Spatial representation of coherence

Ulrich von Hecker[1], Ulrike Hahn[2]

and

Jasmine Rollings[1]

[1]School of Psychology, Cardiff University, UK.

[2]Birkbeck College, London, UK.

Word count: 8,262

Mailing address: Dr. Ulrich von Hecker, School of Psychology, School of Psychology, Cardiff University, Tower Building, 70 Park Place, Cardiff, CF10 3AT, United Kingdom. Tel. 0044 29 2087 6639. E-Mail: vonheckeru@cardiff.ac.uk

Abstract

Four experiments examined spatial correlates of the experience of coherence, that is, the extent to which propositions 'fit together'. Experiment 1 demonstrates for Heiderian triads (i.e., sets of liking/disliking relations between three fictitious persons) that name pairs from balanced triads, such as two friends commonly disliking a third person (high coherence) are seen as closer to each other in physical space as compared to name pairs from unbalanced triads, such as two persons disliking each other and having a common friend (low coherence). This pattern of results is conceptually replicated in two further experiments for categorical syllogisms. Two terms in conclusions from valid syllogisms (high coherence) were seen as spatially closer to each other than when two terms came from invalid syllogisms (low coherence). In the final two experiments, similar closeness effects are demonstrated for word pairs from scenarios that "made sense" in terms of causal connectedness (latent causality) as opposed to word pairs from scenarios perceived as causally unconnected. These findings are discussed in the context of spatial binding theories, applied psychology, and embodied cognition in general, and their methodological implications are highlighted.

Word count: 184

In this research we seek to present evidence for spatial processing as a correlate, or mediating mechanism, of the experience of coherence. 'Coherence', or the extent to which claims or pieces of evidence 'fit' together, is a notion that is central to people's evaluation of information. It has consequently attracted interest from philosophers (e.g., Olsson, 2005; Bovens & Hartmann, 2003), cognitive scientists interested in theory formation and explanation (e.g., Thagard, 2000), and psychologists interested in the evaluation of evidence, testimony, and witness credibility (e.g., Berman & Cutler, 1996; Berman, Narby, & Cutler, 1995; Brewer, Potter, Fisher, Bond, & Luszcz , 1999; Harris & Hahn, 2009). The concept has also been used to shape or reformulate theories on action planning and goal-related reasoning (Thagard & Millgram, 1995), as well as impression formation, where judgments about other people have to be generated based on piecemeal information on traits and behaviours (Kunda & Thagard, 1996). Also, text and discourse comprehension has been described as a coherence problem, involving the simultaneous assignment of meanings to different words and expressions (Kintsch, 1988; MacDonald, Pearlmutter, & Seidenberg, 1994).

At the most general level, coherence is seen as the degree to which a number of ideas make sense as an entire set. This criterion can be applied to propositions within theoretical systems, whereby coherence appears as a criterion for the truth of the whole system (e.g., Quine & Ullian, 1978). Closer to psychology, Thagard (2000) defined coherence in terms of parallel constraint satisfaction problems, whereby in a process of "mental balancing" (p. 3) complementary and conflicting pieces of information are integrated until they all fit together in a satisfying way. In this view, positive constraints may arise from explanatory or causal relations between propositions, from analogy, entailment, justification, or connectedness in a perceptual sense. Negative constraints arise mostly from logical inconsistency or incompatibility, or from negative association (Thagard, 2000). Philosophers, finally, have sought to define formal, probabilistic notions of coherence (e.g., Shogenji, 1999; Olsson, 2002; Fitelson, 2003). These have then been used to examine the extent to which more coherent claims are more likely to be true (Olsson, 2005; Bovens

& Hartmann, 2003; Glass, 2007) and the extent to which more coherent testimony implies more reliable witnesses (Olsson & Schubert, 2007). Without going into formal detail about these definitions, what they have in common is that coherence derives from the degree of (probabilistic) overlap between the individual propositions within a set of propositions, and positive relevance between them. In other words, a set of propositions is more coherent if they are more likely to be true (or false) together, and, if knowing one (or more) increases our belief in the truth of another. Crucially, coherence is a feature of a *set of propositions* or perceptual elements, not of these elements themselves.

Psychological research on coherence has tended to leave the notion unanalysed, and concerned itself primarily with its consequences. Moreover, it has focussed on its impact on beliefs. However, coherence may have other cognitive consequences. Specifically, the central idea examined in this paper is the relationship between perceived coherence and perceptions of physical space. If, as many researchers assume, people's reasoning about abstract relationships maps these on to physical relationships (e.g., Barsalou, 1999, 2008; Niedenthal, Barsalou, Winkielman, Krauth-Gruber, & Ric, 2005) then the coherence of information sets may have other effects. Mental representations of distinct concepts, ideas, or cognitive elements in general may be supported by 'mental space', as space may be used metaphorically to represent relations or processing steps within reasoning (Kant, 1999; Holyoak & Patterson, 1981; Leth-Steensen & Marley, 2000). If so, then elements that are coherent with each other within a context, a set of propositions, or a structure in general, should be seen as 'closer' to one another in space, relative to elements that are less coherent, as is manifest in the intuitive description of coherence in terms of the spatial metaphor of 'fit'. Thus there may also be perceptual consequences of that 'mental' distance in terms of observable patterns of responses in physical space.

