

The function of metaphor framing, deliberate or otherwise, in a social world

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Metaphor frames highlight certain aspects of a target domain and deemphasize others, thereby encouraging specific patterns of inference. A recent series of studies (Reijnierse, Burgers, Krennmayr, & Steen, 2015; Steen, Reijnierse, & Burgers, 2014), however, raises questions about the role of metaphor in communication and reasoning by (a) failing to find metaphor framing effects on a series of policy judgments, (b) critiquing the methods that have been used to test for metaphor framing effects, and (c) arguing that current theories of metaphor processing fail to consider the social-pragmatic dimension of metaphor in communication. Here, I reflect on these concerns and present novel analyses of data collected by Steen and colleagues, which reveal metaphor framing effects in these studies but fail to support a prediction of Deliberate Metaphor Theory (DMT): that extended metaphors are more likely to be remembered. DMT attempts to situate metaphor framing effects more intentionally along a social-pragmatic dimension; developing and testing the theory was a primary motivation of the studies conducted by Steen and colleagues. I discuss the implications of these findings and offer a perspective on how DMT can help grow our knowledge of the function of metaphor in a social world.

Keywords: metaphor, framing, reasoning, pragmatics, language

1. Introduction

The study of metaphor offers a window into the human mind, revealing novel insights about the structure of language systems, the structure of the human conceptual system, as well as the function of these systems in communication and reasoning. A recent series of studies (Reijnierse, Burgers, Krennmayr, & Steen, 2015; Steen, Reijnierse, & Burgers, 2014), however, raises questions about the role of metaphor in communication and reasoning by (a) failing to find metaphor

framing effects on a series of policy judgments, (b) critiquing the methods that have been used to test for metaphor framing effects, and (c) arguing that current theories of metaphor processing fail to consider the social-pragmatic dimension of metaphor in communication.

Here, I reflect on some of these concerns. I start by discussing evidence for the view that metaphors help to organize and represent complex information. In this section I also consider strengths and **weakness** of various methodologies for studying the effect of metaphor on thought. For instance, I argue that in some circumstances contrasting the effects of two metaphorical frames (i.e., the linguistic structures used to study metaphor framing) can be more informative than comparing the effect of a metaphorical frame to a non-metaphorical one.

Then I examine Deliberate Metaphor Theory (DMT; Steen, 2008, 2009). DMT aims to address a perceived limitation of current theories of metaphor by emphasizing the social-pragmatic context in which metaphors are used. The two sets of experiments that failed to find metaphor framing effects were motivated by a desire to develop and test DMT (Reijnierse et al., 2015; Steen et al., 2014). I discuss what these findings mean for the theory and offer a perspective on how DMT can help grow our knowledge of the function of metaphor in a social world.

In both of these sections I present new analyses of data collected by Reijnierse et al. (2015), which were graciously shared with me by the authors. These analyses, which are similar to those used in related work on linguistic framing, reveal that, in fact, metaphors do shape reasoning in ways these authors did not report. They do not, however, seem to support a central prediction of DMT: that extended metaphors are more likely to be remembered.

2. The organizational role of metaphor

One account of how metaphors shape thought appeals to the conceptual structure that they bring to bear on the target domains they are used to describe (Lakoff & Johnson, 2008; Sopory & Dillard, 2002). On this view, metaphorical structure facilitates long-term representation of abstract semantic knowledge (Boroditsky, 2000) and on-line processing of complex issues like immigration (Landau, Sullivan, & Greenberg, 2009) and cancer prevention (Hauser & Schwarz, 2014) by highlighting certain aspects of a target domain and deemphasizing others. Studies of metaphor framing, for example, have found that manipulating a metaphor used to describe a social issue (e.g., “Crime is a *virus*” versus “Crime is a *beast*”) changes how people think about the issue (Thibodeau & Boroditsky, 2011, 2013, 2015): describing crime as a *VIRUS* leads people to suggest reform-oriented interventions; a *BEAST* frame makes people more enforcement-oriented.

Thibodeau and Boroditsky (2011, 2013) designed a series of studies to test how the metaphoric structure that is communicated through metaphor frames affects thought. First, a group of naive participants was asked how they would solve a literal VIRUS or BEAST problem. The responses suggested that people have coherent but diverging schemas for addressing these problems: VIRUSES should be *diagnosed* and *treated*; BEASTS should be *captured* and *contained*.

When a different group of participants was presented with a metaphorical-ly framed description of crime and prompted to suggest a policy intervention, they showed a systematic influence of the metaphors. Peoples' beliefs about how to solve literal VIRUS and BEAST problems seemed to structure how they thought about crime VIRUSES and crime BEASTS. Participants were more likely to suggest addressing crime with social reforms like educational and economic interventions on the VIRUS metaphor (i.e., to focus on the root cause of the problem and promote long-term health). They were more likely to suggest enforcement-oriented interventions on the BEAST metaphor (i.e., by seeking capture and containment).

Larger effects (differences on the order of 20 percentage points) were found in the framing task when participants were asked to write freely about how they thought the city should respond to the problem, suggesting that one way in which metaphors influence thought is by making certain relationships in the target domain more accessible. Smaller effects (differences on the order of 10 percentage points) were found when participants were asked to choose among specified response options, suggesting that the metaphors did not just make certain responses more accessible, but also influenced how participants evaluated the interventions.

