundational pillars of the truth-default state. Communicators are unequivocally truth biased (Bond & DePaulo, 2006; Levine, 2014, 2019). This truth bias is a key contributor to the presence of the truth-default, perception of message veracity, and ultimately, veracity judgements. Truth bias keeps observers locked into the truth-default state, where they may efficiently communicate by accepting an underlying assumption of truthful communication and relinquishing the cognitive strain of skeptically reviewing the veracity of each message received (Levine, 2019).

Aside from the truth-default, truth bias still persists in observers after they have been prompted out of the truth-default state, where it has influenced veracity judgements across deception detection research (e.g., Bond & DePaulo, 2006). Even with manipulations of suspicion, deception detection training, or various interviewing techniques, deception detection research has consistently shown truth bias to be a confounding variable in deception detection accuracy, and this dissertation is no different. This fact is even more relevant when it comes to the context of this experiment. Participants in this dissertation were observers of a recorded interaction through an online mechanism, where their only known motivation for completion of the study was for monetary reward. As research in computer-mediated communication (CMC) continues to

grow and solidify itself as a pillar of interpersonal communication studies, deception detection research should also evaluate how these theories of deception detection may explain deceptive interactions taking place online. This particular study did not have the context of CMC because the participants were simply third-party observers in a type of online media consumption, admittedly. Even still, for this study, it appears that truth bias will persist whenever participants have no incentive, ability, or possibility to actually involve themselves in the interaction considering the number of participants who were in the truth-default, failed the briefing check, and ultimately judged a deceptive cheater as truthful. Even further, it may be that participants simply were not interested in the interaction they were tasked to observe.

Experimental Manipulation of Diagnostic Utility

An implication of measurement in this study is related to the inability, when using Levine's established experimental stimuli (Levine, Blair, & Clare, 2014), to effectively manipulate the two question sets (basic background questioning and diagnostic utility questioning). There is little doubt that the two question sets employed in the study stimuli have different styles and approaches, but the question sets also have similarities which, according to the findings of this study, overpowered the intended differences in questioning. Both background and diagnostic utility questioning take deception detection efforts a step further from simple cue-based detection efforts, but this important ability was lost in the present study given that participants did not perceive any meaningful difference between the two question sets on relevance, the very construct diagnostic utility questioning is supposed to excel at.

According to TDT, the primary vehicles for improving deception detection

accuracy involve providing contextual information to the observer (Blair, Levine, & Shaw, 2010), the direct nature of the questioning in comparison to other types of questioning or cue approaches (Levine et al., 2014), and the utilization of persuasive communication tactics during the interaction or questioning (Levine, 2019). Based on these three vehicles as identified by TDT, there are several explanations for the failure of participants in this study to perceive differences in the two types of questioning sets. Firstly, the disappointing results of the briefing manipulation check directly contribute to the failure of the question set manipulation check because of diagnostic utility questioning's reliance on contextual information (Levine et al., 2014; Levine, 2019). Diagnostic utility's primary claim regarding increasing deception detection accuracy is that it allows those with relevant contextual knowledge to use that knowledge when evaluating the veracity of the message. Yet, in this dissertation, only 76 total participants answered all three briefing questions correctly – questions which were very basic pieces of information about the trivia game. In the end, those who answered at least one question correctly were retained for Phase II data analyses, yet TDT would argue that those who did not answer every question correctly likely could not effectively leverage diagnostic utility questioning because having enough contextual knowledge is necessary to effectively assess the veracity of another's communication. Regardless of the comparison question set, diagnostic utility would not be any more relevant (i.e., the operationalization of this manipulation check) because its basic requirements were not met.

Secondly, the literature related to diagnostic utility questioning research touts the question type's direct nature as another vehicle to its success in increasing detection

accuracy (Levine, Blaire, & Clare, 2014). Although not directly connected to the context of this study, some of the scholarship on diagnostic utility questioning results in the prompting of confessions during the interactions because of the direct, probing nature of the questioning. For this study, there was no interaction between the participants and the interviewee in the trivia game; therefore, there is little to no difference in the relevance or usefulness of diagnostic utility because participants are only observing – and not asking – its direct questions (e.g., "When I interview your partner, what will they say about cheating?"). This is especially relevant considering the scholarship on how individuals actually detect lies, where contextual knowledge, whether discovered before or after the interaction, is often the reason that lies are discovered (Park et al., 2002).