Some support for this idea might be taken from recent findings concerning perceptual correlates of causality, given that causal relations between propositions may engender coherence (Thagard, 2000). Analogous to temporal binding, which links causes to effects (Eagleman &

Holcombe, 2002), spatial perception of two objects has been shown to be distorted such that two objects are seen as closer together when they are causally linked to each other than when they were not (Buehner & Humphreys, 2010). Previous research on causal binding had demonstrated that binding effects resulted as a consequence of the perceiver performing a voluntary action which eventually produced the causal effect (Haggard, Clark, & Kalogeras, 2002). Buehner & Humphreys (2010) showed in addition that mere observation of visual event sequences is sufficient to produce the spatial binding effects. Thus, the perception that two visually presented objects that are perceived to be causally related are also perceived as closer in space than two objects that are not causally related, can therefore be attributed more clearly to the level of mental representation, as opposed to such spatial effects being epiphenomena of action intentions and motor planning (Haggard et al., 2002).

Such effects may well be specific to causal relationships between concrete objects (arising for example from the body's need to coordinate information from multiple modalities, e.g., Stetson, Cui, Montague, & Eagleman, 2006). It is nevertheless worth examining whether similar phenomena might hold for other, more abstract entities and relationships. Coherence between propositions provides an intriguing candidate for such an exploration. Sets of propositions may be coherent to different degrees, so we sought to examine whether spatial proximity may represent such degrees of coherence between elements within sets of propositions.

Below we report the results from five experiments designed to test this basic hypothesis. All experiments examine the perception of physical distances as correlates, or concomitant phenomena, of the experience of more or less coherent sets of propositions. Experiment 1 does so using Heider's (1958) concept of cognitive balance for the generation of coherent and incoherent sets, Experiments 2a and 2b use categorical syllogisms for the same purpose, and Experiments 3a and 3b investigate the spatial representation of coherence using small scenarios (as sets of propositions) that imply causality to different degrees.

Experiment 1: Cognitive Balance

According to Heider (1958), 'balance' is a property of certain sets of propositions. These propositions each represent a relation between two entities, and that relation can have a positive or a negative valence. For example, two people may dislike (and be disliked by) a third person. In a simple set of three such propositions, 'balance' can be read off from the number of positive (or negative) relations. If the number of negative sentiment relations within the triad is uneven (that is, e.g., one), for example, two friends holding opposing attitudes towards an issue, or two people disliking each other, but each being friends with the same third person, then the triad is classified as unbalanced. Otherwise, (i.e., zero or two negative relations) it is balanced. Balanced triads have "Prägnanz", and a "Good Gestalt", which, as Heider (1958) claimed, also means they have a quasi-perceptual quality. Using a variety of different paradigms, balanced patterns of social relations such as triads have repeatedly been found to be learned more easily than unbalanced patterns (Crockett, 1982; DeSoto, 1960; Picek, Sherman, & Shiffrin, 1975; Press, Crockett, & Rosenkrantz, 1969; Zajonc & Burnstein, 1965). Previous research has also repeatedly found that balanced social triads are perceived as making more sense, and, conversely, unbalanced triads are seen as more in need of additional explanations for them to make sense (Brown & van Kleeck, 1989; Crandall, Silvia, N'Gbala, Tsang, & Dawson, 2007; Rudolph & von Hecker, 2006). In the present context, we therefore assume that balanced triads would be seen as more coherent than unbalanced triads.

To test the specific spatial predictions following from the assumption that balanced triads are more coherent than unbalanced triads, the present experiment uses pair-associate-learning (PAL) as a paradigm that has been used repeatedly in the past to investigate the ease of learning balanced versus unbalanced materials (e.g., Zajonc & Burnstein, 1965; Press et al., 1969; for an overview see Crockett, 1982). Basically, name pairs referring to fictitious people are the *stimuli*, and binary symbols denoting either mutual liking or disliking (e.g., "+" or "– ") are the *responses*. In a trial, a participant is prompted with a name pair without a symbol, and then generates a response according to what s/he thinks is the true relation between the two people. Learning about the entire set of propositions (e.g., A and B like each other) occurs via multiple presentations of trials, where feedback on correctness is given immediately after each PAL response. In terms of accuracy, studies using this paradigm typically find balanced materials to be learnt more easily than unbalanced materials, and, furthermore, consistently find positive relations to be learnt more easily than negative relations (for an overview: Crockett, 1982; see also von Hecker, 1994). These two effects are therefore predicted for the present Experiment as well, although they are not in the focus of our inquiry.

As our main question, we examine the possibility that perceptions of coherence have physical concomitants. Specifically, we sought to probe whether perceived distances in physical representations of elements within triads were affected by whether or not the triad from which the elements were drawn was balanced or not. We examined this idea by adding a spatial judgment task to the basic PAL task just described. This task, described in detail below, required participants to make a judgment concerning the physical distance between the names of two triad members as they appeared on the screen. This perceptual task demands neither semantic processing of the names nor the relationship between those named and avoided any direct or explicit metaphoric mapping between the concepts of liking or disliking and space (like=close or dislike=distant, see Baxter, 1984; Blum-Kulka & House, 1989; Spencer-Oatey, 1996). As explained in detail below, the task merely required participants to make a judgment concerning the graphical layout on the screen, without reference to the sentiment ('like'/'dislike') of the relation in question. The perceptual task simply followed a learning trial concerning the sentiment value of the particular relation. Thus knowledge of the sentiment could potentially infuse the spatial response being generated, particularly because it was sufficiently difficult as to leave an amount of perceptual uncertainty on each trial.

Method

<u>Participants</u>

Twenty-eight undergraduate students from Cardiff University (25 female, 3 male, mean age = 19.8 years) took part in the experiment. They received course credit for participation. Since a novel effect was expected, optimal sample sizes could only be approximately planned. We relied on previous research showing the targeted balance effect (15-20, see Crockett, 1982; von Hecker, 1994) and went slightly higher to accommodate what we expected to be a small spatial effect (as one would expect severe limits on how much cognitive factors can bias perception).