Results of follow-up studies suggested that the influence of these metaphors for crime were not driven by simple lexical associations with the words *virus* and *beast*. When participants were asked to list a synonym for *virus* or *beast* before reading a non-metaphorically framed description of crime, there was no relationship between the primes and policy preferences. Further, presenting participants with the metaphor at the end of a description of crime, rather than at the beginning, also failed to influence how participants responded to the crime problem. This result provides additional evidence that metaphor frames are more than lexical primes, and it also highlights the importance of timing in metaphoric reasoning. In order for a metaphor to shape how people think about a target issue, it should be presented early in a stream of processing.

The view that metaphors can shape how people think about complex issues is consistent with findings from a large body of work on metaphor framing (Hauser & Schwarz, 2014; Jia & Smith, 2013; Keefer, Landau, Sullivan, & Rothschild, 2011; Landau, Keefer, & Rothschild, 2014; Landau et al., 2009; McGuire, 2000; Ottati, Rhoads, & Graesser, 1999; Robins & Mayer, 2000; Sopory & Dillard, 2002). For instance, in a meta-analysis of metaphor framing studies conducted between 1983

and 2000, Sopory and Dillard (2002) considered six theories of how metaphor could affect thought. One was termed the ‘Superior Organization’ view, which is the most consistent with the perspective outlined above: It states that metaphors facilitate persuasion by helping to organize and structure information in the target domain. Alternative accounts included the idea that metaphors provide a reader with a type of puzzle to be solved (‘Pleasure or Relief’), that people who use metaphors seem more credible (‘Communicator Credibility’), that metaphors require more processing resources than non-metaphorical language and, in turn, decrease a person’s capacity to consider weaknesses of the persuasive message (‘Reduced Counterarguments’ and ‘Resource Matching’), and that metaphors promote frame-consistent elaboration (‘Stimulated Elaboration’). Sopory and Dillard (2002, p. 408) concluded that “the superior organization account is best supported by the data.”

2.1 What is the right control condition for a metaphor framing study?

One potential limitation of Thibodeau and Boroditsky’s (2011, 2013) work is that their studies did not contrast the two metaphoric frames for crime (*a* and *b*, below) with a non-metaphoric ‘control’ condition (like *c*, below), raising questions about how to interpret the results: “Without such a control condition, it is not possible to determine whether the effect is due to the metaphoricity of the frame, or a general framing effect” (Reijnierse et al., 2015, p. 247; see also Steen et al., 2014).

- a. Crime is a *VIRUS* ravaging the city of Addison.
- b. Crime is a *BEAST* ravaging the city of Addison.
- c. Crime is a *problem* ravaging the city of Addison.

This is a puzzling concern, as the *VIRUS* and *BEAST* frames are similarly metaphorical to each other, but differ in *metaphoricity* compared with “problem” (Thibodeau & Boroditsky, 2015). Thus, it would seem that finding a difference in how people respond to a metaphorically framed description of crime like (a) or (b) compared with how people respond to a non-metaphorically framed description of crime like (c), should raise concerns about the potentially confounding effects of *metaphoricity* as such.

It may be difficult to match metaphoric frames to non-metaphoric frames along a variety of important linguistic dimensions. For instance, compared with the *VIRUS* and *BEAST* metaphors, framing crime as a “problem” connotes a less *severe* instance of crime in a more *conventional* way (Thibodeau & Boroditsky, 2015). Differences in the connoted *severity*, *conventionality*, and *metaphoricity* of the metaphorical and non-metaphorical frames are just a few examples of variables that are confounded between these hypothetical conditions.

The VIRUS and BEAST frames, on the other hand, connote similarly *severe* instances of crime and with similarly *conventional* and *metaphorical* language (Thibodeau & Boroditsky, 2015). By matching two metaphoric frames along these dimensions (i.e., controlling for the potentially confounding influence of these factors), Thibodeau and Boroditsky (2011, 2013) can more confidently infer that differences in the structural entailments of the metaphors (i.e., participants' schematic knowledge of how to address literal VIRUS and BEAST problems) influenced how people thought about crime in their studies.

In sum, comparisons between metaphorically framed conditions license qualitatively different kinds of inferences than comparisons between metaphoric and non-metaphoric frames. If an experiment found that people were more inclined to support social reform after reading about a crime VIRUS than a crime "problem", then it would be difficult to infer whether such a difference resulted from a specific feature of the VIRUS frame, or a general difference in *metaphoricity* (or *valence* or *conventionality*) between VIRUS and "problem." Variance along one dimension (e.g., *valence*, *metaphoricity*, *conventionality*) may lead to a difference between the VIRUS and "problem" descriptions, whereas a different source of variance (e.g., in the conceptual entailments of the metaphors) may lead to a difference between the VIRUS and BEAST descriptions.

Comparing metaphor frames with non-metaphoric counterparts can also be informative, however. If the goal of a public messaging campaign were to promote support for social reform, it might be valuable to compare a VIRUS frame to a discussion of crime as a "problem" in order to answer the question: Is a particular way of talking about crime more likely to elicit support for reform-oriented interventions? But comparing these conditions is less informative in the context of thinking about *how* metaphoric structure influences thought. Diverse sources of variability between metaphorical and non-metaphorical frames makes it difficult to infer why people might respond one way to a metaphorical frame and another way to a non-metaphorical frame.

2.2 Statistical power

A practical concern is also introduced when a non-metaphorical condition is included. Given the moderate effect size elicited by metaphor framing studies, in general ($r = .07$; Sopory & Dillard 2002), and for this specific contrast (a crime VIRUS vs. a crime BEAST), relatively large sample sizes are needed to test for statistically significant differences between the conditions.

