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Protection Motivation Theory in Information Security Behavior Research: Reconsidering the Fundamentals

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Protection Motivation Theory in Information Security Behavior Research: Reconsidering the Fundamentals

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Abstract:

Scholars commonly use protection motivation theory (PMT) by Rogers to examine information systems (IS) security behaviors and behavioral intentions. A recent influential paper by Boss, Galletta, Lowry, Moody, and Polak (2015; hereafter BGLMP) in *MIS Quarterly* outlines correct and incorrect uses of PMT in Information Security behavior research. In this paper, we review some of BGLMP's key recommendations, such as the claim that all IS behavior studies that apply PMT should always use the model of the full theory, contain and measure fear, and measure actual behaviors. We defend an interpretation of Rogers (1975, 1983) that differs from the interpretation that BGLMP propose. We present evidence that Rogers' PMT and the empirical evidence do not adequately support many of BGLMP's suggestions and that these suggestions contradict good scientific practices (e.g., restricting the use of the method of isolation) that the philosophy of science and the original literature on PMT uphold. As a result, if reviewers and editors continue to embrace these recommendations, they could hinder the progress of IS behavior research by not allowing isolation or the combination of different theoretical components. In contrast to BGLMP's paper, we argue that further PMT research can focus on isolated PMT components and combine them with other theories. Some of our ideas (e.g., isolation) are not PMT-specific and could be useful for IS research in general. In summary, we contest BGLMP's recommendations and offer revised recommendations in return.

Keywords: Protection Motivation Theory, Infosec Behavior, Behavioral Information Security, Security Threat, Threat Message, Fear Appeal, Threatening Communication.

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1 Introduction

Most cybersecurity studies in information systems (IS) explain or predict users' information security behaviors and intentions (Karjalainen et al., 2019; Vroom & von Solms, 2004), often applying theories from other disciplines, such as psychology (Vroom & von Solms, 2004) or criminology (Straub, 1991). One of the most frequently applied theories in IS security (infosec) behavior research is "protection motivation theory" (PMT) (Crossler et al., 2013; Johnston et al., 2015), first developed by Rogers (1975, 1983).

A recent paper in *MIS Quarterly* by Boss et al. (2015; hereafter BGLMP) claims that infosec behavior research scholars widely misapply Rogers' (1975, 1983) account of PMT. This paper has been influential in determining what counts as the "correct" application of PMT in infosec behavior research, and other scholars have echoed the authors' recommendations (e.g., Burns et al., 2017, p. 192; Menard et al., 2017, p. 1207)[1]. While we can credit BGLMP for introducing the fundamentals of PMT, valuable criticism can itself be unjustified or lead to improper judgments. For this reason, any criticism must also withstand scientific scrutiny[2].

In this commentary, we review the four key recommendations that BGLMP propose regarding the proper use of PMT in infosec research. Our reading of Rogers' writings on PMT and analysis of PMT in its original field (health psychology) leads us to contest BGLMP's main criticism and recommendations concerning PMT research in infosec. To this end, we defend an interpretation of Rogers that differs from the interpretation BGLMP propose. We support our interpretation with direct quotations from Rogers. We also explain why BGLMP's recommendations are sometimes at odds with good scientific practices, which the philosophy of science helps to outline.

A comparison between what BGLMP say about Rogers' conception of PMT and what Rogers himself says is required because some of BGLMP's recommendations risk promoting (1) certain misconceptions about PMT and (2) questionable scientific practices in infosec behavior research. As an example, BGLMP deem many existing applications of PMT in infosec behavior research incorrect because, for example, they do not measure fear or model it as a mediator. However, Rogers' conception of PMT explicitly rejects the role of fear as a mediator. Although not all infosec behavior scholars necessarily agree with BGLMP's recommendations, no infosec behavior study has explicitly challenged the ideas by BGLMP. As a result, these recommendations could hinder the progress of IS security behavior research if they remain unchallenged. To reduce the risk of IS scholars misunderstanding PMT in the future, a systematic critique and revision of BGLMP's recommendations is warranted. We offer such a critique and revision in this paper. Some of our revised, alternative recommendations are not PMT-specific and could be useful for IS research in general.

2 Reconsidering BGLMP's Recommendations

As we understand BGLMP, their main message can be summarized in four recommendations: infosec behavior researchers should (1) use what BGLMP call the "full nomology" of PMT; (2) manipulate, instead of measure, fear appeal; (3) measure fear and model it as a partial mediator; and (4) explain or predict behavior instead of only behavioral intentions. Moreover, BGLMP legislate one version of PMT as the "correct" version and identify certain research designs as preferred. The points BGLMP raise are worth discussing, and we credit the authors for raising these points. Regardless, and with the highest respect, we demonstrate that the justifications for these recommendations are problematic[3]. Throughout this paper, we provide direct quotations with page numbers to allow readers to double-check our claims about what Rogers (1975, 1983) says and what BGLMP say about Rogers' account of PMT.

[1] Out of the 271 *MIS Quarterly* research articles published from 2015-2020, BGLMP ranks at #6 in terms of the number of times other scholarly research has cited it over the past five years.

[2] In science, we often learn from our mistakes, and that is why we can speak clearly and sensibly about making progress. "Science is one of the very few human activities—perhaps the only one—in which errors are systematically criticized and fairly often, in time, corrected" (Popper, 1963, p. 293).

[3] Some infosec behavior papers discuss PMT and appeals to fear (Johnston et al., 2015; Wall & Buche, 2017; Wall & Warkentin, 2019). We cannot discuss so-called fear appeal research in this paper because it draws on several different theories and research streams. As a rough characterization, PMT is one theory in fear appeal research. We focus on PMT and on BGLMP's claims about the correct use of PMT.

2.1 Recommendation 1: "ISec PMT researchers should ideally use and establish the core or full nomology of PMT before adding non-PMT constructs" (p. 858)

BGLMP's first recommendation includes two points: (a) infosec behavior researchers should use only one version of PMT, and (b) infosec behavior researchers should always use the full theory (or, at least, its core parts) and should be careful when extending the theory. BGLMP also provide empirical support to support these claims. We discuss these arguments next.

2.1.1 "Full Nomology", "Core Nomology", and Rogers' Model of PMT

Figure 1, which we take from BGLMP paper, presents the foundation of BGLMP's recommendations: the "full nomology" and "core nomology" models.

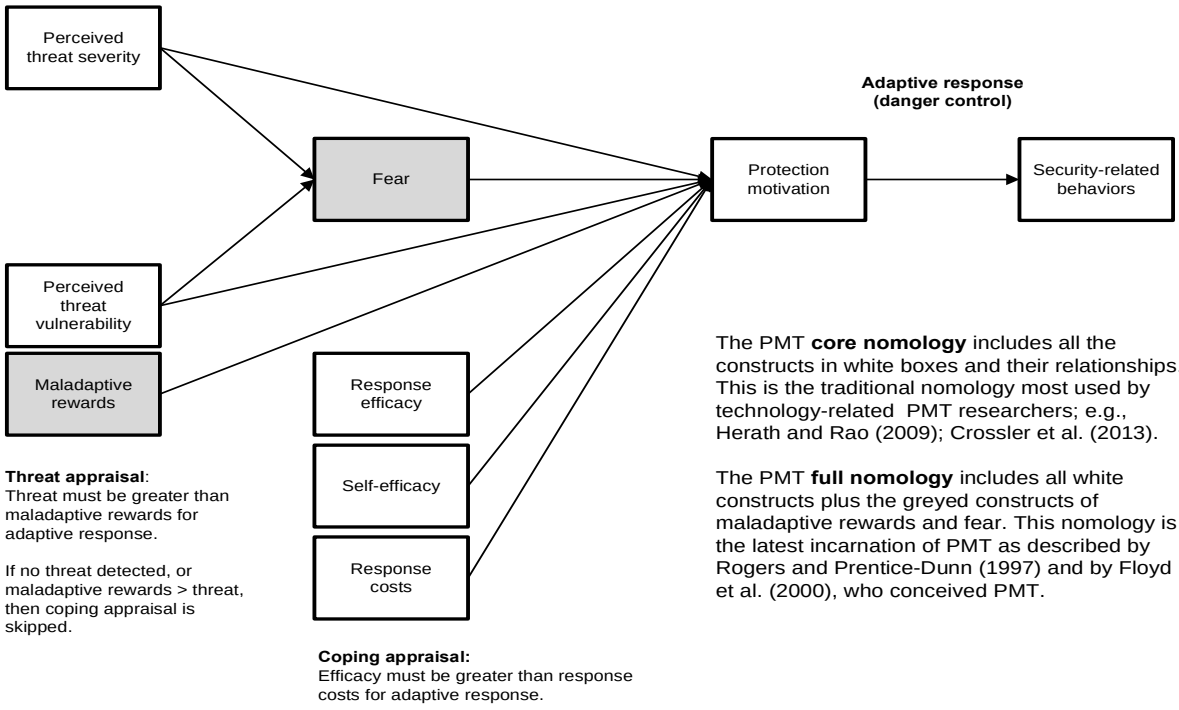

Figure 1. "Full Nomology" and "Core Nomology" in PMT According to BGLMP (Reproduced from BGLMP, Figure 1)

We start by comparing Figure 1 with two figures that Rogers and Prentice-Dunn (1997) present. Floyd et al. (2000) reprint those two figures, which contain the central reference BGLMP use to support their PMT model. We can combine these PMT figures (Floyd et al., 2000), as we illustrate in Figure 2.