Materials

12 names were selected from a thesaurus of English first names, six male and six female, of approximately equal popularity. A stimulus would appear on the screen, e.g., "Trisha and Helen", and the response symbol would be either a "+" or a "–". Participants were instructed that these two symbols meant that the two people in the stimulus pair liked each other or disliked each other. Stimuli and responses were prepared such that 12 stimulus-response pairs resulted, corresponding to four triads. Two of these triads were balanced (one male, one female), and two were unbalanced (one male, one female).

Procedure

Participants were seated at a distance of approximately 70 cm from a computer screen. For one trial, one of the 12 stimuli was presented in the centre of the screen, in white ASCII typewritten mode on a black background, with the plus or minus at the centre of the line (as feedback), and the names to the left and right of the centre, at an overall lateral extension of approximately 30 mm on each side. There were 12 such stimuli, meaning that a single block was comprised of 12 trials presented in a random order.

There were 13 blocks overall, the first three without the spatial component. For a block without spatial component, participants were presented the name pairs in a random sequence. They were instructed to respond to each name pair accurately, without emphasis on a speeded response, but within a response window of 5 seconds. Participants used the left and right arrow keys which were marked "+" (left) and "–" (right), and those signs also appeared in the lower right quadrant of the screen, to assist response mapping. Upon presentation of each stimulus, participants indicated which of the two signs was correct for the pair of persons on the screen. Participants were instructed that, at the beginning, they would have to guess which sign would be correct, but that they would be able to learn through repeated exposure to the same stimuli.

For a block including the spatial component, on each trial, a fixation cross was first presented, mid-screen, for 600ms, upon which the two names were presented with a gap of 43mm width for 1500ms, with the centre of the gap replacing the fixation cross. Participants were instructed to keep their gaze fixed at the midpoint of the gap between the names, where the cross had been.

After this time, a string of small x's was presented for 700ms at a distance of 15cm below the two names (see Figure 1). The number of x's in the string was chosen such that in half of the trials, the string would just fit into the gap between the two names, for the other half of trials it would overshoot either at the left or right end by one character (with left or right randomly determined each time). After this, the two names at a gap stayed on screen for another 1500ms, before a prompt appeared, asking the participant to indicate *fit* (left arrow key) or *no fit* (right arrow key). The available response window for this was five seconds. A *fit* response indicates that the perceiver thinks the gap is wide enough for the x's to go in, therefore the two names being sufficiently distant from each other. A *no fit* response indicates that the gap is deemed too narrow, that is, the names are too close together for the x's to fit between them. Note that the nature of this spatial task has no bearing on, or makes no reference to, the sentiment value of the relation between the two names, or the configuration of the social triad where it was taken from. Test trials and post-experimental interviews revealed that when a participant kept their gaze to the gap as instructed, it was indeed fairly difficult at this stage of the task to exactly discern whether the string of x's would fit into the gap or not. Immediately after the response was made, the name pair was again presented at a slightly lower position on the screen, without gap, and the participant was prompted to indicate whether the two people liked each other or not, as described above.

Instructions and procedure for the trial blocks with spatial component were designed such that participants would anticipate themselves having to indicate the relation sign immediately after the spatial task. We assumed that during the first three blocks, i.e., those without spatial component, participants would have already encoded some knowledge about the associations between name pairs and signs, such that the execution of the spatial task would proceed in the presence of such knowledge.

Participants were informed about the differences in procedures between blocks of trials excluding and including the spatial task by an additional set of on-screen instructions that was presented to them after the third block was finished (i.e., after those blocks without the spatial task had been completed). In all blocks, the interval between the participant's final response to one name pair and the appearance of the next pair was 2 seconds.

The experiment lasted about 25 minutes, including debriefing.

Results

One participant had to be excluded because of missing data due to not following instructions, so the sample was reduced to N=27. As part of the post-experimental interviews, participants were asked whether they perceived the triadic configurations amongst the pairwise relations they had to learn and repeat in this experiment. None of the participants said they had noticed such configurations.

Accuracy for *like*/*dislike* responses

Though not our main focus, we first present the basic results concerning participants' learning of the relevant relations. This simply allows us to set our study in the context of past research on paired associate learning of Heiderian triads. In establishing that our PAL task replicates the main findings of past studies, we show also that the addition of the spatial task does not alter or interfere with participants' acquisition of triad knowledge. For each participant, accuracy scores were calculated as the proportions of correct responses out of the numbers of relations of a given category across all 13 blocks. There were 26 relations that were balanced/positive, 52 balanced/negative, 52 unbalanced/positive, and 26 unbalanced/negative. These scores were subjected to a 2 (Type of triad: balanced vs. unbalanced) x 2 (Type of relation: positive vs. negative) within-subjects ANOVA. Table 1 illustrates the means. Type of triad yielded a significant effect, F (1,26) = 4.19, MSE =.01, $p < .05$, *partial* $\eta^2 = .14$, as did Type of relation, F (1,26) = 7.08, MSE =.01, $p < .01$, *partial* $\eta^2 = .21$, as did the interaction, F (1,26) = 10.06, MSE =.007, $p = .004$, *partial* $\eta^2 = .28$. Relations from balanced triads were learned better than those from unbalanced triads, and positive relations were learned better than negative ones, thus replicating past findings (Crockett, 1982; DeSoto, 1960; Picek, Sherman, & Shiffrin, 1975; Press, Crockett, & Rosenkrantz, 1969; Zajonc & Burnstein, 1965).