As noted above, Thibodeau and Boroditsky (2011, 2013) found that manipulating the frame for crime led to shifts in the order of 10 percentage points in a dichotomous judgment about how to approach crime-reduction. This shift

corresponds to an effect size of about .15 (Cramer's V). With β set to .8 and α to .05, a power analysis reveals that about 150 to 200 participants should be included in each condition of the sample in order to find a reliable influence of the frames – when contrasting a crime *VIRUS* to a crime *BEAST*.

If one expects a non-metaphorical framing condition to elicit an intermediate level of support for the policy interventions, as was predicted by Steen et al., (2014), then one is testing for even smaller effects – in the order of 5 percentage points. Such tests would require a substantially larger sample (see Thibodeau & Boroditsky, 2015).

2.3 Extended metaphor

Both of these issues – identifying the right control condition and sampling with sufficient power – apply to studies of extended metaphor. Comparing a condition that does not instantiate a metaphor with conditions that vary in the dosage of a given metaphor confound *metaphoricity* (and *valence*, and *conventionality*, and other linguistic dimensions) with metaphor *extendedness* (i.e., instantiating a given metaphor throughout a description of a target issue, as opposed to a single or more limited use of the metaphor). For instance, consider the pair of experiments reported by Reijnierse et al. (2015) (see Table 1).

In Experiment 1, participants either read about a crime “problem” or a crime *VIRUS*. If participants read about a crime *VIRUS*, they encountered 1, 2, 3, or 4 sentences that included metaphoric language likening crime to a *VIRUS*. In Experiment 2, participants either read about a crime “problem” or a crime *BEAST*. If participants read about a crime *BEAST*, they encountered 1, 2, 3, or 4 sentences that included metaphoric language framing crime as a *BEAST*.

As a metaphor is *extended* to more sentences in the report, the report becomes more *metaphorical* overall, making potential differences between conditions tricky to interpret. Imagine people responded to a crime report with four *VIRUS* sentences in one way and to a crime report with only one *VIRUS* sentence in another way. What could we infer about the cause of such a difference? It might be the result of specific features of the *extended VIRUS* frame *per se*, or a general difference in *metaphoricity* or *valence* or *tone* between conditions. Further, to be able to reliably test for differences between each of these conditions, an extremely large sample is needed. Below, I re-analyze data from Reijnierse et al.'s (2015) study to address some of these concerns.

Table 1. Overview of Reijniere et al. (2015)'s methods and results

Task	Participants read a description of crime and then rated support for policy interventions designed to reduce crime.
Manipulation	Crime framed as a “problem” or VIRUS (Experiment 1); Crime framed as a “problem” or BEAST (Experiment 2). If crime was framed metaphorically (as a VIRUS or BEAST), then metaphoric language was used in (extended to) 1, 2, 3, or 4 sentences of the crime report.
Measures	<p>Attitudes toward <i>reform</i>- and <i>enforcement</i>-oriented policy interventions (4 items per policy type, each rated on a 1–7 scale, averaged into one measure of support for <i>reform</i>-oriented policy support and one measure of support for <i>enforcement</i>-oriented policy support).</p> <ol style="list-style-type: none"> 1. <i>Reform</i>-oriented policies  <ol style="list-style-type: none"> a. Reform education practices b. Create after school programs c. Expand economic welfare programs d. Create jobs 2. <i>Enforcement</i>-oriented policies <ol style="list-style-type: none"> a. Increase prison sentences b. Increase street patrols c. Punish criminals faster d. Set higher maximum penalties
Prediction	As the report is infused with extended metaphoric language, participants will be more inclined to support the frame-consistent policy interventions (<i>reform</i> on VIRUS; <i>enforcement</i> on BEAST).
Results	Null effect in both experiments.
Interpretation	The relationship between the frames and policy interventions is insufficiently specified by the metaphor (i.e., “the distance between the task of rating the effectiveness of policy measures and our research question of investigating whether a metaphorical framing effect takes place might have been too big”; p. 260).

3. Analysis 1: Comparing metaphor frames

3.1 Methods

Reijniere et al. (2015) conceptualized their study as two separate experiments: one tested the effect of extending a VIRUS frame for crime; the other tested the effect of extending a BEAST frame for crime. Data from approximately 400 participants were collected for each study (i.e. 800 in total), roughly evenly distributed

into five conditions in each experiment: with 0, 1, 2, 3, or 4 sentences that included a given metaphor. For the reasons described above, I will compare data from the *VIRUS* condition with data from the *BEAST* condition, rather than analyze the data from these conditions separately. Applying the same inclusion criteria as Reijniere et al. (2015); that participants live in the US, have a good performance rating and spent at least 5 seconds (but less than 60 seconds) reading the report; leaves data from 715 participants for analysis.

Of note, Reijniere et al. (2015) do present a power analysis, which informed their decision about how many participants to include. This is a laudable approach. They found that 305 participants were needed in each experiment to find a reliable effect ($\beta = .8$, $\alpha = .05$), given a medium effect size (.25). There is, however, a concern with their method of conducting this analysis, since it was not grounded in prior work on metaphor framing. Effect sizes in metaphor framing studies tend to be more moderate than what was predicted (in the range of .07 to .15; Sopory & Dillard, 2002; Thibodeau & Boroditsky, 2011, 2013). Thus, contrasting the effects of the frames against one another addresses two concerns: It is better suited to answering questions about *how* metaphors influence thought and affords more statistical power to detect these effects.