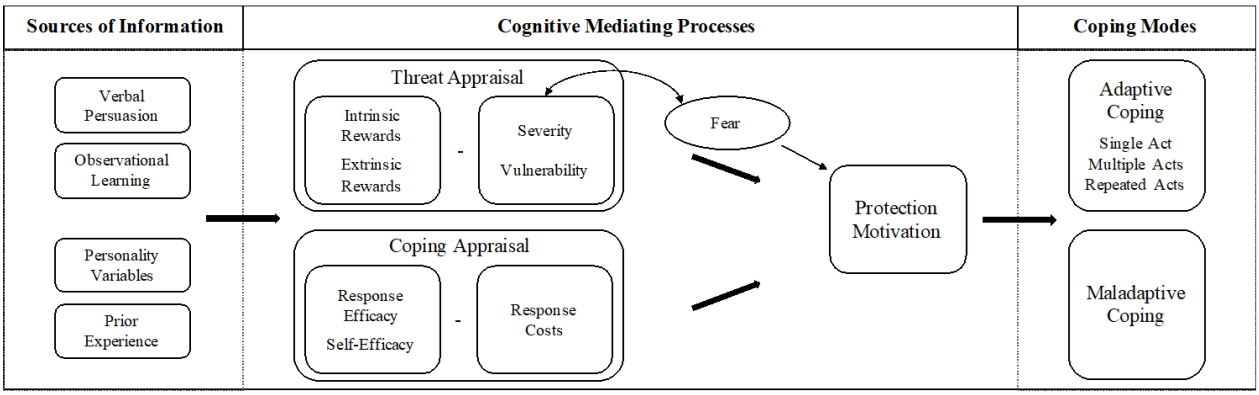

Figure 2. The Full Model of Protection Motivation Theory

According to Rogers, PMT comprises three parts: (1) "sources of information", which act as inputs; (2) a "cognitive mediating process" through which these inputs influence human behaviors; and (3) "coping modes" that illustrate these behaviors. The cognitive mediating process is further broken down into "threat appraisal" and "coping appraisal". Rogers addresses threat appraisal before evaluating coping options and then combines the two concepts to form "protection motivation". Protection motivation leads to "maladaptive" or "adaptive" coping, both of which can manifest in different forms. For example, Rogers differentiates "maintaining a protective behavior", "stopping an existing deleterious action", and "initiating a protective action" as different forms of coping modes (Floyd et al., 2000, p. 412). Although BGLMP label their model the "full PMT nomology", they only focus on the cognitive mediating process (BGLMP, p. 839) and omit sources of information and coping modes. Thus, BGLMP's "full nomology" does not represent what Rogers (Floyd et al., 2000, p. 410; Rogers & Prentice-Dunn, 1997, p. 114) refers to as the "Overall Model of Protection Motivation Theory"—which we present in Figure 2 of the present paper—because it contains only one of the three key parts of PMT. If we focus only on the cognitive mediating process of PMT, BGLMP's "full nomology" and "core nomology" models are similar but also different in important ways from the "Overall Model of Protection Motivation Theory" (Floyd et al., 2000, p. 410; Rogers & Prentice-Dunn, 1997, p. 114). BGLMP's "full nomology" merges "intrinsic rewards" and "extrinsic rewards" as "maladaptive rewards". It also includes fear as a partial mediator, which our Figure 2 does not include for a justified reason (see Section 2.3.1 and Footnote 16 for more detailed explanations of the role of fear in PMT and in our Figure 2). BGLMP's "core nomology" is a subset of the "full nomology", which omits fear and maladaptive rewards. BGLMP refer to the version depicted in their Figure 1 as "the third and latest version" (p. 842) of PMT, citing two studies by Rogers for support (Floyd et al., 2000; Rogers & Prentice-Dunn, 1997). However, both studies recognize only two versions of PMT, namely, the original (Rogers, 1975) and the revised (Rogers, 1983) versions. More specifically, Rogers and Prentice-Dunn (1997) explicitly state that the "revised version (Rogers, 1983)…will be the focus of this chapter" (p. 114), and Floyd et al.'s (2000, p. 412) meta-analysis was designed to test the 1983 version. Thus, the third version of PMT to which BGLMP refer does not exist, at least not based on the two key references they cite.

BGLMP also criticize how scholars use the original (Rogers, 1975) and revised (Rogers, 1983) versions of PMT in the infosec behavior literature. Specifically, BGLMP state:

> *Although the original version (Rogers 1975) was abandoned long ago, it is often incorrectly cited and used in ISec literature. A second version is closer to the current one, but omits some key changes related to fear, and thus is also often incorrectly used.* (p. 843)

We thank BGLMP for highlighting the contrast between Roger's 1975 and 1983 papers. Rogers has criticized a particular aspect of his 1975 version of PMT. Rogers (1975) initially expected a "multiplicative relation" between severity, probability of occurrence, and coping response efficacy (p. 99). However, as the empirical evidence did not support this speculated relation, he later suggested that the relation was additive. In Rogers' (1983, p. 158) own words, "The original multiplicative combination rule has been rejected". However, although Rogers rejects the multiplicative relation, he does not reject the entire 1975 version of the theory. In the same (1983) paper, Rogers concludes that "most of the empirical evidence supports the original theory of protection motivation" (p. 160). Thus, if infosec behavior researchers cite Rogers (1975), for example, to refer to the basic ideas of cognitive mediating processes for threat appraisal, these ideas are still valid, and the 1975 version should be considered an appropriate citation as the original source of the idea.

The lack of support for the original version of the multiplicative relation does not mean that there are no interactions among the PMT constructs. Although Rogers rejects the original form of the interaction, he also clarifies that the general idea of interacting processes appears to be correct: "The additive model holds within each appraisal process. When combining components between the two processes, second-order interaction effects occur" (Rogers, 1983, p. 170; Rogers & Prentice-Dunn, 1997, p. 177). Thus, the criticism that BGLMP (p. 858) level against Liang and Xue (2010) is partially incorrect because Rogers did not simply remove the original multiplicative effect; he replaced it with another kind of interaction. In fact, Liang and Xue's (2010) study is partially consistent with the new interaction. Nevertheless, the rejection of the original multiplicative relation in a non-infosec behavior context should not mean that infosec behavior scholars cannot study the relation in the infosec behavior context (cf. Liang & Xue, 2010).

2.1.2 "Full Nomology": Recommendations and Isolation

A key motive behind BGLMP's paper is the "incomplete treatment of the core and full nomology of constructs in PMT" in the IS literature (p. 856)[4]. We thank BGLMP for raising this issue. However, contrary to this recommendation, focusing on specific aspects of PMT is a common practice in PMT research (in psychology), especially at the level of an individual research paper. Even Rogers himself does not always use the full version of PMT[5], and most of the research Rogers and Prentice-Dunn (1997) reviewed, as well as the studies in Floyd et al.'s (2000) meta-analysis, focuses on testing only parts of the PMT model. This is also true in the more recent literature, which our review of recent applications of PMT in health psychology (Appendix A) indicates. These observations raise two issues. First, BGLMP cite Floyd et al.'s (2000) study, which is based on partial tests of PMT, as providing "strong support" (p. 839) for BGLMP's claim about full nomology, yet BGLMP reject the validity of such partial tests in the infosec behavior context. Unfortunately, BGLMP do not explain why partial tests of PMT are an undesirable research practice in the infosec behavior context, while at the same time, these practices, when applied in the health psychology context, provide "strong support" for the same theory.

Second, there is a good reason why scientific studies commonly focus on partial tests of a theory or specific aspects of a phenomenon while excluding or omitting others (Mäki, 2004). It is because they are following the "method of isolation" (Mäki, 1992, 2004). Although we cannot discuss all the different reasons for and benefits of using isolation in this paper, scientists use isolation in general because a phenomenon of interest is often too complex to be studied as a whole and all at once; the complexity may become manageable only if they omit some aspects of the phenomenon[6]. Many philosophers of science, such as Mäki (2004), claim that "the method of isolation is a ubiquitous method used in all scientific disciplines" (p. 321)[7]. Scholars have identified the frequent use of isolation as a feature that separates science from non-science (van Benthem, 2012, p. 784). Thus, it has become commonplace in the philosophy of science to note that virtually all models and theories omit some variables and relationships. For example, Wimsatt (2007, p. 96) notes, "*Any* model implicitly or explicitly makes simplifications, ignores variables, and simplifies or ignores interactions among the variables in the models and among possibly relevant variables not included in the model" (Wimsatt, 2007, p. 96).

Two examples from the social sciences can help demonstrate the utility of the method of isolation. The first example is a famous study of business management in which Collins (2001) studied 11 companies to discover what caused good companies to become great in terms of financial performance while others did not. The author tried to explain an important and comprehensive phenomenon: firms' financial performance. Although Collins' (2001) study produced a best-selling business book and consequently influenced the thinking of business managers, some may ask whether the study genuinely contributed to our understanding of why some companies perform better than others. Unfortunately, the answer seems to be "no" because most of the key findings—for instance, that it is important to invest in technologies that support the business or that good leadership matters—were too general to be useful (Walker, 2006). For example, not all companies that invest in technologies that support their business will enjoy financial success in return. In addition, one can challenge the causal order of the explanans (explanatory factors). For example, strong financial performance can enable good leadership or investment in technology (Niendorf & Beck, 2008). Placing limits on the explanatory factors (Mäki, 2004)—that is, practicing isolation—may have helped Collins avoid such issues.