Fit – no fit responses

The focus of our investigation, however, are participants' fit/no fit responses. To interpret these, we first analysed to what extent participants were accurate in saying that a given string of x's fit into the gap. That is, across all 10 blocks containing the spatial component, the proportion of correct responses in terms of *fit* vs. *no fit* was calculated for each of the four categories of relations, that is, balanced/positive, balanced/negative, unbalanced/positive, and unbalanced/negative (see Table 2). Performance was more accurate than expected by chance in all four categories (all t's (26) > 3.88, p's <.001, as tested against .50). We then evaluated these accuracy scores with a 2 (Type of Triad: balanced vs. unbalanced) x 2 (Type of relation: positive vs. negative) within-subjects ANOVA. Type of Relation was significant as a main effect, F (1,26) = 8.12, MSE =.005, $p < .01$, *partial* $\eta^2 = .24$, revealing that positive relations (M = .63) were given more correct spatial responses than negative relations (M = .59). Neither Type of Triad nor the interaction were significant. These analyses indicate that accuracy did not vary as a function of balance. Whatever effects balance might have on perceived 'fit' is consequently not a result of purely perceptual differences across trials.

Most crucially, to determine whether balance did have a biasing effect on fit/no fit responses we repeated the 2 x 2 ANOVA (Type of Triad x Type of Relation) on the proportion of fit responses, irrespective of accuracy (see Table 3)[1]. In line with the main hypothesis, a main effect of Type of Triad was observed, F (1,26) = 10.12, MSE =.004, p = .004, *partial* η^2 = .28, showing that participants were more likely to say the string of x's fitted into the gap when the name pair in question came from a unbalanced triad (M = .45) than when it came from a balanced triad (M = .41). There was also a tendency for Type of Relation to have an effect, F (1,26) = 4.02, MSE =.02, p = .06, *partial* η^2 = .13, whereby positive relations (M = .46) were associated with more *fit* responses than negative relations (M = .41). The interaction was not significant.

Additional SDT analyses

The results of these analyses are further confirmed by signal detection modelling (SDT, see Macmillan & Creelman, 1991). Here, hit rates (the proportion amongst all trials in which the string of x's actually fitted in the gap, when the response was "fit") and false alarm rates (the proportion of trials in which the string of x's did not fit in the gap, but the response was "fit") were computed for each participant for all four types of relations. Discrimination parameters d' were estimated from these proportions, with higher values corresponding to more accurate discrimination of whether or not an objective fit was present. Response tendency parameters C were also estimated, with higher values corresponding to greater tendencies to say "no fit" in general, that is, independent of discriminative performance. Again, two analyses of variance, one for each type of parameter, established that d' values were higher for positive (M = .946) than for negative relations (M = .601), F (1,26) = 10.44, MSE =.318, p = .003, *partial* η^2 = .28, with no other significant effects on discrimination accuracy. C values were higher for relations that came from balanced (M = .429) than unbalanced triads (M = .224), F (1,26) = 10.82, MSE =.108, p = .003, *partial* η^2 = .29, with no other significant effects.

Re-analysis on the basis of subjectively perceived triads

The above analyses indicate both that participants generally learned the relations, and thus the implied balance, as intended and that balance had the expected effect on judgments of fit. However, learning was also not perfect. Given that it is the subjective impression of balance, and with it ‘coherence’, that matters, we also conducted a supplementary analysis that conditioned solely on participants actual classifications. Such an analysis would lead to different results only if participants had consistently discrepant beliefs about specific triads, which meant that these triads were systematically assigned to a different experimental condition than assigned by design.

We re-analysed the proportions of fit responses with subjectively perceived balance as the criterion. That is, in each trial block all triads were classified as balanced or unbalanced on the basis of the participant’s responses, on that trial, as to whether the two persons liked or disliked each other. For example, if the three relations from a given triad ABC, were presented in the learning phase as *balanced* (“AB dislike, BC dislike, AC like”), but were remembered in that particular block by the participant as “AB like, BC dislike, AC like” (thereby committing one error at the first relation AB which changes the balance status of the triad), this triad was subjectively classified as *unbalanced* in that block. In this way, each triad in each block was re-classified as balanced or unbalanced, based on the subjective response pattern concerning the three likes or dislikes that it had elicited from the participant in that block[2]. This led to a re-classification into a different balance status for 23 percent of all triads presented in the experiment; that is, 256 triads changed their designed balance status as the result of subjective analysis, as counted across 27 participants and 10 relevant trial blocks, each containing four triads (= 1080 presented triads in total).

An ANOVA with the same design as above yielded a significant effect for Type of Relation, $F\,(1,26) = 7.54$, $MSE = .03$, $p = .01$, *partial* $\eta^2 = .22$, whereby relations reported as positive (M = .49) were associated with more *fit* responses than negative relations ($M = .39$). Supporting the main hypothesis, a main effect of Subjective Type of Triad was observed, $F\,(1,26) = 4.47$, $MSE = .009$, $p = .04$, *partial* $\eta^2 = .14$, showing that participants were less likely to say the string of x’s fitted into the gap when the name pair in question came from a triad in which relations had been so reported, irrespective of correctness, as to form a subjectively balanced triad ($M = .42$) than when it came from a subjectively unbalanced triad ($M = .46$). Additional SDT analyses corroborated this pattern.

d' values were only found different as a function of Type of Relation, F (1,26) = 4.19, MSE = 1.17, p = .05, *partial* η^2 = .13, replicating the above finding that positive relations were better discriminated (.89) than negative relations (.47), with no further effects. *C* values, however, only differed as a function of balance, F (1,26) = 10.72, MSE =.148, p = .003, *partial* η^2 = .28, with no other significant effects, showing as predicted that participants displayed a stronger tendency to say "no fit" in the case of relations from balanced (M = .37) than unbalanced triads (M = .13). Thus re-analysis on the basis of 'subjective balance' confirms the main results.