Thirdly, persuasive tactics during deceptive interactions, although proposed in TDT as a vehicle to increasing detection accuracy through diagnostic utility questioning, were outside the scope of this study's abilities. Levine (2019) uses his experience observing the interviewing techniques of expert interrogators of federal law enforcement when describing the most effective diagnostic utility questioning. To him, the ability to utilize persuasive tactics such as follow up questions and pressuring cheaters to answer quickly could be used to great effect in prompting contextual information from the deceiver. In this dissertation, observers of the trivia game contestant were obviously unable to take advantage of these tactics, as they were merely observing rather than interacting with the trivia game interviewee. This contextual consideration is likely to influence the difference in relevance between the two questioning sets as well because diagnostic utility questioning could not capitalize on yet another key vehicle to success. The present study relied on Levine's experimental materials (Levine, 2007-2011; Levine

et al., 2010) which ultimately did not result in our participants being able to distinguish between the two types of questioning sets. There are no statistical analyses providing evidence of clear manipulation between the outcomes of diagnostic utility and background questioning up to this point in the literature, as typically no manipulation checks are employed to empirically assess differences in usefulness or utility between questioning with diagnostic utility versus basic background questioning (e.g., Levine, Blair, & Clare, 2014; Levine, Clare, et al., 2014). There are quite a few pieces of evidence showing diagnostic utility works at increasing deception detection accuracy (Levine et al., 2014), but without a tried-and-true experimental manipulation to provide evidence of a difference between the question sets, further research is necessary in order build upon the foundation of evidence of diagnostic utility's role in deception detection.

Interviewing and Questioning in Deception Detection

This dissertation also contributes to the literature on interviewing techniques in deception detection through its use of two separate question sets. An assumption was made in the present study that the objective difference in most question sets used for deception detection would result in clearly perceived differences between the questioning types, as evidenced by manipulation check results. The experimental stimuli used in this study employed two standard question sets (i.e., background questioning versus diagnostic utility questioning), used the context of the communication differently, and featured different tones in asking questions (e.g., diagnostic utility questioning uses direct questions in a slight accusatory tone). TDT has several propositions about questioning as a superior tool for deception detection (Levine, Clare, & Blair, 2014; Levine, Shaw, & Shulman, 2010; Levine et al., 2014), and these propositions are constructed on the

underlying assumption that questioning, as an active form of deception detection, is superior to other passive forms of deception detection because the receiver of the deceptive message is engaged with the deceiver. This type of engaged interaction should increase the likelihood that cues of deception or inconsistencies in deceiver communication rise to the surface and become detectable.

Unfortunately, this study showcased how the observation of good questioning is insufficient in and of itself to translate to an improvement in deception detection. This study operated on an assumption that the observation of interview interactions would prompt similar or equal contextual knowledge for observers as it does questioners, yet over half of this study's participants were incorrect in their veracity judgement – a stark decline in judgement accuracy in comparison to the average, slightly better than chance accuracy found in Bond and DePaulo's meta-analysis (2006). It may be that this dissertation instead provides evidence that deception detection outcomes are influenced by diagnostic utility questioning to a greater degree for the interviewer only (or the person in the interaction who is questioning the suspected deceiver; Burgoon & Buller, 2004), rather than for third party observers of the interaction.

In sum, the findings related to interviewing and deception detection provide evidence that the scope of TDT is related strictly to interpersonal interactions. Much of the modules of TDT are founded on the interpersonal context (e.g., "A Few Prolific Liars; Serota et al., 2014", "How People Really Detect Lies; Park et al., 2002", and "Content in Context; Blair et al., 2010"), yet there was one study conducted by Levine, Blair, and Clare (2014) which showed observers of interviews using diagnostic utility had increased deception detection accuracy, yet the findings of that study could not be

replicated here.

Engagement (or Lack Thereof) in Deception Detection

Although the study results were disappointing, these findings should not be taken as evidence condemning the use of diagnostic utility questioning; rather, these findings showcase the complicated nature and context dependency of deception detection. Most of the foundational assumptions and theoretical predictions of TDT were based on experiments which happened in a laboratory environment which entailed interpersonal, face-to-face communication. Only one of Levine's follow up studies to the trivia game experiment showed recordings of interviews to participant observers, and these videos were shown to students in a classroom setting (Levine, 2019). The context of the current study was completely online, where participants watched a video through an online service with no interpersonal connection to the interviewee. This (apparently complete) lack of engagement with the interaction is a possible contributor to the strength of the truth-default in situations where there is little to no interpersonal connection.