A classic psychological study by Bandura et al. (1961) of the Bobo doll illustrates the second and alternative example. This study enquired into the causes of aggressive behavior in children. However, it did not set out to explain all or even the most important causes of aggressive behavior but only studied a very narrow question: To what degree did the children's observations of aggressive behavior influence their aggressive behavior toward a Bodo doll, and how did this effect vary between the sexes? The findings of the study, which scholars later replicated in other settings, made at least two fundamental

[4] Here, BGLMP's critique is twofold. First, "no published ISec PMT article can be classified as adhering fully to PMT" (BGLMP, p. 840). Second, "virtually every ISec study made major, unsupported adaptations to PMT" (BGLMP, p. 840).
[5] For example, the components of intrinsic and extrinsic rewards and the response costs in PMT emerged during the revision of the theory between 1975 and 1983. However, Rogers conducted a study on PMT in 1996 and did not measure these two factors in that study (Sturges & Rogers, 1996).
[6] For example, a "realistic" model in biochemistry would have several hundred complex, dynamic, reticulated parts (activities or entities) and connections between these parts (Love & Nathan, 2015; Thagard, 2003). However, no singular empirical study can examine all these parts at once in detail.
[7] "Every concept, model and theory is based on an isolation of a slice of the things and properties in the world to the exclusion of the rest of what there is" (Mäki, 2004, p. 321).

contributions to psychology (Hock, 2009, pp. 90-92). First, the study contributed to social learning theory by demonstrating how children can learn from observing adults. Second, the study has served as the foundation for hundreds of more recent studies on how violence shown in the media affects children. Most of these studies use the method of isolation, including those among the "forty studies that changed psychology" identified by Hock (2009).

To summarize, while BGLMP explain the cognitive mediating process part of PMT appropriately, the PMT literature in its home discipline (health psychology) does not support the requirement that infosec behavior researchers should always use the entire PMT model (the "full nomology" or the "core nomology"). In infosec behavior research, Aurigemma et al. (2019) suggest that whether one uses the full or core PMT model "depends partially on the research objective" (p. 11)[8], which seems reasonable. We add that infosec behavior scholars do not need to use either the "full" or "core" PMT model because, for instance, one study may focus on only one PMT component (e.g., threat appraisal) or whether security concerns even raise fear. Finally, when compared with the "Overall Model of Protection Motivation Theory" (Floyd et al., 2000, p. 410; Rogers & Prentice-Dunn, 1997, p. 114), BGLMP's version of a "full PMT nomology" is also isolated since it omits the sources of information (observational learning, personality variables, and previous experiences) and coping modes inherent to the original PMT model (Rogers & Prentice-Dunn, 1997, p. 114). Even if we return to the origin, Rogers himself often tested only parts of the PMT, as did Floyd et al. (2000) in their meta-analysis. Any model or theory omits some variables and relationships (Wimsatt, 2007, p. 96). This has recently been called idealization in IS (Iivari, 2023; Siponen & Klaavuniemi, 2021). Recent IS thinking also highlights how scientific progress commonly contains specialization, "in which seeking theoretically deeper or more accurate explanations or predictions has led to narrowing the scope of a model" (Siponen et al., 2023, p. 1; see also Craver, 2009; Iivari, 2023). Aiming at such specificity often requires focusing on some components and ignoring others (Iivari, 2023).

2.1.3 The Claim About Non-PMT Constructs and PMT Spinoffs

Although many infosec behavior studies combine PMT components with other theories, BGLMP criticize this practice: "Adding non-PMT constructs to PMT models or creating PMT spinoff models can…distance the resulting model from PMT in ways that are not theoretically justified" (p. 841). Here, BGLMP also raise an important debate regarding whether combining constructs is acceptable and what counts as "theoretically justified". We analyze this claim from two perspectives: (a) whether Rogers' original research, which BGLMP at least implicitly endorse, combines constructs with PMT, and (b) whether infosec behavior researchers who have added such constructs have presented adequate theoretical justifications for the additional constructs.

Although BGLMP criticize such a practice, adding new components to PMT has been a common and accepted practice in its home discipline (psychology, or health psychology) ever since Rogers published the original PMT (Rogers, 1975)[9]. For example, Milne et al. (2002) have augmented PMT with "volitional intervention", which is a factor placed after the "protection motivation" that targets the long-term maintenance of coping behaviors. More recently, Wong et al. (2016) have proposed a sedentary PMT variant that includes goal and implementation intentions, and Mullan et al. (2016) have introduced four versions of PMT that emphasize different variables of the original PMT model to explain four types of food-handling intentions not covered in PMT (Rogers & Prentice-Dunn, 1997, p. 114). Even what Rogers and Prentice-Dunn (1997, p. 114) refer to as the overall model of protection motivation theory came into being with many revisions and extensions relative to the 1975 version of PMT. Rogers himself added components to it—for instance, "self-efficacy", which originally stemmed from Bandura's (1977) theory of behavioral change (Rogers, 1983). Finally, PMT itself was inspired by expectancy-value theory (Rogers,

[8] "Even though the difference in variance explained between our core and full PMT models may suggest that future research should always build off the full PMT nomology, we do not make that recommendation. Which version of the PMT is theoretically justified depends partially on the research objective" (Aurigemma et al., 2019, p. 11). We believe that for Aurigemma et al. (2019, p. 11), "which version" refers to either the "core" or "full" models.

[9] Some studies that appear to be PMT extensions are not. For example, Cummings et al. (2018) add many factors to the PMT framework, including "general scientific knowledge", "media attention", and "trust". Although these factors do not sound like PMT and none of the names are shown in the PMT diagram, they are still viable "sources of information" (see Figure 2), and thus, are consistent with Rogers' (1983) PMT.

1975), but there are no claims in PMT's home discipline (health psychology) that PMT is a theoretically unjustified spinoff of that theory[10].

Deciding whether infosec behavior researchers have added new constructs without theoretical justification depends on what we count as "theoretical justification". A naïve definition of "theoretical" would require that a construct appears in a specifically named original theory applied in the relevant study. However, this definition is problematic because it would, for example, render the 1983 modification of PMT theoretically unjustified, given that this version included elements not included in the original 1975 iteration (Rogers, 1983). More generally, adopting this standard would prevent scholars from modifying existing theories because a modification, by definition, cannot exist in the original version of the theory.

A more generous characterization of "theoretical justification" is that a study provides a plausible explanation for "how", "why", or "when" questions concerning the theorized relation (Bacharach, 1989). Such explanations seem to be common in IS infosec behavior research, and it would be difficult to publish a study in the basket of IS journals that did not provide an explanation for the statistical hypotheses tested. For example, BGLMP criticize Lee et al. (2008) for using "prior experience" as a "non-PMT" construct. However, it is not clear why Lee et al.'s (2008) explanation that "by experiencing the frustrating and annoying virus or worm infection, Internet users become aware of the seriousness of and their vulnerability to virus threats and, eventually, they come to realize that appropriate virus protection behaviors are needed" (p. 448) is not an adequate theoretical explanation. Moreover, the claim that "Lee et al. added 'prior experience' outside of PMT" (p. 856) is factually incorrect because prior experience is already a part of the full PMT (see Figure 2).

Similarly, BGLMP criticize a study by Johnston and Warkentin (2010) for not providing theoretical justification. Quoting Venkatesh and Davis (2000), Johnston and Warkentin (2010) explain that:

> *People may choose to perform a behavior, even if they are not themselves favorable toward the behavior or its consequences, if they believe one or more important referents think they should, and they are sufficiently motivated to comply with the referents.* (p. 554)

We find this to be one justified theoretical explanation for the role of social influence in influencing behavioral intent. Even if one agrees that providing theoretical justifications for a tested model is important for infosec behavior research, it seems that PMT studies generally do provide such justifications, which renders BGLMP's spinoff claim much weaker—if not invalid.

2.1.4 Empirical Evidence: Claims About Model Fit and Explanatory Power

With good intentions and an admirable goal of improving the theory, BGLMP (pp. 840-841, 858) not only recommend the use of a "core nomology" but also state that any PMT extensions should be compared against the "core nomology" model in terms of (a) explanatory power and (b) model fit[11]. They also exemplarily evaluate their model against alternative models built in accordance with previously published, PMT-based infosec behavior research on these two metrics. This comparison forms the empirical support for BGLMP's claim that infosec behavior researchers should use the "full PMT nomology" in their models (BGLMP, pp. 853-855). We thank BGLMP for starting a discussion on how scholars should evaluate models empirically. However, there are substantial problems with the general principle of using these benchmarks to compare models and with how BGLMP implement the comparisons. Addressing these problems is important since they form the empirical support for BGLMP's "full nomology" and "core nomology" recommendations, which could otherwise make their conceptual arguments persuasive to some readers.

We start by adducing two reasons for the statistical claim that the explanatory power of a model (the R^2 statistic) is not the ultimate yardstick of the goodness of a statistical study. First, if a theory has two elements—say, the coping appraisal and threat appraisal processes—then a statistical model with

[10] Rogers and Prentice-Dunn (1997, p. 127) discuss extensions to the theory. They regard the theory of planned behavior (Maddux, 1993), the extended parallel process model (Witte, 1992), and ordered protection motivation theory (Tanner et al., 1991) as elaborations or revisions of PMT rather than spinoffs of PMT or non-PMT studies.