Discussion

Concerning basic accuracies for mutual like/dislike sentiments associated with the name pairs, the observed positivity main effect is a classical finding and has been shown to occur regularly when using the PAL, independently of balance effects (Crockett, 1982, von Hecker, 1994). Again in line with these previous findings, accuracy levels for positive and negative relations were not statistically different when part of a balanced triad. In terms of the binary response format of the PAL, the positivity effect might reflect people's mild tendency to assume mutual liking between other people as a default (Nilsson & Ekehammar, 1987; Sears, 1983). The balance main effect, a classical finding as well (see Crockett, 1982) appears to be, for a large part, carried by the simple effect due to negative relations being especially well learned when part of a balanced triad (see the significant interaction). Zajonc & Burnstein (1965, p. 161) already noticed the particularly high error rates for negative relations from unbalanced triads.

The main interest, however, lies with the spatial judgment task. As the values in Table 2 show, the spatial task was quite difficult (which aligns with subjective, post-experimental reports), although participants were able to perform better-than-chance. This state of affairs was intended as part of our basic experimental setting. It seemed essential that the task should not leave participants completely at random with respect to perceiving fit or no fit of the x's into the gap, while at the same time leaving a degree of uncertainty in the generating of a response. Such uncertainty, we reasoned, would facilitate a process by which the spatial representation a participant might associate with the particular name pair would have a chance to influence that response.

In line with this, the main result is that participants were more likely to say the string of x's fit into the gap when the name pair in question came from an unbalanced triad than when it came from a balanced triad. We interpret this as the balanced triads "making sense" more than unbalanced trials and therefore seeming more coherent than unbalanced triads (see Brown & van Kleeck, 1989; Crandall, Silvia, N'Gbala, Tsang, & Dawson, 2007; Rudolph & von Hecker, 2006) which, in turn, translated into greater 'proximity', that is, the constituent elements within balanced triads showed shorter distances between each other than the constituent elements within unbalanced triads. In terms of the SDT analysis, this main result corresponds to finding stronger response tendencies in the *no fit* direction for balanced than unbalanced relations, independent of how well the objective *fit* and *no fit* could be discriminated for balanced versus unbalanced materials. In sum, participants clearly did not behave as if there was a mapping of (positive vs. negative) sentiment quality onto distance, but rather as if the mapping was one of coherence onto distance. The same basic pattern resulted when analyzing spatial responses as a function of subjectively perceived balance.

Of lesser interest, though nevertheless relevant, is that positive relations tended to be more associated with fit responses than negative ones, which was not predicted. Metaphorical matching (in this case, positive corresponding to *close*, and negative to *distant*) is unlikely to be responsible for this, because the prediction from this would be the opposite pattern: If positive relations were associated with shorter distances, and negatives with longer distances in a metaphoric way (Baxter, 1984; Blum-Kulka & House, 1989; Spencer-Oatey, 1996), one would expect more *fit* responses to negative relations than positives, the opposite of what was observed. One possible explanation is that the expressions *fit* and *no fit*, used as labels for the response options, were semantically congruent with positivity versus negativity, which might have led participants to generate *fit* responses with a higher probability in view of a name pair that stood in a positive sentiment relation as compared to when the name pair stood in a negative sentiment relation, all other things being equal.

Finally, positive relations were associated with higher levels of fit-no-fit accuracy (as corroborated by the d' analysis), a finding which was also not expected and, again, is not central to our argument here. A possible explanation lies in the general observation that negative information attracts more attention than positive or neutral (for an overview see Rozin & Royzman, 2001), making it possible that processing resources directed at the semantic content of the relation between the two names were relatively more taxed in the negative than in the positive case. If so, resource availability for the spatial task proper would suffer more in the negative than in the positive case. Finally, the procedure used here proved unobtrusive in the sense that, as post-experimental interviews showed, participants were not aware of the triadic configurations amongst the relations they learn (see also von Hecker, 1994). Despite this, balance effects did emerge, not only in terms of memory accuracy for the sentiment qualities of the relations, but also in terms of spatial effects reported above. This may hint at the possibility that the cognitive processes in discerning higher coherence in balanced than unbalanced triads do not necessarily operate at the conscious level.

Experiment 2a: Categorical syllogisms

The fact that the results of Experiment1 cannot be explained by a simple mapping of 'positive' to 'close' and 'negative' to 'distant', but rather rely on balance as a property of the entire triad, suggests that the observed effect is based on a more abstract notion of coherence. One would thus expect such effects of coherence in other tasks and domains. In particular, one would expect effects of coherence even in domains that have no connection whatsoever with space. Arguably, one of the most abstract domains possible is the evaluation of logical truth, given that logical truth abstracts away from the specific semantic content of propositions and is determined only by logical connectives (e.g., 'and', 'or', 'not') and quantifiers ('some', 'all').

The present experiment therefore sought to test for effects of coherence on perceptions of physical space in a second domain: categorical syllogisms. A categorical proposition is a proposition that asserts or denies that all or some of the members of one category are included within another category, for example: All birds are animals. A categorical syllogism (syllogism) is a set of three categorical propositions, the first two of which are called premises, and the third conclusion. Together, the whole set of three propositions forms an argument subject to logical scrutiny, once premises and conclusion have been mentally represented (Johnson-Laird, 1983). In particular, if the conclusion follows logically from the two premises, the syllogism as a whole is called (logically) valid; if it does not follow, it is invalid. As a notion, logical validity involves necessity: In a logically valid argument the conclusion must be true if the premises are true. This necessity bestows coherence. For certain valid syllogisms such as "All A are B, all B are C, therefore all A are C" coherence is, in fact, *maximal* in the formal sense described by Bovens and Hartmann (2003). We assume here that valid syllogisms, once their logical validity is appreciated by an individual, will be seen as more coherent (e.g., All A are B, all B are C, therefore all A are C), than syllogisms which are invalid (e.g., All A are B, all B are C, therefore all C are A; see Appendix B for valid and invalid syllogisms as used here). The present experiment tests the hypothesis that the experience of coherence is reflected in spatial processes. More specifically, we test the hypothesis that on-screen representations of the two concepts mentioned in the conclusion (A and C, see above) should be perceived to be at a lesser distance to one another in the case that this conclusion follows logically from the two premises (valid syllogism) as compared to a case when it does not follow (invalid syllogism).