Engagement in deception detection is a key unspoken principle of TDT, mainly through its increase after the abandonment of the truth-default state, where the individual is in a heightened state of skepticism and veracity evaluation (Levine, 2014, 2019). Future research with TDT (especially when participants are observers rather than interactants in the potentially deceptive interaction) may wish to bring considerations of engagement in interpersonal deception to the forefront. Further, future research may wish to investigate the claims of TDT in potentially deceptive interactions that occur in mediated contexts such as virtual chats or video calls to examine TDT's propositions in an environment that is both engaging and interactive.

This dissertation also implicates another line of research into the truth-default state within the context of online media consumption. As with the discussion regarding engagement, the lack of interaction in this study for participants really defines this study's context as one of media consumption instead of interpersonal deception (Ahlers, 2006). The example of a participant using either their smart phone or a computer to watch a video of an interview is more akin to social media consumption, where users scroll through videos and images. Future research may be able to leverage this context for TDT, where the truth-default state may be tested in scenarios where the participant is scrolling through a series of videos of political candidates or potential dating partners, for example. TDT's propositions and assumptions may apply well to this context, given an adequate level of engagement on the part of participants – especially the module which predicts the processes which observers go through to abandon the truth-default. This study's context is very close to this, though limited by only using video recordings of an interviewee rather than using content more likely to be found in social media or online consumption. Although research with TDT has not yet reached this category of communication research, the findings from this study could provide some general preliminary guidance to researchers interested in expanding the scope of TDT to online media consumption.

Limitations

There are several limitations worthy of discussion in order to frame these findings in context. The first limitation with the most influence over this dissertation is the decision to sample participants from Amazon's Mechanical Turk. None of the prior experiments or studies that informed this experiment have used this service when recruiting participants, but the weaknesses of MTurk appeared to shine in this study.

There are a couple of decisions which contributed to poor results concerning the briefing check for this study. In order for participants to be paid for participation, they had to complete the survey; there was no stipulation that participants had to pass the briefing check quiz in order to participate. This enabled participants to click through most of the study without any real consequence other than removal from analysis; there was no consequence impacting their participation in the study. Requiring participants to pass the briefing check in order to participate would have given us a 100% score on the briefing check for each participant on the study, ensuring participants retain the necessary contextual information for deception detection: a cornerstone piece to diagnostic utility questioning.

Another limitation to this dissertation is the operationalization of continuous perceived message veracity. A core goal of this dissertation was to evaluate how observers of deceptive messages perceive the message's veracity in both a continuous way and dichotomous way. Dichotomous measures of perceived veracity have been a staple of deception detection research, where having too much variance in the outcome measure would result in inconsistencies between participant veracity judgments. For example, in a 7-point continuous measurement of perceived veracity, those who judged a message as truthful could score a message as a five, six, or seven, but be equally correct in their veracity judgements. This constrains the analysis of judgment accuracy, which is why a dichotomous operationalization of perceived veracity was employed here for analyses pertaining to the accuracy or correctness of the judgment. However, hypotheses two and three, based on propositions of TDT, used perceived veracity as the outcome rather than objective veracity. Thus, it became paramount to use a continuous

operationalization of veracity in order to capture the apparent differences in levels of truthfulness or deceptiveness observers perceive in messages. This practice is still a good one to use for future research, however future iterations should consider keeping a consistent number of items for each veracity assessment.

The use of continuous assessments of perceived veracity was necessary, but the specific scale used presented additional challenges in that the index was based on the type of questioning set to which participants were randomly assigned. Our decision to measure veracity in this way was based on past research which has taken the same approach (Burgoon et al., 1995), but such a choice prevented each participant's perceived veracity from being measured equally. Future examinations of TDT's propositions on perceptions of message veracity should consider continuous operationalizations which do not use the specific items from the two questioning sets themselves as scale items.