In the analysis below, I treat some of the variables differently than how they were treated by Reijniere et al. (2015). First, as noted above, I compare the two metaphor framing conditions to each other, treating this comparison as a between-subjects factor. Second, I treat metaphor *extendedness* as a manipulation of a continuous variable, with structured variability in the degree to which the metaphor was used. Reijniere et al. (2015) analyzed this variable as a nominal factor. Third, I analyze support for the *reform-* and *enforcement-oriented* policies in a single omnibus test. Policies were analyzed separately by Reijniere et al. (2015). Since ratings of support for these policies were related to one another – both conceptually and empirically, $r(713) = -.229$, $p < .001$ – and measured within-subjects, they should be analyzed as a within-subjects factor (Howell, 2012).

3.2 Results

If extending the *VIRUS* frame increases support for *reform-oriented* policies and extending the *BEAST* frame increases support for *enforcement-oriented* policies, as Reijniere et al. (2015) predict, a mixed effect ANOVA model on ratings of policy support should reveal a statistically significant three-way interaction between the frames (treated as a between-subjects factor), metaphor *extendedness* (treated as a between-subjects scalar variable), and policy type (*reform* or *enforcement*; treated as a within-subjects factor). That is, people who read that crime is a *VIRUS*, compared with people who read that crime is a *BEAST*, should support *reform-oriented*

policies (but not *enforcement-oriented* policies) more as the report is infused with VIRUS language (and vice versa for the BEAST condition).

This is, in fact, what the analysis revealed, $F(1, 1421) = 4.111, p = .043$ (see Figure 1). Consistent with prior work and a modified version of Reijnerse et al. (2015)'s hypothesis (that takes into consideration comparison between the conditions), support for the *reform-oriented* policies tended to increase as a function of how many times the VIRUS frame was instantiated, $r(352) = .088, p = .097$, while support for *enforcement-oriented* policies tended to increase as a function of how many times the BEAST frame was instantiated, $r(359) = .123, p = .019$.

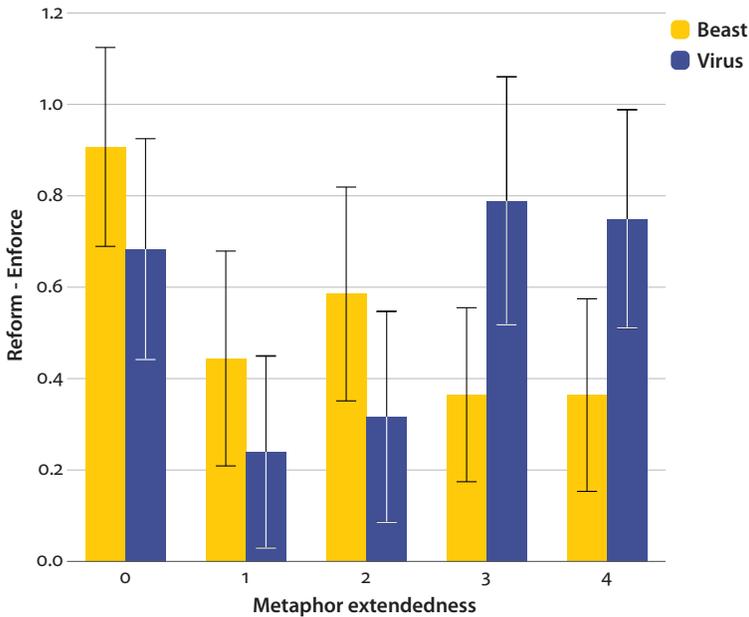


Figure 1. Mean ratings of *reform* policies minus mean ratings of *enforcement* policies by metaphor frame and *extendedness* (the number of sentences in which the metaphor was used). Error bars denote standard errors of the mean differences.

In other words, people were more likely to support frame-consistent policies (VIRUS and *reform*, BEAST and *enforce*) when the report included more metaphors, $r(713) = .105, p = .005$. The number of metaphors included in the report did not influence support for frame-inconsistent policies (VIRUS and *enforce*, BEAST and *reform*), $r(713) = -.005, p = .891$.

The analysis also revealed two statistically significant main effects that are illustrated in Figure 2. Overall, participants were more likely to support *reform-oriented* policies ($M = 5.39, SD = 1.06$) over *enforcement-oriented* ones ($M = 4.85, SD = 1.39$), $t(714) = 7.519, p < .001$, consistent with prior work (Thibodeau &

Boroditsky, 2011, 2013, 2015). Participants were also more likely to support either policy when the description included more metaphors, $r(713) = .074$, $p = .047$. As noted by Reijnierse et al. (2015), participants found the description more *vivid* when it included multiple instantiations of the metaphor, which may have elicited more forceful suggestions to implement policy reforms. Indeed, perceptions of the report's *vividness* were correlated with (averaged) ratings of policy support, $r(713) = .183$, $p < .001$.