[11] "A key issue is that virtually every ISec study made major, unsupported adaptations to PMT without (a) testing the core PMT nomology or (b) demonstrating that its changes actually improve the explanatory power of PMT or that the alternative model it developed enjoys better model fit than PMT" (BGLMP, p. 840). In addition, "This limitation occurs because the studies do not provide the model-fit statistics required to demonstrate that an extended model improves on a baseline model. By using at least the core, established PMT nomology fully, ISec researchers may be able to increase the explanatory power of their models and may find that non-PMT additions are neither helpful nor necessary" (BGLMP, p. 841).

variables for both elements will make better predictions (concerning the sample) than a simpler model that contains a variable for only one of the elements. Thus, relying on explanatory power as a criterion for what to include in the model would prevent us from using the method of isolation to gain relevant results for practical and scientific work. Strictly applying this principle would also prevent all studies from using PMT as the fit model because a broader theory that encompasses PMT, such as the unified model of information security policy compliance (Moody et al., 2018), found in infosec behavior research, or Rogers and Prentice-Dunn's (1997) proposed PMT extensions, would generally predict intentions better than PMT. Applying the explanatory power rule in this case would mean that PMT should never be used without these extensions. Second, focusing on R^2 can lead scholars to include irrelevant variables. For example, advanced age strongly predicts death; thus, including age as a predictor would improve the explanatory power of any model that includes death as a dependent variable. But this does not mean that every medical trial of a life-saving medication should include age as an explanatory variable in a regression model. Whether a person dies due to advanced age is, in most cases, unrelated to the effectiveness of a medication and thus falls beyond the scope of these studies. Indeed, methodological writings make it clear that maximizing R^2 by including more explanatory variables is generally inadvisable (Allison, 1999, p. 31; Schisterman et al., 2009; Singleton & Straits, 2009, p. 556; Wooldridge, 2013, pp. 205-206). These concerns are succinctly summarized by Moksony (1999): R^2 "is quite immaterial given that our intention is to establish a particular causal relationship, not to prepare a full list of the various causes of a phenomenon" (p. 3).

Model fit is a poor criterion for judging whether one should add variables to a model. The values of the most common statistics (e.g., Root Mean Square Error of Approximation (RMSEA), Comparative Fit Index (CFI), and Tucker-Lewis Index (TLI)) depend on the number of observed variables, which is a well-known problem in the Structural Equation Modeling literature called the "model size effect" (Kenny & McCoach, 2003; Moshagen, 2012; Shi et al., 2019). This effect refers to the tendency of the T_{ML} statistic, which is a crucial basis for other common statistics (e.g., RMSEA, CFI, and TLI), to overreject correct models that consist of many observed variables in moderate or small samples due to an inflated Type I error rate (Moshagen, 2012; Shi et al., 2019). Thus, when comparing models of different sizes, it is impossible to judge whether a difference in the model fit reflects differences in the degree of misspecification or simply differences in model sizes.

Given the general problems with using R^2 or model fit as criteria for deciding when variables should be included in a model, and in light of the specific problems in BGLMP's empirical analysis (see Appendix B), we find it difficult to see how the empirical data BGLMP offer support their conceptual claim that there is one correct version of PMT and that scholars should always use this "correct" model as a reference point in the infosec behavior context.

2.2 Recommendation 2: "ISec PMT researchers should ideally use fear appeal manipulations when conducting security-related PMT studies" (p. 858)

We strongly support experimental studies in which the researchers can manipulate the type of fear appeal or its level because, through such studies, the researchers can directly assess the effect of an intervention (Siponen & Baskerville, 2018). However, we contest BGLMP's statement that researchers must include fear manipulation[12]. The main problem with this recommendation is that restricting the use of PMT to only these experimental studies would rule out many interesting research questions and useful research designs. Moreover, the original PMT research to which BGLMP refer does not support this recommendation because, in that research, the authors already justified the use of stimuli other than fear appeal manipulation (Floyd et al., 2000; Rogers, 1983; Rogers & Prentice-Dunn, 1997). We demonstrate this below.

2.2.1 On the Role of Manipulated Fear Appeals in PMT Studies

Rogers originally designed PMT to explain the effects of fear appeals, but the theory soon evolved to be applicable to "any situation involving threat" (Rogers, 1983, p. 172). The revised version of PMT (Floyd et al., 2000; Rogers & Prentice-Dunn, 1997) included sources of information about the threat in addition to the experimental presentation of fear appeals (Figure 2). These sources of information could include (a)

[12] Specifically, BGLMP state that "proper fear-appeal manipulations are a core assumption of proper PMT use" (p. 858) and that "not using fear-appeal manipulations violates PMT and causes potentially spurious and misleading results that undermine the established PMT nomology" (p. 858).

verbal persuasion (e.g., "fear appeals"), (b) observational learning (e.g., observations of how a risk affects others), (c) personality (e.g., a person with strong general anxiety), or (d) previous experience (e.g., feedback from experiences with a risk) (Floyd et al., 2000, p. 409; Rogers, 1983, p. 167; Rogers & Prentice-Dunn, 1997, p. 114). Given that many of these sources of information extend beyond simple fear appeals and contain elements that cannot be subjected to experimental manipulation (e.g., personality), the requirement that studies applying PMT should always manipulate fear appeals is not consistent with Rogers' works. For this reason, Rogers and Prentice-Dunn (1997, p. 114) acknowledge that PMT may be used in static studies (e.g., survey studies).

2.2.2 Methodological Concerns About Discouraging Non-Experimental Studies

Beyond the fact that some sources of information in PMT cannot be manipulated, requiring that these variables should, when possible, be studied only through manipulation is methodologically problematic. First, even if fear appeals were an integral part of PMT, the method of isolation allows researchers to focus only on parts of a theory. For example, Rogers and Prentice-Dunn (1997) and Floyd et al. (2000) reviewed many studies that examined only some parts of the mediating cognitive process and whether it influences behavioral outcomes without measuring or manipulating the fear appeals that may have triggered this process. Furthermore, as Rogers and Prentice-Dunn (1997, p. 114) themselves suggest, while experimentally induced cognitions may involve all the variables in the cognitive mediation framework of PMT, some of them may not be active in static studies (e.g., survey-based studies). Second, experimental studies often force researchers to use artificial elements, and the generalizability to real-world settings can be questionable (Singleton & Straits, 2009, pp. 449-452). For example, in Study 2 of BGLMP, the students were recruited for an experiment, and the students thus knew that the researchers were manipulating their behavior. One might expect the students to react very differently if they received a real virus warning during their normal, daily computer use. Requiring that a researcher manipulate fear appeals also rules out the use of quasi-experiments (Singleton & Straits, 2009, Chapter 8) in which the researcher does not manipulate fear appeals but nevertheless identifies a clear, exogenous source of variance.

To summarize, not every research question should or can be studied with experiments, and experimental settings can undermine ecological validity. Requiring that every study manipulate fear appeals is likelier to impede, rather than facilitate, the progress of infosec behavior research. While we agree that experimental studies that manipulate fear appeal are useful, they cannot be the only acceptable type of PMT study.

2.3 Recommendation 3: "ISec PMT researchers should measure fear when conducting security-related PMT studies" (p. 859)

BGLMP's recommendation to study fear is based on the strong assertion that "the third and latest version extended PMT to more *strongly emphasize* maladaptive rewards and reinstated fear as an *important partial mediator* (Floyd et al. 2000; Rogers and Prentice-Dunn 1997)" (BGLMP, p. 842)[13]. As discussed, notwithstanding the fact that a third version of PMT does not seem to exist, neither of the papers cited identify fear as a partial mediator or strongly emphasize this factor. Instead, the papers state the exact opposite: "Fear plays only an indirect role" (Rogers & Prentice-Dunn, 1997, p. 115)[14]. And again, "[The original PMT] asserts that attitude change is not mediated by or a result of an emotional state of fear" (Rogers, 1983, p. 158).

2.3.1 The Role of Fear in PMT

Some might infer that the way fear appears in graphical representations of the revised version of PMT (Rogers, 1983, Figure 6-2) implies that fear is a key part of PMT, but this is not the case (Rogers, 1983, p. 169). To understand what Rogers meant with the symbol for fear in his figure, consider the 1997 paper's only mention of fear's role in PMT:

[13] Moreover, BGLMP (2015) claim that these studies provide evidence of the role of fear in PMT: "Existing non-ISec PMT research has supported fear as a key partial mediator in PMT (e.g., Floyd et al., 2000; Rogers & Prentice-Dunn, 1997)" (p. 838).

[14] Similarly, Floyd et al. (2000) did not collect data on any relations involving fear (e.g., Table 1 in Floyd et al., 2000), and thus, did not provide support for the claim that fear is an important mediator in PMT. Moreover, Rogers and Prentice-Dunn (1997) list severity, vulnerability, response efficacy, and self-efficacy—but not fear—as the central variables in PMT (p. 118). Additionally, although Rogers and Prentice-Dunn (1997) list several research opportunities related to PMT, they do not mention fear as an opportunity (p. 128). Clearly, the works by Rogers and his coauthors and Floyd et al. (2000) do not support BGLMP's claim that fear has a central role.