Lastly, the recruitment requirements concerning the briefing check for participants in this study were not strict enough and therefore limited the capability to give participants contextual knowledge to drive the strengths of diagnostic utility questioning. This was limited in two ways. First, participants did not have to pass the briefing check to receive payment for the study, they were only removed from analysis for failing to pass the check. Because of this, the study had a population which varied in the amount of contextual knowledge known. Although this variance was accounted for in subsequent analyses, namely hypotheses two through four, there were only 76 participants who answered all three questions correctly, which limited the ability to truly test diagnostic utility questioning given its reliance on content in context. This study should have implemented a requirement to answer all three briefing check questions

correctly to receive payment, and future research should consider the same.

The second final piece to the limitations regarding recruitment requirements is found in the delivery of the briefing information. For this study, participants were briefed using a page of text which gave participants clear, simple, to-the-point information that was needed to have context about the trivia game interview. Unfortunately, it is more likely that participants skimmed or skipped the briefing information, resulting in poor performance on the briefing check. Had the briefing check been delivered through video, where they were required to interact with the information needed to give them context, this study may have seen greater performance on the briefing check, especially when coupled with requiring participants to answer all three correctly to earn their reward.

Summary

The final chapter provides a discussion of results for this dissertation and includes an overview of the study, a discussion of theoretical and practical implications, and a review of the more important limitations influencing the interpretation of study results. This chapter overviews the details of the truth-default's overwhelming presence in Phase I, going into detail as to how TDT explains the pervasiveness of the truth-default state across various contexts and interactions. This chapter also discusses the effect of the truth bias in Phase II, where even after prompting out of the truth-default operationally, participants were still extremely biased into believing the interviewee they observed was being truthful. This chapter also explains the difficulty of establishing statistically meaningful differences between the typical question sets used in this study and other deception scholarship, and how this might influence the deception detection literature on questioning and diagnostic utility in particular. Finally, this chapter outlines some

limitations of this study which hinder its interpretive utility and discusses some potential future directions for TDT and deception scholarship.

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Appendix A

Briefing Manipulation Check
(Phase I)

Before you watched the second video, you were briefed about the details behind the trivia game and the interview you observed. Please answer the following questions about the details of the trivia game to show what you recall from the briefing. There is only one correct answer for each question.

1. What would interviewees earn with each correct answer to the questions in the trivia game?
    a. Research credit
    b. Bragging rights
    c. A random gift item
    d. Cash
2. Who left the room in the middle of the trivia game?
    a. The interviewee
    b. The partner
    c. The trivia administrator
    d. Another person
3. What was a unique detail about the interviewee's partner in the trivia game?
    a. They were just another interviewee.
    b. They were a confederate (i.e., they were "in on" the experiment).
    c. They were a friend of the interviewee.
    d. They were a friend of the trivia administrator.

Appendix B

Diagnostic Utility Manipulation Check (from McCornack et al., 1992) (Phase II)

Use the scale points below to describe your opinion regarding how well the interviewee's answers related to the briefing information you received before watching the second video.

The interviewee's answers were _____ to the briefing information I received before watching the second video.

Irrelevant	1-2-3-4-5-6-7	Relevant
Inappropriate	1-2-3-4-5-6-7	Appropriate
Nonapplicable	1-2-3-4-5-6-7	Applicable
Not pertinent	1-2-3-4-5-6-7	Pertinent

Appendix C

Veracity Judgment
(Phase II)

As you watch this second interview, please respond to the following questions about the truthfulness of the interviewee. Below are the questions the interviewee is asked. After each question, please rate how truthful you believe the interviewee was during the interview using the following scale:

Not truthful at all 1-2-3-4-5-6-7 Completely truthful

Background Questions

1. How truthful was the interviewee's answer when he was asked "Did you find the trivia questions difficult?"
2. How truthful was the interviewee's answer when he was asked "Was team work much of a factor?"
3. How truthful was the interviewee's answer when he was asked "In looking at your score, you did better than other groups. Does this surprise you?"
4. How truthful was the interviewee's answer when he was asked "How would you explain your success?"
5. How truthful was the interviewee's answer when he was asked "Did cheating occur when I left the room?"
6. How truthful was the interviewee's answer when he was asked "Why should I believe you?"