Recent research has shown that the content in syllogisms, particularly in the conclusions, can have effects in terms of participants' logical reasoning being triggered by their affective response towards the content (Klauer and Singmann, 2013). Therefore in this experiment, materials were generated from highly artificial contexts, such that strong affective responses towards them were unlikely.

Method

Participants

Following a power analysis (Faul, Erdfelder, Lang, & Buchner, 2007) based on the proportion of *no overshoot* responses, and on a required power of .70, sixty-two undergraduate students from Cardiff University (49 female, 13 male, mean age = 22.2 years) took part in the experiment[3]. Participants received course credit for their participation.

Materials

In order to minimize content effects, artificial contexts were created that would render existing world knowledge irrelevant, as explained to participants in a general instruction beforehand, as follows: "For all these syllogisms, please imagine you are in a garden where genetic manipulation took place such that normal biological rules are not pertaining. There might be unusual combinations of things and features, but just take it as science fiction. Instead, please focus entirely on the logical structure in making your decision." 12 syllogisms were constructed, six in each of the categories *valid* (e.g., All apple-trees have a red mark; All trees with a red mark are conifers; therefore: All apple-trees are conifers) and *invalid* (e.g., All fruits are ripe; All ripe fruits are cubic eggs; therefore: All cubic eggs are fruits). The sets of valid and invalid syllogisms had the same number of *All*- and *Some*- quantifiers and were made as equal with respect to their logical form. That is, three of the *valid* syllogisms were of the form [All A are B, All B are C, therefore: All A are C], and three were of the form [Some A are B, All B are C, therefore: Some A are C]; whereas amongst *invalid* syllogisms, three were of the form [All A are B, All B are C, therefore: All C are A], and three were of the form [All A are B, Some B are C, therefore: Some A are C]. For a complete list of materials in this experiment see Appendix B.

Procedure

Participants were seated in front of the computer. After reading instructions, they were presented with a training sheet on which syllogisms in abstract form were shown along with correct evaluations (valid vs. invalid), for the participant to clearly understand the concept of a categorical syllogism, and to receive some clarification and training with respect to evaluating syllogisms. Participants then received further training sheets without solutions for their individual practice, containing syllogisms of the type to be tested upon in the main experiment, whereby explanations were still available from the experimenter, until a criterion of one flawless sheet of evaluations was reached.

After this preparatory stage, the remainder of the experiment was carried out at the computer, and comprised four blocks. In each block, the 12 syllogisms were presented one by one, in a random order. Syllogisms appeared on the screen one at a time and the participants were allowed to consider them for as long as they wished. Participants were asked to evaluate each syllogism as valid or invalid, without responding immediately, instead keeping their evaluation in mind for a later response. This was done to ensure that the representation of the syllogism as valid (coherent) or invalid (incoherent) remained active during the ensuing spatial task. The spatial task was then administered in the same way as described in Experiment 1, now using the two concepts from the conclusion of the syllogism at hand to create the gap on the screen. Different from Experiment 1, the two response keys for the spatial task were now labelled '*overshoot'* (left arrow key) and '*no overshoot'* (right arrow key). Note that whilst in Experiment 1 the case of the width of the x's being accommodated by the width of the gap was associated, on the response side, with a positive expression (*fit*), it was now associated with a negative expression (*no overshoot*). These changes were made in order to be able to generalise across different response formats, and to address possible concerns about specific effects originating in the particular *fit* vs. *no fit* labelling (see above)[5].

Directly after responding to the spatial task, a five-second response window opened up for the participant to press one of the arrow keys as indicated on the screen, to say whether the syllogism at hand was valid or invalid. Feedback was given for this decision. The experiment lasted about 25 minutes, including debriefing.

Results

Two participants were excluded for low accuracies (.50-.60 proportions correct) in solving the syllogisms, so the sample was reduced to N=60.

Accuracy for *valid*/*invalid* responses

For each participant, accuracy scores were calculated as the proportions of correct responses for valid and invalid syllogisms across all 4 blocks. Valid syllogisms were found to be judged as valid more correctly (*M* = .87, SD = .12) than invalid syllogisms were as invalid (*M* = .74, SD = .20), $t(59) = 6.42$, $p<.001$.

Overshoot – no overshoot responses

As to the perceptual task, it was first examined to what extent participants judged correctly whether or not a given string of X's would overshoot. Across all 4 blocks, the proportion of correct responses in terms of *overshoot* vs. *no overshoot* was calculated for valid and invalid syllogisms from which the elements for the perceptual task were taken. Performance was in the same range as previously observed in Experiment 1, significantly better than chance (both *t'*s (30) > 5.00, p < .001), and not significantly different ($t < 1$) between valid syllogisms (*M* = .59, SD = .14) and invalid syllogisms (*M* = .61, SD = .13).

Central to the present hypothesis, we looked at the proportion of *no overshoot* responses across all 4 blocks, irrespective of accuracy, as a function of validity. In line with the main hypothesis, participants were more likely to say the string of x's did not overshoot the gap when the pair of concepts in question came from an invalid syllogism (*M* = .46, SD = .17) than when it came from a valid syllogism (*M* = .41, SD = .16), $t(59) = -2.17$, $p < .03$, meaning that in the case of invalid syllogisms, the gap between the two concepts were thought to be wider.