> *Fear plays only an indirect role in threat appraisal. Research reviewed by Rogers (1983) found that fear influences attitudes and behavior change, not directly, but indirectly by affecting the appraisal of the severity of the danger. Rippetoe and Rogers (1987) discovered that fear can have an indirect and detrimental effect on attitude change by inducing maladaptive coping, specifically defensive avoidance.* (p. 115)

This paragraph directly contradicts BGLMP's claim that "fear increases protection motivation" (p. 843) by saying that increased levels of fear make certain protective behaviors *less likely*. Thus, although Figure 2 contains fear as a construct, the correct interpretation of that figure, we proposed, is not that fear is a partial mediator but that fear may inhibit the cognitive processes that PMT explains[15]. Given that neither BGLMP nor the PMT references they cite explain how or why fear would mediate the effects of threat appraisal, we must conclude that BGLMP's claim about mediation lacks theoretical justification in PMT.

To better understand the relation between PMT and fear, it is worth reviewing some of the history of PMT. Before PMT, most theorizing of fear appeals centered on the idea that fear appeals cause fear, which then causes action (Rogers, 1975, pp. 95-96). Thus, scholars considered fear the key mediating variable between fear appeals and action (Rogers, 1975, pp. 95-96). However, as Rogers (1983) explains, the empirical evidence did not support this explanation, which led to proposals of alternative theories. The most prominent of these theories was Leventhal's (1970) parallel response model, which posits that people respond to a fear appeal via two processes: through a fear control process, they seek to reduce the emotion of fear, and through a danger control process, which is a rational process, they seek to protect themselves from the threat. These two processes are independent and do not cause each other (Leventhal, 1970, p. 124). Rogers proposed PMT to explain how the danger-control process works (Rogers, 1983, p. 157). Thus, Rogers originally proposed PMT to provide an alternative explanation for how fear appeals produce changes in behavior that do not require fear as a mediator:

> *The proposed formulation asserts that attitude change is not mediated by or a result of an emotional state of fear, but rather is a function of the amount of protective motivation aroused by the cognitive appraisal processes. The emphasis is thus upon cognitive processes and protection motivation, rather than fear as an emotion.* (Rogers, 1975, p. 100)

PMT usually refers to coping behaviors that people do not perform immediately but rather through sustained cognitive processes, such as taking medication (Rogers, 1983, p. 166). According to PMT, emotions such as fear are necessary only if immediate physical responses are needed to avert a threat (Rogers, 1983, p. 166). This grants a certain importance to fear in some risk-avoidance situations, but if we assume that infosec behavior research should also explain or predict long-term or repeated behaviors, fear may not be a particularly relevant concept for infosec behavior studies.

2.3.2 Knowing the Mechanism is Not Necessary to Study the Effect

Although BGLMP's claim that "no ISec PMT-related research has measured fear to examine the efficacy of a manipulated fear appeal" (p. 838) may be true, many readers may be led to think that researchers must measure mediating variables to study the success of a fear appeal in provoking a protection motivation, or ultimately, behavior. But researchers do not always need to perform such a measurement. We can assess the effectiveness of a fear or threat appeal without measuring fear (even if the effect would be fully mediated by fear). This point is not PMT-specific. For example, if you hold a pen in your hand, you can open your hand and observe the pen falling. You can then infer that opening your hand caused the pen to fall, even if you do not know anything about the theories of physics. In fact, numerous medical treatments and drugs are in use even though we do not fully understand their underlying mechanisms (Siponen & Baskerville, 2018)[16]. Thus, identifying causal effects does not necessarily require understanding the underlying mechanisms (Singleton & Straits, 2018). Philosophers of science widely acknowledge this point (Ylikoski, 2013). For instance, a former editor-in-chief of *Philosophy of Science*

[15] We note that even if one could interpret Figure 2 independently of Rogers' claim that PMT is simply a path diagram for a structural equation model, the correct interpretation of fear would be that it is simply correlated (the two-headed curved arrow) with the threat appraisal process and can have a causal effect on protection motivation. However, the diagram itself indicates nothing about the direction of these effects.

[16] For example, medical professionals regularly use the medication propofol to initiate and maintain general anesthesia. While researchers discovered this anesthetic effect accidentally, medical professionals have used the drug routinely for decades. However, although we have strong evidence that propofol has a strong sedative effect on humans, the exact mechanism remains unknown (Antonakis, 2017). The so-called scientific method (Hypothetico-Deductive) also emphasizes results (Siponen & Klaavuniemi, 2020).

notes that scholars can make useful predictions in social science with "no understanding of the situation" (Salmon, 1989, p. 407).

If researchers are aiming for practical applicability, then the important questions concern whether and under which conditions the effect works—not how it works (Siponen & Baskerville, 2018)[17]. Researchers should especially remember this distinction when measuring fear may hinder the implementation or measurement of other variables. For instance, although a certain security threat may not naturally provoke the experience of fear, the intentional measurement of fear, by itself, can provoke an individual to consciously evaluate their fear, and this fear may affect the evaluation of other decision-making factors, which may lead to biased results. Researchers should not decide to include fear in research practices a priori; this decision should be considered in concert with other issues (see Section 2.2.2), as we have suggested. We share BGLMP's claim that "the goal is to change security behaviors, not just to increase protection motivation" (p. 844). For this purpose, measuring possible mediators is not necessary, although it can be useful (Singleton & Straits, 2009, pp. 23-29).

2.3.3 On Measuring Fear

BGLMP presented two new scales for measuring fear based on previous research. The first scale comprises four items based on Milne et al.'s (2002) work: "I am worried/frightened/anxious/scared about the prospect of losing data from my computer" (BGLMP, p. A9). The second scale comprises six items based on Osman et al.'s (1994) work: "My computer has a serious malware problem", "My computer might be seriously infected with malware", "My computer might become unusable due to malware", "The amount of malware on my computer is terrifying", "I am afraid of malware", and "My computer might become slower due to malware" (BGLMP, p. A10).

We thank BGLMP for offering these scales to measure fear. However, we respectfully contest the degree to which extent these scales measure fear. Although Milne et al. (2002) labeled their scale "fear", they did not justify the scale or provide any evidence that the scale is a valid measure of fear; the items appear to be more closely related to the concepts of worry or concern (McCaul & Mullens, 2008) than fear. In contrast, several studies have validated the pain anxiety symptoms scale Osman et al. (1994) proposed. However, the actual items BGLMP used—which include "I have a serious medical condition", "I might be seriously ill", "Sensations are terrifying", "Afraid of dying", and "I might become paralyzed" (see Osman et al., 1994)—are not the original items. It is questionable whether the scale validity evidence of the original scale (Osman et al., 1994) transfer to BGLMP's questions. Finally, a more careful look at the items BGLMP used raises the question of whether these statements reflect malware vulnerability or concern about the adverse consequences of malware rather than fear (consider, e.g., "Afraid of dying" versus "I am afraid of malware").

If we consider BGLMP's measurements of fear, it becomes obvious that the fear they measured has a different nature from what Maddux and Rogers (1983) measured via self-reports (namely, six adjectives for moods: fright, tension, nervousness, anxiety, discomfort, and nausea) or what Rogers (1983) noted as physiological indicators (e.g., skin conductance and heart rate), both of which emphasized health risks' emotional effects on people in relation to bodily dimensions. In infosec behavior research, the only work that directly measured physiological reactions to fear appeals applied neuroscientific functional magnetic resonance imaging (fMRI) methods and suggested that exposure to information security threat messages does not invoke fear in terms of brain activation in the amygdala and the limbic regions (Warkentin et al., 2016). Again, while we do not claim that fear does or does not exist in the fear appeal research phenomena, we propose that researchers need not always measure it, as it does not seem to be a crucial influence on protection motivation in the case of information security threats or health psychology (Section 2.3.1). And even if it is crucial, researchers should design, develop, and validate its measures carefully before use.

[17] Consider a randomized experiment in which researchers asked two groups of users to change their passwords. The researchers made this request via emails that were either neutral or contained an appeal to fear. In the group that received the fear appeal, 30% of the users changed their passwords, compared to just 15% in the neutral message condition (Siponen & Baskerville, 2018). Because of the experimental design, we can claim that the fear appeal in the message made it twice as effective. This result is important because of the clear policy implication, even if we do not fully understand why the fear appeal message was more effective.

2.4 Recommendation 4: "ISec PMT researchers should ideally model and measure behaviors, not intentions" (p. 859)

BGLMP insist that studies using PMT *must* measure actual behaviors and not just intentions[18]. While we agree that general infosec studies may have more practical value if researchers measure behaviors, Rogers (1983) recommended the opposite: "Protection motivation is best measured by behavioral intention" (p. 172). We discuss this conflict in detail in the following subsections.

2.4.1 On the Leading PMT-Research Argument

BGLMP justify the claim that PMT-based infosec behavior research must always measure behaviors as follows: "Leading PMT-based health research examines actual behavioral change, not just intentions (e.g., Fry & Prentice-Dunn 2006; Milne et al. 2000)" (BGLMP, p. 844). However, the two core citations do not support this claim. Fry and Prentice-Dunn's (2006) experiment measured the self-reported behavior of performing breast self-examinations and not the actual behavior. In Milne et al.'s (2000) meta-analysis, 40.7% of the studies measured only intentions and not behavior[19]. It is difficult to interpret these studies as evidence that leading PMT-based health research examines actual behavior and not just intentions.

2.4.2 Why Did the Original PMT Focus on Intentions?

BGLMP also argue "that actual behaviors are useful for ISec research because the goal is to change security behaviors, not just to increase protection motivation" (p. 844). Again, while we share the practical concern here, it is not clear how this goal differs from the goal of health psychology research, where the ultimate objective is also to change people's health behavior. Therefore, we see no reason why infosec behavior researchers should use PMT differently from health psychology researchers, who have commonly studied intentions (Rogers & Prentice-Dunn, 1997).