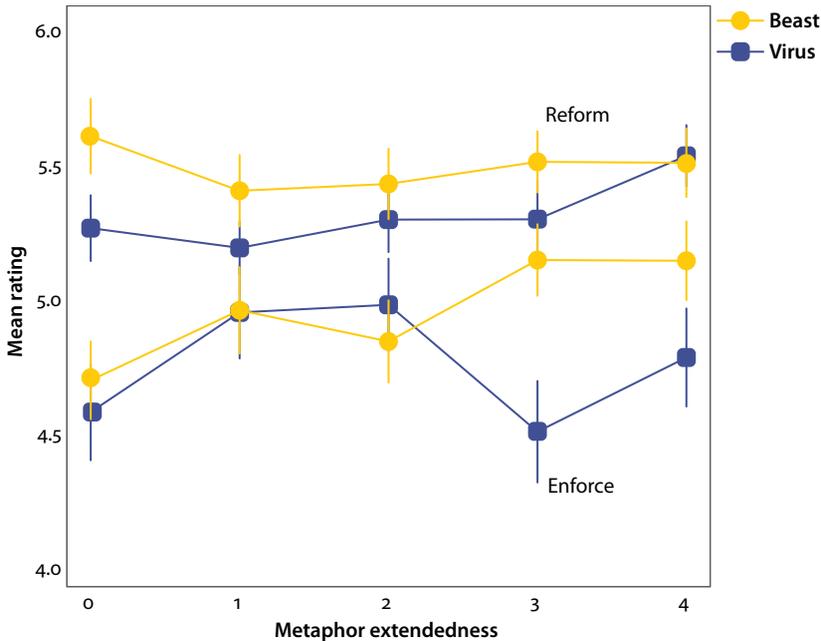


Figure 2. Mean ratings of *reform*- and *enforcement*-oriented policies by metaphor frame and the metaphor *extendedness* (the number of sentences in which the metaphor was used). Error bars denote standard errors of the means.

Note that without comparing the effects of the two metaphor framing conditions, *vividness* is a confounding variable (unless it is the mechanism through which extended metaphors are thought to influence participants' policy judgments). Importantly, there was no interaction between the frame (VIRUS or BEAST) and metaphor *extendedness* (0–4) on ratings of *vividness*, $F(1, 711) = 1.111$, $p = .292$, suggesting that the *vividness* of the two *extended* metaphor conditions increased at a similar rate. Therefore, comparing the effects of extending the VIRUS metaphor with the effects of extending the BEAST metaphor controls for the general relationship between *extended* metaphor and text-*vividness*.

There were subtle differences in how the materials for the VIRUS and BEAST conditions were designed and presented to participants. These differences may

account for certain patterns of results, but do not account for the three-way interaction. For instance, the order in which participants answered questions about the policy interventions differed between conditions: in the VIRUS version, participants rated support for *reform-oriented* policies before rating support for *enforcement-oriented* policies; the opposite order was presented to participants in the BEAST version. This change does not seem to explain why people tended to prefer the *reform-oriented* policies more as the description was infused with VIRUS metaphors, or why people tended to prefer the *enforcement-oriented* policies more as the description was infused with BEAST metaphors. Similar reasoning applies to the other notable difference between the VIRUS and BEAST versions of the study: participants who read about a crime VIRUS were told that crime had been rising over a 10-year period, while participants who read about a crime BEAST were told that crime had been rising in the city over a 1-year period.

Thus, when the metaphor framing conditions are contrasted against one another, policy approaches are treated as a within-subjects measure, and the number of metaphor instantiations is modeled as a scalar variable rather than a nominal variable, the data show the pattern predicted by Reijniere et al. (2015), which is consistent with prior work (Thibodeau & Boroditsky, 2011, 2013, 2015).

3.3 Discussion

One interpretation of this result is that extended metaphors are more influential than a single (or more limited) instantiation of a metaphor – in line with the hypothesis offered by Reijniere et al. (2015). However, without qualification, such a conclusion would obscure important nuances of *how* metaphors shape thought. By imposing a structure on the target domain, metaphors influence how people interpret and represent information about that domain (Lakoff & Johnson, 2008; Sopory & Dillard, 2002; Thibodeau & Boroditsky, 2011).

It is no accident that metaphors seem to play an especially prominent role in how people think about abstract concepts like TIME (Boroditsky, Fuhrman, & McCormick, 2011; Clark, 1973; Traugott, 1978). People have some direct experience of domains like TIME, JUSTICE, and ANGER, but these experiences are consistent with multiple representational structures (Boroditsky, 2000; Clark, 1973; Gibbs, 1996; Kövecses, 1986; Lakoff & Johnson, 2008; Traugott, 1978).

The dynamic relationship between source and target domains means that a given metaphor can be made more influential by surrounding it with information that is more susceptible to being shaped, or by using the metaphor in a way that better situates this information within the structure of the source domain (Black, 1962; Gentner, 1983; Thibodeau, 2016).

Some descriptions of crime are probably not susceptible to the influence of a metaphor frame. If a crime report detailed the systematic failure of police to do their job, thereby causing an increase in crime, participants would probably suggest that the city hire better police officers, regardless of whether crime was framed as a VIRUS or a BEAST. Some degree of ambiguity is needed in order for the structure of a metaphorical source domain to serve a function.

How information is situated within the frame also matters. As Thibodeau and Boroditsky (2011) showed, presenting a metaphor frame at the end of a crime report did not influence suggestions for addressing crime. This is because people seek to resolve ambiguity in real time (Bever, 1970); metaphor frames do not seem to reshape representations that have already been established. When the metaphor is presented early, on the other hand, people use the structure it provides to construct an on-line representation of the issue (cf. Bransford & Johnson, 1972).