Additional SDT analyses

These data were analysed in terms of SDT. Hit rates (proportion amongst all trials in which the string of x's actually fitted in the gap, when the response was "no overshoot") and false alarm rates (proportion of trials in which the string of x's did not fit in the gap, but the response was "no overshoot”) were computed for each participant for all four types of syllogisms. Higher (conservative) values of response tendency parameters *C* meant greater tendencies to say “overshoot” in general, that is, independent of discriminative performance. Discrimination parameters did not differ between valid (*d’* = .80, SD = 1.39) and invalid syllogisms (*d’* = .81, SD = 1.05), $t(59) = -.05$, *ns*, but response tendencies were more conservative for valid (*C* = .38, SD = .84) than for invalid syllogisms (*C* = .12, SD = .78), $t(59) = 2.71$, $p < .01$. This directly replicates the findings with balanced and unbalanced triads of Experiment 1 above.

Re-analysis on the basis of subjective validity

Again, we conducted a supplementary analysis based on participants' subjective perceptions of each syllogisms' validity as an alternative criterion. Across all four experimental blocks, a spatial response to a trial was counted for the validity category that corresponded to the participant’s own judgment (*valid* vs. *invalid*), rather than to factual validity. Proportions of *no overshoot* responses were higher when the pair of concepts came from a syllogism that participants found invalid (*M* = .47) than when it came from one they found valid (*M* = .44), although statistically this was only a tendency, $t(59) = -1.67$, $p < .05$, *one-tailed*. Whilst average *d’* parameters were not different between conditions, $t(59) = -1.06$, *ns*, response tendency parameters *C* were significantly higher for valid (*M* = .32) than invalid syllogisms (*M* = .11), $t(59) = 1.75$, $p < .04$, *one-tailed*.

Discussion

As in Experiment 1, participants performed significantly above chance at the spatial task, but found this task quite difficult which presumably left some ambiguity in generating a response to each spatial trial. Using materials and a response mapping (i.e., left arrow key: *overshoot* vs. right arrow key: *no overshoot*) that were different from Experiment 1 this experiment conceptually replicated the main finding from Experiment 1 which is that participants were less likely to say the string of x’s fitted into the gap when the pair of words in question came from a coherent cognitive structure than when it came from an incoherent one. As the SDT analysis additionally demonstrated, logical validity did not have an influence on spatial discrimination performance per se (*d'*), but did have an influence on the general tendency to say "overshoot", irrespective of spatial discrimination. In the present experiment we assumed that logically valid categorical syllogisms would be experienced as more coherent than logically invalid ones, once their logical validity had been appreciated. Thus, higher coherence of valid syllogisms would be represented by a tighter (spatially closer) representation of the constituent terms in mental space. Importantly, all results were paralleled by a re-analysis using the participants' own classification of the syllogisms as valid or invalid. As the logical evaluation of syllogisms is not a very common cognitive routine, we made sure at the outset of the procedures that participants were familiar with the concepts of formal logical validity, and, if necessary, were trained up to a criterion in performing the evaluation of syllogisms. The present experiment extends our argument beyond Experiment 1 in that for the syllogistic materials, it is possible to associate the observed spatial effect more precisely with a concept of coherence that emphasizes the logical connection between elements of a cognitive structure. The spatial effect of representing coherent cognitions as closer together than non-coherent cognitions was again replicated, and was reflected in a more conservative response tendency, independent of the discriminative performance at the spatial task, for valid versus invalid syllogisms.

Experiment 2b: Replication and the role of conclusion believability

Given the novelty of the result, and given also that people's logical reasoning abilities have often been challenged (Stanovich, 1999) we sought to replicate the result in a further experiment. In particular, it is an often reported finding in the logical reasoning literature that people's judgments of the validity of an argument –which pertains to a relationship between the premises and the conclusions- is influenced by the intrinsic believability of the conclusion itself (e.g., Evans, Narston, & Pollard, 1983; Oakhill, Johnson-Laird & Garnham, 1989; Newstead, Pollard, Evans, & Allen, 1992). Specifically, implausible conclusions seem to alert participants to (logically) fallacious arguments (Evans, 2005). Adding in a manipulation of conclusion believability thus provides an interesting further test of the extent to which the coherence based spatial bias effect is based solely on coherence as opposed to intrinsic properties of individual propositions.

Method

Participants

Twenty-four undergraduate and post-graduate students, as well as university administrative staff from Cardiff University (15 female, 9 male, mean age = 26.3 years) took part in the experiment. The sample size was aimed at the same magnitude as in Experiment 1, in order to detect an effect of a comparable size as found there[3]. The participants were recruited through the University notice board and received £4.00 for their participation.

Materials

20 syllogisms were constructed, five in each of the categories *valid/believable* (e.g., All fruits in the crate are sour fruits; All lemons are in the crate; therefore: All lemons are sour fruits), *valid/unbelievable* (e.g., Some magicians can conjure; No human can conjure; therefore: Some magicians are not humans), *invalid/believable* (e.g., All children like chocolates; Some dogs like chocolates; therefore: Some children like dogs), and *invalid/unbelievable* (e.g., All glasses are in the cupboard; Some things in the cupboard are plates; therefore: Some glasses are plates)[4]. Care was taken to include only syllogisms with a maximum of two mental models (Johnson-Laird, 1983, see Appendix B). This was done to ensure that the materials would not tap too much into logical abilities, but would instead afford a clear and swift appreciation of the presence, or lack, of coherence.