The focus on intentions in Rogers' PMT has certain important implications. Our interpretation is as follows. PMT is named "protection motivation theory" and not "protection behavior theory", indicating that it is not constructed as a theory of actual behavior. The intention to engage in protective behaviors is a more direct and better measurement of motivation, as Rogers himself suggested (1997, p. 116; 1983, p. 470; 1975, p. 98). In contrast, actual behavior may be affected by factors other than motivation, such as action instructions or coping possibilities (Schwarzer, 2008). It is important to separate two different questions: (a) What is the empirical support for the protection motivation theory, and (b) Does motivation translate into behavior? Regarding the first question, Rogers' (1983, p. 172) view ("protection motivation is best measured by behavioral intention") is reasonable. However, regarding the second question, namely, "to what extent does motivation or intention translate into behavior," there seem to be better models and theories than PMT, such as the health action process approach (Schwarzer, 2008).

3 Discussion

BGLMP have drawn much attention for situating PMT in infosec research, and their general concern for improved scientific thinking is important. We must credit the authors for such merits. Regardless, our comprehensive study of PMT leads us to contest some of BGLMP's recommendations. One of their key concerns is that existing PMT studies in infosec behavior research "deviate dramatically from PMT" (p. 838). BGLMP's recommendations for remedying the situation center on the proposed role of fear as a partial mediator between threat appraisal and protection motivation. With the greatest respect, this idea harbors a misunderstanding of Rogers' (1983) PMT, which argues that:

> *Attitude change is not mediated by or a result of an emotional state of fear, but rather is a function of the amount of protective motivation aroused by the cognitive appraisal processes. The emphasis is thus on cognitive processes and protection motivation, rather than on fear as an emotion.* (p. 158)

[18] A "PMT model must be characterized by the following properties and assumptions…. In addition to intentions, it measures actual protective behaviors as a more complete test of the efficacy of PMT" (p. 841).

[19] In Floyd et al. (2000), 41.5% of the studies measured only intentions. In both meta-analyses, the studies that measured intentions were either more common or about as common as studies that measured only behavior (14.8% vs. 33.9%) or both intentions and behavior (44.4% vs. 24.6%).

We point out that the key studies (Floyd et al., 2000; Rogers & Prentice-Dunn, 1997) BGLMP use to support the role of fear as a mediator between the threat appraisal process and protective motivation do not present evidence supporting this role of fear. In fact, a more detailed reading yields counterevidence for this claim. For example, Rogers and Prentice-Dunn (1997, p. 115) state, "Fear plays only an indirect role in threat appraisal", and high levels of fear are likely to make protective motivations less effective because they induce maladaptive coping. This seems consistent with the convex relationship between perceived threats and protection motivations proposed in technology threat-avoidance theory (Liang & Xue, 2010). We also point out several methodological issues that undermine the empirical support for the claim that fear is an important mediator in PMT.

In addition to the central but unsubstantiated recommendation that researchers include fear as part of any PMT study in infosec behavior research, BGLMP make three other recommendations. Two of these recommendations—the manipulation of fear appeals (Recommendation 4) and the measurement of actual behavior (Recommendation 2)—are valuable in combination because they allow researchers to directly assess intervention effect rates (Siponen & Baskerville, 2018). The more relevant question, however, is whether the group in the high fear appeal condition differs from the group in the low fear appeal condition in the propensity to use the simulated antivirus software. Table 3 in BGLMP (p. 848) suggests that the answer is "no"; the 0.02 difference in the means of the groups is neither practically meaningful nor statistically significant[20]. Why did the study not demonstrate the intended effect? This is an interesting question that we can study further by focusing on whether the manipulation failed to affect intentions, which PMT is well-suited to do. Alternatively, if intentions were affected but did not materialize in behavior, another theory would be more suitable.

Many complications arise in studying the full process from sources of information that form a fear appeal or a threatening situation that triggers a cognitive mediating process, in which the threat appraisal and combined appraisal processes interact to induce a protection motivation (which, ultimately, may influence actual behavior). To clearly establish the causal order of this process, one must temporally separate these stages (Kline, 2015) and possibly introduce experimental manipulation at multiple stages to ensure internal validity. Given that this may not be achievable in a single study, a researcher faces the challenge of conducting a comprehensive study that tackles every aspect of the phenomenon—but only by sacrificing rigor. The method of isolation (Mäki, 1992, 2004), according to which scientists focus on only a part of a phenomenon at a time, presents a much more attractive alternative. Therefore, most PMT studies in its home discipline (health psychology) in our sample (Appendix A) use isolation and omit some constructs.

We highlight two reasons why isolation—that is, only focusing on some variables of interest—is important. First, isolation helps us manage complexity by focusing on delimited aspects and omitting others (Mäki, 2004). Second, specialization is an important part of scientific progress. Specializing in a certain aspect—for instance, in the effects of perceived vulnerability, perceived severity, response efficacy, and self-efficacy on intention—can benefit some infosec behavior scholars (Iivari, 2023; Siponen et al., 2023). Such specialization is often necessary for increasing theoretical depth in terms of explanatory or predictive accuracy (Iivari, 2023; Siponen et al., 2023). In this way, infosec behavior research can deepen scholars' understanding of diverse phenomena. Without isolation, theoretical specialization becomes impossible. When infosec behavior research progresses in terms of depth (e.g., explanatory or prediction accuracy) (Iivari, 2023; Siponen et al., 2023), we run into situations where we lack the space to explain the omissions, even if we want to explain them. In fact, "Isolations can be brought about by simply omitting items without mentioning them. Omissions are characterized by silence about the excluded items" (Mäki, 2004, p. 322).

We summarize our recommendations for PMT infosec behavior research in Table 1.

[20] Substituting the statistics from Table 3 in the equation for the independent samples t-test (StataCorp, 2017, p. 2900) yields $t = \frac{0.40-0.38}{\sqrt{{}^{0.49^2}/_{130}+{}^{0.49^2}/_{142}}} = 0.34$, which corresponds to the two-tailed p value of 0.74. This can be verified with the following Stata command: `ttesti 130 0.40 0.49 142 0.38 0.49, unequal`. It is unclear how BGLMP calculated the z statistic of 3.97 with the same data and using the same software.

Table 1. BGLMP's Recommendations Versus Our Recommendations

BGLMP's Recommendations	Our Respective Recommendations
Recommendation 1: "ISec PMT researchers should ideally use and establish the core or full nomology of PMT before adding non-PMT constructs" (p. 858).	
Researchers should use the third and latest version of PMT and avoid citing the 1975 and 1983 versions.	• Rogers and the PMT literature in health psychology recognize only two versions of PMT: the original 1975 version and the revised 1983 version. • Although the original multiplicative combination rule did not receive empirical support, empirical studies have supported other elements of the 1975 PMT version. • If a researcher refers to an idea introduced in the 1975 version, that version should be cited, even if scholars have rejected some parts of it.
Studies should always include all constructs of a theory.	• ISec studies can focus on certain components of PMT and omit some others in an individual study. For example, ISec scholars can specialize in certain aspects (e.g., threat appraisal) or study the role of fear to deepen our understanding of that aspect. Of course, isolation does not mean that any study setting is acceptable.
ISec researchers should avoid adding non-PMT constructs to PMT models without theoretical justification.	• ISec researchers should explain what role new constructs play in their model and why (if possible); ISec studies that apply PMT seem to do this already.
SEM model fit and explanatory power (R^2) should be used as criteria for including elements in a model.	• Model-building decisions are primarily based on theory. • The explanatory power of a model (the R^2 statistic) is not the ultimate yardstick for the goodness of a statistical study. SEM model fit is an invalid criterion for judging whether variables should be added to the model. Researchers should compare models based on proper tests (e.g., for nested models, the χ^2 test, and for non-nested models, certain information criterion indices, such as Akaike information criterion; Kline, 2011, Chapter 8) instead of comparing model fit statistics.
Recommendation 2: "ISec PMT researchers should ideally use fear appeal manipulations when conducting security-related PMT studies" (p. 858).	
Manipulating fear appeals is a core element of the proper use of PMT.	• Experimental studies that manipulate variables are useful, but so are other kinds of research designs. • The revised 1983 PMT model expanded the theory from threat appeals to address sources of information more generally; thus, focusing only on fear appeals is not justified. • Not all sources of information in PMT can be manipulated, and, therefore, they require either observational or quasi-experimental research designs. • Researchers must conduct an experimental study to manipulate variables, and such studies can have lower external validity than observational studies.
Recommendation 3: "ISec PMT researchers should measure fear when conducting security-related PMT studies" (p. 859).	
Fear is an important partial mediator in PMT.	• Fear plays only an indirect role in Rogers' PMT and does not belong to its core part. Thus, modeling fear as a mediator is inconsistent with Rogers' PMT and is not required per Rogers' PMT. • Scholars can assess the effectiveness of a fear or threat appeal without measuring fear, and according to PMT, fear arousal is not necessary for a fear appeal to lead to a behavioral change. • In response to BGLMP's claim that fear facilitates behavioral change, scholars have argued for the opposite effect (Rippetoe & Rogers, 1987). • ISec researchers should always thoroughly study the original papers that present a theory before relying on those papers in their own research.
Researchers should measure fear, and they should do so with survey scales.	• The measurement scales BGLMP use appear to be problematic and require further research and validation studies. • The PMT literature in its home discipline (i.e., health psychology) presents multiple ways that researchers can measure or even manipulate fear through physiological reactions. In addition, they can use fMRI.
Recommendation 4: "ISec PMT researchers should ideally model and measure behaviors, not intentions" (p. 859).	