Factors like the *novelty* and *aptness* of the metaphor may also moderate its influence (Bowdle & Gentner, 2005). Saying that crime is “feral and predatory” may represent a particularly *novel* instantiation of a BEAST metaphor for crime and lead people to use the structure provided by the source domain more explicitly. On the other hand, saying that crime is a “monkey” may not provide any structure at all because describing crime as a monkey is not particularly *apt* (cf. Glucksberg, 2001).

Reijnierse et al.’s (2015) stimuli illustrate both of these points: that ambiguity is important and that the way ambiguity is resolved depends on how the frame is presented. For instance, the non-metaphorical sentence, “We need a new *policy* to make our city *secure*” may actively encourage participants to suggest hiring more police officers or to increase prison sentences by using the word “security,” even if the first sentence of the report frames crime as a VIRUS (i.e., in some cases, the non-metaphoric language is not susceptible to being shaped by the metaphor frame).

On the other hand, substituting the non-metaphorical, “[Crime] is *unpredictable* and *serious* ... We need to *stop* it” with the BEAST-metaphorical, “[Crime] is *feral* and *predatory* ... We need to *trap* it,” may import additional structure from the source domain. In this case, the metaphor is *more* suggestive of a specific frame-consistent inference: that the city needs to capture and contain criminals (i.e., the metaphoric language is not only more susceptible to being shaped but also contributes to a frame-consistent interpretation).

Therefore, an alternative interpretation of the finding is that as the descriptions became more metaphoric, they afforded participants more opportunities to construe the issue in a way that was consistent with the frame (i.e., to use their schematic knowledge of how to address a literal VIRUS *infecting* or BEAST *preying* on a community). In some cases this was because the metaphoric substitutions were *less* suggestive of particular policy responses (e.g., replacing “security” with

“immune”). In other cases it was because the metaphoric substitutions were *more* suggestive of particular policy responses (e.g., replacing “serious” and “stop” with “predatory” and “trap”).

Simply distinguishing between a frame that is metaphorical or not, or a passage that contains a single metaphor or multiple metaphors fails to account for how information is processed and used by people to communicate with one another (Clark, 1996; Gibbs, 2013; Rumelhart, 1979). In other words, the design of the stimulus materials fails to account for the social-pragmatic dimension of metaphor processing.

4. Deliberate Metaphor Theory

Deliberate Metaphor Theory (DMT) encourages consideration of the communicative context in which metaphors are encountered (Krennmayr, 2011; Steen, 2008, 2011, 2015). Unlike Conceptual Metaphor Theory (Lakoff & Johnson, 2008) or the Career of Metaphor Hypothesis (Bowdle & Gentner, 2005), which seek to explain how linguistic or conceptual structures influence metaphor processing, DMT emphasizes social-pragmatic factors – the “third dimension” of metaphor.

In one sense, the distinction between deliberate and non-deliberate metaphor is clear. Metaphors in education, advertising, politics, and science are sometimes invoked intentionally to explain the topics they describe. Speakers who employ them often make clear why they are useful for thinking. For instance, Clark (1996) invites readers of his work to think of language as a joint action by comparing it to “two people waltzing, paddling a canoe, playing a piano duet, or making love” (p. 3). Clark (1996) does not stop here, however, and explains (p. 3):

When Fred Astaire and Ginger Rogers waltz, they each move around the ballroom in a special way. But waltzing is different from the sum of their individual actions – imagine Astaire and Rogers doing the same steps but in separate rooms or at separate times. Waltzing is the joint action that emerges as Astaire and Rogers do their individual steps in coordination, as a couple. Doing things in language is likewise different from the sum of a speaker speaking and a listener listening. It is the joint action that emerges when speakers and listeners – or writers and readers – perform their individual actions in coordination, as ensembles.

It feels safe to say that Clark (1996) is using this metaphor deliberately, since he has highlighted the general correspondence between conversing and dancing as well as several specific ways in which the two activities are similar. Steen (2015) cites a similarly clear example: “Imagine your brain as a house filled with lights. Now imagine someone turning off the lights one by one. That’s what Alzheimer’s

disease does” (p. 1; originally from Nash, 2000). Like Clark (1996), Nash (2000) has explicitly invited his readers to think of a topic metaphorically. The weight given to the metaphor in both cases, and the style in which they are written, suggests the use of these metaphors was deliberate. In contrast, when people say that they are *in* love or *on* time, they are probably not using container or spatial metaphors for a deliberate purpose. These are just conventional tropes (Keysar, Shen, Glucksberg, & Horton, 2000; but see Thibodeau & Durgin, 2008 and Gibbs, 2011, for evidence that even these kinds of metaphors influence how people think).

Steen (2008, 2015) argues that listeners process deliberate uses of metaphor differently than non-deliberate uses of metaphor. DMT’s central prediction is that processing deliberate metaphors requires more attentional resources because deliberate uses of metaphor are processed as comparisons (cf. Gentner, 1983), rather than through a categorization mechanism (cf. Glucksberg, 2001).

On the surface, it would seem that the distinction between the deliberate and non-deliberate use of metaphor would correspond to a distinction between deliberate (conscious, effortful) and non-deliberate (heuristic) processing of metaphor. However, this interpretation is explicitly negated (Steen, 2015, p. 3): “even though attention and comparison are here intentional in that they construct an appropriate meaning for the utterance, this does not imply that attention and comparison are conscious. It does not imply that they require deliberation either ... deliberate metaphor is not the same as deliberative metaphor.” Instead, Steen favors a more limited claim that processing deliberately-used metaphors “can afford further post-comprehension processes such as recognition, interpretation and appreciation” (cf. Chun & Turk-Browne, 2007).