Diagnostic Utility Questions

1. How truthful was the interviewee's answer when he was asked "Tell me, in as much detail as you can, what happened during the trivia game?"
2. How truthful was the interviewee's answer when he was asked "How well did you do in the trivia game?"
3. How truthful was the interviewee's answer when he was asked "Which questions did you and your partner get right?"
4. How truthful was the interviewee's answer when he was asked "For the answers you got right, explain how you knew the right answer?"
5. How truthful was the interviewee's answer when he was asked "In detail, what happened when the experimenter left the room?"
6. How truthful was the interviewee's answer when he was asked "Did any cheating occur?"
7. How truthful was the interviewee's answer when he was asked "When I interview your partner, what will they say about cheating?"
8. How truthful was the interviewee's answer when he was asked "Did you and your partner discuss cheating?"
9. How truthful was the interviewee's answer when he was asked "If someone did cheat, what should happen to them?"

Dichotomous Veracity Measurement

If you had to choose only one option, do you think the interviewee was being honest or deceptive in the video you just watched?

- Truthful
- Lying

Appendix D

Message Coherence

Please respond to the following descriptions of the interviewee's responses. Select the value that you feel best represents the interviewee's responses.

The interviewee's answers to the interviewer's questions were…

Illogical	1-2-3-4-5-6-7	Logical
Incoherent	1-2-3-4-5-6-7	Coherent
Irrational	1-2-3-4-5-6-7	Rational
Inconsistent	1-2-3-4-5-6-7	Consistent
Incompatible	1-2-3-4-5-6-7	Compatible

Appendix E

Sender Believability

Please respond to the following descriptions of the interviewee's responses. Select the value that you feel best represents the interviewee's responses. Response scale:

1 = *Strongly Disagree*, 7 = *Strongly Agree*

1. The interviewee was confident and composed. (Honest)
2. The interviewee was pleasant and friendly. (Honest)
3. The interviewee was engaged and involved. (Honest)
4. The interviewee gave plausible explanations. (Honest)
5. The interviewee avoided eye contact. (Deceptive)
6. The interviewee appeared hesitant and slow in proving answers. (Deceptive)
7. The interviewee conveyed uncertainty in their tone of voice. (Deceptive)
8. The interviewee excessively fidgeted with hand and foot movements. (Deceptive)
9. The interviewee appeared tense, nervous, and anxious. (Deceptive)
10. The interviewee portrayed an inconsistent demeanor over the course of the interaction. (Deceptive)
11. The interviewee conveyed uncertainty with their words. (Deceptive)

Appendix F

Lie Acceptability

Please respond to the following statements by selecting the value that you feel best represents your own opinion about lying in general. Response scale:

1 = *Strongly Disagree*, 7 = *Strongly Agree*

1. Lying is immoral. (R)
2. It is ok to lie in order to achieve one's goals.
3. There is no excuse for lying to someone else. (R)
4. Honesty is always the best policy. (R)
5. It is often better to lie than to hurt someone's feelings.
6. Lying is just wrong. (R)
7. Lying is no big deal.
8. There is nothing wrong with bending the truth now and then.

Appendix G

General Communicative Suspicion

Please respond to the following statements by selecting the value that you feel best represents your own opinion about other people in general. Response scale:

1 = *Strongly Disagree*, 7 = *Strongly Agree*

1. I often feel as if people aren't being completely truthful with me.
2. Most people only tell you what they think you want to hear.
3. When I am in a conversation with someone, I frequently wonder whether they are really telling me the truth.
4. People rarely tell you what they're really thinking.
5. When I first meet someone, I assume that they are probably lying to me about some things.
6. Most people are basically honest. (R)
7. Anyone who completely trusts someone else is asking for trouble.
8. When I ask a stranger for directions, I frequently wonder whether they are being truthful.
9. When I am talking to others, I tend to believe what they say. (R)
10. People seldom lie to me. (R)
11. Most people follow the saying "honesty is the best policy." (R)

Appendix H
Demographic Information

Please respond to the following demographic questions.

What is your age? _____ years

What is your gender? (Check one)
- Male
- Female
- Male to Female Transgender
- Female to Male Transgender
- Nonbinary
- Other (please specify)
- Prefer not to answer

What is the race/ethnicity with which you most closely identify? (Check one)
- Asian/Asian American
- Black/African American
- Latinx/Hispanic
- Native American
- Pacific Islander
- White/Caucasian
- Middle Eastern
- Other (specify)

Of what country (or countries) are you originally from?

Of what country (or countries) are you a citizen?

Do you currently reside within the United States?

- Yes
- No

What is your highest level of education earned?
- No formal education
- High school diploma
- Associate's Degree
- Vocational Training
- Bachelor's Degree
- Master's Degree
- Doctorate Degree
- Prefer not to answer