PMT should measure actual behaviors.	• Measuring behaviors can be useful, but requiring that every study measure behavior can hamper the progress of ISec research. • PMT, per Rogers, measures intention, and ISec scholars can follow Rogers' advice and examine intention. • Other theories are more useful than PMT for explaining how intentions turns into actions.

In addition to infosec behavior, many of these recommendations could be useful to research privacy behavior or cybercrime behavior. For example, the choice of whether to measure intention or behavior may vary depending on the research objectives and the topic (e.g., socially disapproved behavior versus socially approved behaviors). Each measure (e.g., intention vs. behavior) may contain different strengths and weaknesses. Therefore, claims such as "Researchers should ideally model and measure behaviors, not intentions" (BGLMP, p. 859) are problematic. If reviewers make such claims, they risk uniformly rejecting good research that does not measure behavior but addresses other important questions. As an example, scholars can be interested in (I) future behavior, (II) morally questionable behavior, (III) criminal behavior in the IS context. For these purposes, measuring actual or current behavior may be suboptimal.

4 Conclusions

BGLMP's PMT paper in *MIS Quarterly* outlines guidelines for future applications of PMT in infosec behavior research. We credit BGLMP for stoking the debate over what PMT is fundamentally about. However, in this paper, we contest some of their recommendations. We also outline alternative and more relaxed recommendations. We explain why Roger's PMT values intention as a measure of PMT. Furthermore, we explain why PMT research can focus on isolated PMT components and combine them to increase prediction accuracy without establishing what BGLMP referred to as a "full PMT nomology". The role of fear in Roger's PMT is also reconsidered. We hope that our arguments regarding the proper isolation of specific aspects of a phenomenon under study will influence the theorization process in other research domains.

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Appendix A: Review of Recent PMT Studies in Health Psychology

In Appendix A, we present our review of the use of PMT in recent health psychology research. We began the review by searching for "PMT" or "protection motivation theory" on Google Scholar, limiting the search to papers published since 2014. We downloaded the studies and scrutinized them one by one until no more PMT-relevant studies appeared on the following search result pages. We excluded all studies from fields other than health psychology, such as information systems and environmental management, or that were not written in English. We then excluded studies that mentioned PMT (e.g., in the literature review) but did not use it in the main research design. We further excluded two studies that were published only in a local university's journal and one study that only discussed the meaning of self-efficacy in PMT on a conceptual level. This review process yielded 40 usable studies.

Of the 40 studies, none use all components of the full or overall PMT model as presented in Figure 1 in this paper. Only two studies (5%) use adaptive and maladaptive coping. Thirteen studies (30%) use the "sources of information" part of PMT, although none consider all four categories (verbal persuasion, observational learning, personality, and previous experience). Only 15 studies (38%) include any form of behavior measurement. Eighteen studies (45%) include some kind of fear measurement, but no study claims that fear is a partial mediator. Fifteen studies (38%) combine PMT components with constructs from one or more other theories or modify PMT in some way that departs from Rogers' account. Overall, researchers in health psychology appear to use components of PMT selectively and adapt them in a way that fits the research questions and needs (or, at least, that is how the researchers justify them). As no single PMT construct is used in all studies, no evidence exists (in our sample; see Table A1) for the claim that there is one "core" or correct version of PMT.

Table A1. PMT Components Used in Recent Health Psychology Studies (2014-2018)

Study	Verbal Persuasion	Observational Learning	Personality	Previous Experience	Rewards	Costs	Severity	Vulnerability	Response Efficacy	Self-Efficacy	Fear	Adaptive Coping	Maladaptive Coping	Protection Motivation	Behavior	PMT Components Used With Other Constructs/Notes
Cummings et al. (2018)		✓[1]					✓	✓	✓	✓				✓		✓ Value dispositions, media attention
Bai et al. (2018)		✓[1]		✓		✓	✓	✓	✓	✓	✓			✓		
Malmir et al. (2018)					✓	✓	✓	✓	✓	✓	✓			✓		
Sabzmakan et al. (2018)					✓	✓	✓	✓	✓	✓	✓			✓	✓[2]	
Chambers et al. (2018)					✓	✓	✓	✓	✓	✓				✓	✓[2]	
Babazadeh et al. (2017)					✓	✓	✓	✓	✓	✓				✓	✓[2]	

Table A1. PMT Components Used in Recent Health Psychology Studies (2014-2018)

Study	Verbal Persuasion	Observational Learning	Personality	Previous Experience	Rewards	Costs	Severity	Vulnerability	Response Efficacy	Self-Efficacy	Fear	Adaptive Coping	Maladaptive Coping	Protection Motivation	Behavior	PMT Components Used With Other Constructs/Notes
Amaral et al. (2017)				✓	✓	✓	✓	✓	✓	✓				✓		✓ Long- and short-term perceived severity were distinguished
Kristoffersen et al. (2017)								✓	✓						✓[2]	✓ Certain PMT factors were used: self-rated health and expectations for future health as health vulnerability; preventive health beliefs and health behavior index as response efficacy
Gharaei et al. (2017)		✓[1]					✓				✓			✓		
Karmakar et al. (2017)					[3]	✓	✓	✓	✓	✓				✓		
Tesson et al. (2016)				✓[4]		✓	✓	✓	✓	✓				✓		
Ghahremani et al. (2016)						✓	✓	✓	✓	✓	✓			✓		
Chambers et al. (2016)					✓	✓	✓	✓	✓	✓				✓		✓ Communication skills; parental involvement; impact of alcohol
Chen (2016)							✓	✓	✓	✓				✓		✓ Perceived food risk; management quality; perceived product safety liability
Wong et al. (2016)							✓	✓	✓	✓				✓	✓[2]	✓ Implementation intention

Table A1. PMT Components Used in Recent Health Psychology Studies (2014-2018)

Study	Verbal Persuasion	Observational Learning	Personality	Previous Experience	Rewards	Costs	Severity	Vulnerability	Response Efficacy	Self-Efficacy	Fear	Adaptive Coping	Maladaptive Coping	Protection Motivation	Behavior	PMT Components Used With Other Constructs/Notes
Dehdari et al. (2016)						✓	✓	✓	✓	✓	✓			✓		
Liu et al. (2016)				✓				✓	✓	✓				✓	✓[2]	✓ Many control variables were found to be important for the target health behavior, such as medical insurance status. More PMT variables were measured, but only the factors listed here had statistically significant influence
Devlin and Dillard (2016)				✓			✓	✓			✓	✓	✓	✓		✓ Social identification theory
Xiao et al. (2016)					✓	✓	✓	✓	✓	✓				✓		
Ruthig (2016)				✓	✓	✓	✓	✓	✓	✓	✓			✓		
Xu and Chen (2016)					✓	✓	✓	✓	✓	✓				✓		✓ PMT was combined with a dual-processing view of information processing
Mullan et al. (2016)							✓	✓	✓	✓				✓		Although not combined with other constructs, PMT performed differently for different food-handling behaviors
Fenech (2016)							✓	✓	✓	✓	✓			✓		✓(Qualitative study) Understanding of the situation; effective communication with a multidisciplinary team; competence and confidence

Table A1. PMT Components Used in Recent Health Psychology Studies (2014-2018)

Study	Verbal Persuasion	Observational Learning	Personality	Previous Experience	Rewards	Costs	Severity	Vulnerability	Response Efficacy	Self-Efficacy	Fear	Adaptive Coping	Maladaptive Coping	Protection Motivation	Behavior	PMT Components Used With Other Constructs/Notes
Mirkarimi et al. (2015)		✓[1]			✓	✓	✓	✓	✓	✓				✓	✓	
Asimakopoulou et al. (2015)						✓	✓	✓	✓	✓	✓			✓		
Zhang et al. (2015)					✓	✓	✓	✓	✓	✓				✓		
Williams et al. (2015)						✓	✓	✓	✓	✓	✓			✓	✓[2]	
De Steur et al. (2015)		✓[1]				✓	✓	✓	✓	✓	✓			✓		
Rahaei et al. (2015)		✓[1]			✓	✓	✓	✓	✓	✓	✓			✓	✓[2]	
Dehbari et al. (2015)					✓	✓	✓	✓	✓	✓	✓			✓	✓[2]	
Zare Sakhvidi et al. (2015)					✓	✓	✓	✓	✓	✓	✓			✓	✓[2]	
Baghiani-Moghadam et al. (2015)							✓				✓				✓[2]	
Gaston and Prapavessis (2014)							✓	✓	✓	✓				✓	✓[2]	✓ Health action process approach (HAPA)
Dehdari et al. (2014)[2]						✓	✓	✓	✓	✓	✓			✓		✓ Implementation intention
Daniel et al. (2014)							✓	✓	✓	✓				✓		✓ Health action process approach (HAPA)
Yan et al. (2014)					✓	✓	✓	✓	✓	✓				✓	✓[2]	

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Ralph et al. (2014)					✓	✓	✓	✓	✓	✓				✓		
Wu et al. (2014)					✓	✓	✓	✓	✓	✓				✓		
Xiao et al. (2014)		✓[1]		✓	✓	✓	✓	✓	✓	✓				✓		
Ritland and Rodriguez (2014)	✓	✓		✓			✓	✓	✓	✓	✓	✓	✓	✓	✓	✓ Introduced the social context of danger in the model

Appendix B: Methodological Issues in Model Testing and Comparison

In Appendix B, we present a more detailed analysis of BGLMP's recommendation (pp. 840-841, p. 858) that scholars should compare any PMT extensions they introduce against the "core nomology" model using 1) explanatory power and 2) model fit[21]. We also scrutinize BGLMP's analysis and compare it against alternative models built based on previously published, PMT-based infosec behavior research on these two metrics (explanatory power and fit). We examine the pattern of regression coefficients used as support for the claim that infosec behavior researchers should use the "full PMT nomology" in their models. Substantial issues are identified with both the general principle of using these benchmarks to compare models and the way BGLMP perform the model comparison.