An aspect of this prediction can be tested with data collected by Reijniere et al. (2015). At the end of the study, participants were told, “The report you read contained the sentence: ‘Crime is a .’”; participants were asked to fill in the blank. Support for DMT’s prediction would be found if people were more likely to remember the metaphor frame in conditions where it had been instantiated more frequently, since extending a metaphor is thought to signal its deliberate use. In other words, a guiding assumption of the design of Reijniere et al. (2015)’s materials was that “extended metaphor constitutes a case of deliberate metaphor” (p. 247) and the primary prediction of DMT is that people should be better able to remember deliberate uses of metaphor.

5. Analysis 2: Were people more likely to remember extended metaphors?

To analyze whether metaphor *extendedness* affected metaphor recall, I computed the proportion of participants who recalled the ‘correct’ metaphor frame (i.e.,

“virus” or a synonym like “disease” for the VIRUS condition; “beast” or a synonym like “predator” for the BEAST condition). Then I tested whether rates of correct recall differed as a function of how many times the metaphor was used (in 1, 2, 3, or 4 sentences). Figure 3 shows that the majority of participants in all conditions remembered the frame (84.6%, 95%CI = [.831, .861]), and that recall was not affected by metaphor *extendedness*.

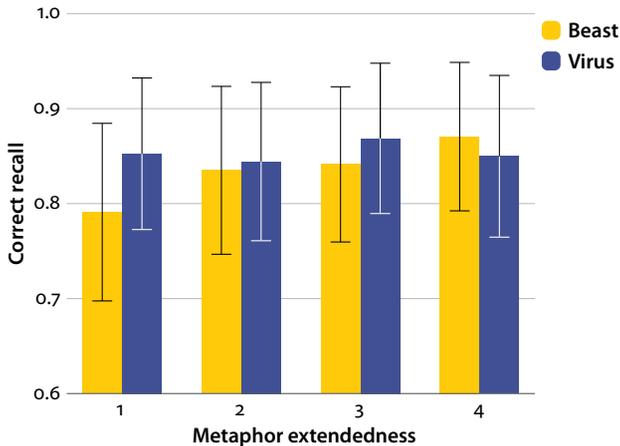


Figure 3. Mean recall for the metaphor frame by framing condition and metaphor *extendedness* (the number of sentences in which the metaphor was used). Error bars denote 95% CIs.

A logistic regression model with the number of sentences that included a metaphor as a continuous predictor revealed no significant differences in rates of recall as a function of metaphor *extendedness*, $\chi^2(1) = 1.012$, $p = .31421$ ($B = .105$, $SE = .105$, $p = .315$). There was also no effect of the metaphor frame on recall, $\chi^2(1) = 0.325$, $p = .569$, or an interaction between the metaphor frame and metaphor *extendedness* on recall, $\chi^2(1) = 0.718$, $p = .397$.

5.1 Discussion

Using data collected by Reijnierse et al. (2015), I tested a prediction of Deliberate Metaphor Theory: that instantiating a metaphor more often would make people more likely to remember it, because DMT posits that deliberate metaphors will be processed with more attention, and enhance encoding. In contrast to what the

1. The deviance between the models (i.e., difference in likelihood ratios) is reported as an index of model fit: model deviance approximates a χ^2 distribution with the number of added parameters as its degrees of freedom (Menard, 2002).

theory predicts (and a core assumption of the design of the experiment), manipulating the degree to which the metaphor was extended had no impact on how likely people were to remember it.

The rate of recall in the present study was notably higher than what has been found in prior work using a similar paradigm. For instance, Thibodeau and Boroditsky (2013) found that 47% (95%CI = [.427, .512]) of participants remembered the metaphor frame they were exposed to; Steen et al. (2014) found that 72% (95%CI = [.670, .764]) of participants remembered the metaphor frame they were exposed to (among the 350 participants in the replication conditions of Experiment 4).

One possibility is that the way the report was introduced made the metaphor sufficiently memorable and led to a ceiling effect (Howell, 2012). In contrast to past work, participants in Reijnierse et al. (2015)'s study were told about the crime problem from the perspective of the city's mayor, "In his latest speech, Mayor Smith of the city of Addison announced that crime has steadily increased in his city over the past 10 years/year. Smith said: 'Crime is a ...'" Presenting the report as a quotation or as the view of a politician may affect how people process the passage. In other words, the report may contain a variety of social-pragmatic cues that promote close reading. These cues may have overshadowed a potential influence of the extended metaphor.

Another possibility is that simply extending a metaphor, without drawing explicit attention to it (e.g., "think about language as a dance"; "imagine your brain as a house filled with lights"), is not sufficient for people to process the metaphor as deliberate.

The latter possibility, that people may not recognize extended metaphor as deliberate, represents a fundamental challenge to DMT (see, e.g., Gibbs, 2015a, 2015b; Steen, 2015). Can we measure whether a person used a metaphor deliberately or not? What linguistic structures or social cues reliably signal the use of deliberate metaphor? Are listeners sensitive to these signals?

Of note, prior work found no difference in the efficacy of the metaphor framing manipulation as a function of whether participants remembered the frame (i.e., both people who remembered the frame and those who did not were influenced by the metaphor; Thibodeau & Boroditsky, 2013), which raises additional questions about the value of distinguishing between deliberate and non-deliberate uses of metaphor.