Issues With Model Fit Comparisons

To understand which of the two comparisons BGLMP applied, we first tried to reconstruct the models they used in their Studies 1 and 2. To do so, we applied a degree-of-freedom calculator (Cortina et al., 2017) but could not get results to match. Furthermore, we realized that the degrees of freedom were not constant, at 443 or 444 in Study 1's model and 2020 or 2121 in Study 2's model. This indicates that Bell et al. might have modified the models between runs. Contacting the authors did not resolve the issue. Thus, we were left guessing about what the authors did in the paper. Moreover, BGLMP suggest that the most valid model comparisons use the results from the last column of their Table 6[22]. These appear to be fit indices from the CFA model (BGLMP, p. 852, par. 1) instead of the structural regression model for the message with high fear appeal (BGLMP, p. 852, par 3), which would be the right benchmark. Thus, it is not clear which specific models they compared. In the following, we discuss possible model comparisons that they could have used in this situation and explain why they are problematic.

While researchers calculate different fit indices differently, all commonly used indices quantify how well a specified model can reproduce the sample covariances, either in an absolute sense (e.g., χ^2, RMSEA) or relative to a comparison model (e.g., CFI, TLI). Researchers typically compare models fitted to either the same data or different samples of the same variables (i.e., multigroup analysis). Given that, in this case, the reported results do not reveal the exact model specifications, we consider two different scenarios: 1) the models in BGLMP were fitted to different datasets containing different variables, or 2) the models were fitted to the same dataset containing the same variables.

Comparing models that are fitted to different sets of variables is problematic for at least two reasons. First, if the original model is mis-specified, adding more variables as controls will not solve the issue because the constraints implied by the smaller model will still be present in the expanded model. Thus, model fit is an invalid criterion for judging whether one should control variables to the model. Indeed, the purpose of statistical controls is to test for alternative explanations for data (e.g., Bernerth et al., 2018; Morgan & Winship, 2007, Chapter 5; Singleton & Straits, 2009, Chapter 4; Spector, 2019) and not to improve the model fit in an atheoretical manner. Second, the values of the commonly used statistics depend on the number of observed variables, known as the model size effect (e.g., Kenny & McCoach, 2003; Moshagen, 2012; Shi et al., 2019). This makes it difficult to judge whether a difference between the indexes of a small and large model reflect differences in the degree of misspecification or simply differences in model size. This is particularly true with CFI and TLI, where the indices may change simply because the null model one uses for calculating these indices fits very differently when the number of indicators increases. To summarize, comparing SEM models fitted to different variables rarely, if ever, makes sense.

[21] "A key issue is that virtually every ISec study made major, unsupported adaptations to PMT without (1) testing the core PMT nomology or (2) demonstrating that its changes actually improve the explanatory power of PMT or that the alternative model it developed enjoys better model fit than PMT" (BGLMP, p. 840). Also, "This limitation occurs because the studies do not provide the model-fit statistics required to demonstrate that an extended model improves on a baseline model. By using at least the core, established PMT nomology fully, ISec researchers may be able to increase the explanatory power of their models and may find that non-PMT additions are neither helpful nor necessary" (BGLMP, p. 841). Furthermore, "A lesson from our research is that before ISec researchers expand or truncate PMT, they need to demonstrate that their new use of PMT is a theoretical and empirical improvement on the intended use and modeling of PMT. For example, before adding social influence, researchers need to test the full nomology of PMT with proper model-fit statistics, which are available only via covariance-based SEM—notably not via PLS, which lacks these statistics and is more appropriate for preliminary model development, not for testing well-established nomologies (Lowry and Gaskin 2014)—and then test the addition of social influence. Otherwise, it will be impossible to ascertain whether the addition of the construct is an improvement to PMT or actually degrades model fit" (BGLMP, p. 851).

[22] "When reviewing Table 6, it is useful to compare the numbers from the previous studies against the examination of our full model (described as Study 2c), which includes some experimental non-PMT covariates" (BGLMP, p. 854)

Comparing two models fitted to the same data for the purpose of testing whether a variable should be in the model is also problematic. If we fit a model to data and then fit the same model that excludes the effects of one or more variables, this is equivalent to testing the hypothesis that the excluded effects are zero. In other words, the model fit comparison does not answer the question of whether the excluded variable should be in the model but whether the effect of the excluded variable would be zero if we included it. Thus, if one performs the recommended model comparison by fitting two models to the same data, BGLMP's guidelines essentially state that a researcher should not study specific parts of PMT unless one can simultaneously provide evidence against the other parts of the theory.

There are also problems in how BGLMP carry out the model comparisons[23]. BGLMP rely on an informal comparison of RMSEA, CFI, and TLI indices. This approach is not ideal, as it is generally difficult to determine when a difference in these indices indicates a meaningful difference in model fit; the use of a nested model χ^2 test and various information criterion indices (e.g., AIC), which scholars typically use for non-nested models, is more defensible (Kline, 2011, Chapter 8).

Issues With Comparing the Estimated Coefficient, Statistical Significance, and Explanatory Power

Apart from the general problems with using R^2 for comparing models, there are also specific problems in how BGLMP perform this comparison, which puts into question the validity of their conclusions. The R^2 statistic is calculated from the estimated coefficients. Thus, before comparing R^2s, it is essential to first ensure that the estimates are comparable in the first place. In latent variable models, these comparisons only make sense if measurement invariance is established (Kline, 2011, Chapters 9, 11; Little, 2013). Moreover, BGLMP appears to use standardized coefficients to compare regression coefficients between the high and low fear-appeal subsamples. This is problematic because a difference in standardized coefficients may not indicate a difference in the strength of relationships but simply in the differences in the variances of two subsamples' constructs. Jarvis et al. (2012) and Petter et al. (2012) explain this issue in detail, albeit in a different context.

The R^2 comparisons appear to be rigged[24] in two different ways. First, a large share of the R^2 statistics for the model in Study 2c, which BGLMP claim is the most relevant of their models, appear to originate from the control variables that the other models did not include[25]. Second, the model in Study 2 (Figure 8 in BGLMP) includes a behavioral variable in a full mediation configuration, but they omitted this variable from the comparison models (Figures 9-12 in BGLMP). This is problematic because if these relationships are, in fact partial, mediations, then the effects may be inflated because a correlation that is caused by an omitted direct effect is incorrectly attributed to the mediation relationship, inflating these estimates. This is a concern because the final model does not fit the data perfectly; the χ^2 test rejects the model by a wide margin. While this can occur for trivial misspecifications if the sample size is large enough, it nevertheless indicates a problem that researchers should diagnose and explain the source of misfit to readers (Kline, 2011, Chapter 8)[26]. As most of the new constraints imposed by the structural regression model over the CFA model come from the full mediation specification, it is quite likely that the full mediation model is at least partially mis-specified (i.e., partial mediation would be more correct). Thus, BGLMP overstate their regression paths, these paths' significance, and consequently, the R^2 statistic.

Finally, BGLMP use parameter significances as evidence for the superiority of one model over another. However, they provide this evidence through an informal comparison instead of a proper statistical test. This is problematic because differences in statistical significance (i.e., two p values for two coefficients) do not imply that coefficients differ significantly (i.e., one p value for the difference of two coefficients; for an

[23] As an additional problem, the interaction model in BGLMP's replication of Liang and Xue (2010) appears to have been constructed incorrectly. Given that Stata does not support distribution-analytic approaches to latent moderation, we assume that BGLMP applied one of the available product indicator approaches. These approaches involve selecting indicators for the products and possibly transforming these indicators and freeing or constraining the right parameters in the model (Little et al., 2006; Marsh et al., 2004). These procedures are designed so that the covariance constraints add hold exactly in the population and should therefore influence fit indices calculated from the sample by only a small amount. The only plausible explanation for the fact that the fit of the Liang and Xue (2010) replication differs dramatically from the other replications is that BGLMP performed the former replication incorrectly.

[24] We note that this may be unintentional.

[25] The R^2 values with and without a control group were .777 and .419, which means that adding the controls increased R^2 by .358 or +85%.

[26] Inspection of the descriptive fit indices provides additional evidence that although recommendations vary, the commonly used cutoffs in the SEM literature are 0.05 for RMSEA and 0.95 for TLI and CFI (Kline, 2011, Chapter 8), which the results of the structural regression models do not quite reach.

excellent demonstration, see Cumming, 2011, Chapter 1). Thus, a simple comparison of two sets of p values cannot provide evidence that one model is statistically significantly superior to another.

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