At a high-level, DMT makes an important point: that more attention should be paid to social-pragmatic factors in metaphor processing. However, in its current form, it may be more appropriate to think of DMT as a suggestion for researchers to consider these factors in greater detail, rather than as a scientific theory. This point applies to all areas of psycholinguistic research (e.g., Clark, 1996; Gibbs,

2013). From my perspective, the biggest weakness of DMT as a theory is that it tries to use the mental state of a speaker as an explanatory variable: to explain how metaphors are processed by listeners in terms of the intentions of speakers, rather than in terms of more tractable variables like linguistic structures or social cues. Linguistic structures (extended metaphor) and social cues (an explicit appeal to think of a target domain through a metaphor) are thought to signal the deliberate use of metaphor, but are not the explanatory variables *per se* on the theory.

Consider an example that Steen (2015) uses to argue for the validity of distinguishing between deliberate and non-deliberate uses of metaphor: sometimes people ask questions like, “Why did you use that metaphor?” (p. 1). The example is supposed to motivate an intuition that people sometimes use metaphors deliberately, since the question is appropriate in response to someone who explains Alzheimer’s through a deliberate metaphor, but odd in response to non-deliberate metaphors like, “He’s *in* love” or “She’s *on* time.”

The problem with the example, though, is that the question is posed by the listener to the speaker. In order for this question to arise, the listener must recognize that a metaphor was used. In addition, they probably perceive the use of the metaphor as deliberate, which is why they ask for clarification. This example, therefore, shows that listeners sometimes *think* metaphors are used deliberately – but not that speakers *use* metaphors deliberately. Since DMT does not seek to explain variability in metaphor processing in terms of the perceptions of listeners, this example is misleading. As stated, DMT seeks to explain variability in metaphor processing in terms of the intentions (mental states) of speakers.

Psychologists generally think of mental states as dependent variables, and mental states as hard to measure. People have a hard time introspecting on their own beliefs and intentions (Keysar & Bly, 1995; Nisbett & Wilson, 1977), and DMT does not seem to provide a method for addressing this challenge. Although there seem to be clear cases in which people use metaphors deliberately (“think about language as a dance”; “imagine your brain as a house filled with lights”), as well as cases in which people use metaphors non-deliberately (*in* love; *on* time), there is also a lot of middle ground between these extremes. For instance, in revising this paper, I noticed a number of metaphors that I had used in writing the paper (e.g., *grow* our knowledge, *middle ground*). But I don’t know if I used them deliberately or not. Just as listeners may process a deliberately-used metaphor in a non-deliberate (not conscious or effortful) fashion, speakers may unwittingly use these structures and cues (e.g., extend a metaphor without realizing it).

Ultimately, it seems that the theory will have to appeal to linguistic structures and social cues as more tractable explanatory variables. Existing theories of metaphor processing like the Career of Metaphor Hypothesis (Bowdle & Gentner, 2005) treat these signals as explanatory variables and have shown that they matter.

For instance, people prefer simile form for novel comparisons (e.g., “The mind is *like* a kitchen”) and metaphor form for conventional ones (e.g., “The mind is a computer”). People are also faster reading novel comparisons presented as similes and conventional comparisons presented as metaphors (Glucksberg & Keysar, 1990). This has been taken as evidence that people process novel metaphors as comparisons and conventional metaphors as class-inclusion statements (Bowdle & Gentner, 2005).

One could imagine a parsimonious extension of existing theories of metaphor processing to address a multitude of social-pragmatic cues, as in: ‘people are more likely to process metaphors as comparisons when the metaphor is novel *or* when the speaker explicitly invites the listener to compare the source and target domains.’

Of course, these structures and cues are used for a purpose. When a speaker deliberately encourages comparison between a source and target domain, they are encouraging a particular way of thinking about the target domain. In this way, DMT reminds us that linguistic structures have important functions because language is a joint action (like a waltz).

A number of important empirical questions emerge from thinking about language from a social-pragmatic perspective. To test these questions, it might be useful to distinguish between metaphor production and metaphor comprehension. With regard to the production of metaphor, one might ask: When do people think that they are using a metaphor deliberately? What linguistic structures and social cues correspond to a speaker’s intuition? With regard to the comprehension of metaphor, one might ask: Are people sensitive to the linguistic structures and social cues that seem to signal a more deliberate use of metaphor? In what ways do these signals affect how people process and use metaphors? Addressing these questions may indeed grow our knowledge of how metaphors shape thought.

6. Conclusions

A robust literature on metaphor framing supports the view that metaphors help to organize complex information, which shape the inferences that people make about the domains they are describing (Sopory & Dillard, 2002; Thibodeau & Boroditsky, 2011, 2013, 2015). For instance, people are more likely to support crime-reduction programs that emphasize social reform on a *VIRUS* frame and more likely to support crime-reduction programs that emphasize enforcement on a *BEAST* frame.

These effects may also be explainable in terms of the intentions of speakers. People use metaphors deliberately because metaphors organize complex information in specific ways. Deliberate Metaphor Theory captures the intuition that people use metaphors purposefully, but DMT is not (yet) testable as a scientific

theory. It is unclear how to measure metaphor deliberateness, whether listeners are sensitive to metaphor deliberateness, and whether using a metaphor deliberately has implications for metaphor processing.

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