Procedure

All procedures were identical to those in Experiment 2a. Again we used '*overshoot'* (left arrow key) and '*no overshoot'* (right arrow key) as response options. The experiment lasted about 35 minutes, including debriefing.

Results

Accuracy for *valid*/*invalid* responses

As previously, we first analysed accuracy scores to check that we replicated standard effects within the domain. In light of past research, participants' accuracy data should show effects of validity, believability and a validity x believability interaction. For each participant, accuracy scores were calculated as the proportions of correct responses in each category of syllogisms across all 4 blocks, that is, valid/believable, valid/unbelievable, invalid/believable, and invalid/unbelievable. These scores were subjected to an ANOVA (see Table 4 for the means). Validity yielded a significant effect, F (1,23) = 10.18, MSE =.02, $p < .001$, *partial* $\eta^2 = .36$, as did Believability, F (1,23) = 12.05, MSE =.009, $p = .002$, *partial* $\eta^2 = .34$, as did the interaction, F (1,23) = 16.75, MSE =.01, $p < .001$, *partial* $\eta^2 = .42$. Valid syllogisms (M = .87) were judged more correctly than invalid syllogisms (M = .77), and unbelievable syllogisms (M = .85) were judged correctly more often than believable syllogisms (M = .79). The difference in proportions correct between believable and unbelievable conditions was not significant for valid syllogisms ($p = .35$), the same difference was, however, significant for invalid ones, $t(23) = -4.51$, $p<.001$. An invalid syllogism was significantly more often (falsely) judged as valid when its conclusion was believable than when it was unbelievable, thus replicating the classical belief bias effect.

Overshoot – no overshoot responses

Our main interest, as in Experiment 1, lay in participants' performance on the perceptual task. We first examined the extent to which they judged correctly whether or not a given string of x's would overshoot. Across all 4 blocks, the proportion of correct responses in terms of *overshoot* vs. *no overshoot* was calculated for each of the four categories of syllogisms from which the elements for the perceptual task were taken, that is, valid/believable, valid/unbelievable, invalid/believable, and invalid/unbelievable (see Table 5). Performance was more accurate than expected by chance in all four categories (all t's (23) > 2.71, p's <.01, as tested against .50). When submitted to a 2

(Validity: vaild vs. invalid) x 2 (Believability: believable vs. unbelievable) within-subjects ANOVA these data showed no significant accuracy differences.

Central to the present hypothesis, we then looked at the proportion of *no overshoot* responses (as comparable to the *fit* responses in Experiment 1) across all 4 blocks, irrespective of accuracy, as a function of Validity and Believability (see Table 6). In line with the main hypothesis, a main effect of Validity was observed, F (1,23) = 7.54, MSE =.009, p < .01, *partial* η^2 = .25, showing that participants were more likely to say the string of x's did not overshoot the gap when the pair of concepts in question came from an invalid syllogism (M = .48) than when it came from a valid syllogism (M = .43). No other effect was significant.

Additional SDT analyses

Again, the data were analysed in terms of SDT. Hit rates (proportion amongst all trials in which the string of x's actually fitted in the gap, when the response was "no overshoot") and false alarm rates (proportion of trials in which the string of x's did not fit in the gap, but the response was "no overshoot") were computed for each participant for all four types of syllogisms. In two analyses of variance no significant effects at all were found for *d'* values, but response tendency parameters *C* were found higher for valid (M = .435) than invalid syllogisms (M = .129), F (1,23) = 6.37, MSE =.366, p = .019, *partial* η^2 = .21, with no other significant effects. This again replicates the findings from the previous experiments.

Re-analysis on the basis of subjective validity

Once again, a supplementary analysis examined participants' spatial judgments contingent on participants' judgments of validity rather than actual validity. Across all four experimental blocks, a spatial response to a trial was counted for the respective validity category (*valid* vs. *invalid*) that corresponded to the participant's own judgment about the particular syllogism as valid or invalid, rather than its factual validity. Proportions of *no overshoot* responses were then analyzed as a function of subjective validity and believability. Paralleling the previous results, a main effect of Validity was observed, F (1,23) = 4.34, MSE =.017, p < .05, *partial* η^2 = .16, showing that participants were more likely to say the string of x's did not overshoot the gap when the pair of concepts came from a syllogism they found invalid (*M* = .475) than when it came from a syllogism they found valid (*M* = .420). No other effect was significant. Average *d'* parameters were not found to be different between conditions at all, whereas response tendency parameters *C* were found higher for valid (*M* = .489) than invalid syllogisms (*M* = .021), F (1,23) = 6.26, *MSE* =.840, p = .02, *partial* η^2 = .21, with no other effects.

Discussion

Experiment 2b replicates the findings of Experiment 2a that logical validity as a relation between a set of premises and a conclusion gives rise to the spatial judgment effect of coherence. Furthermore, the believability manipulation indicates that the observed effect is based on coherence between propositions and that intrinsic properties of the conclusion are insufficient to generate biased spatial judgment. Whilst we replicated the standard belief bias in the validity evaluation data (see Table 4) reflecting the classical pattern (see Klauer, Musch, & Naumer, 2000), conclusion believability had no effect on spatial judgment.

It is worth noting that in the belief bias literature, it has been recently emphasized that SDT methodologies as opposed to using ANOVA-based analyses of accuracy difference scores can prevent misleading interpretations (e.g., Heit & Rotello, 2014). In particular, response bias parameters *C* were used to account for belief bias effects in past experimental studies of logical reasoning. In a similar vein we use SDT parameters here to analyse the spatial effect, relying mainly on the interpretation of *C* parameters for which our approach makes predictions. Participants' tendency to say 'overshoot' was shifted in the conservative direction (i.e., a greater proneness to say "overshoot") in cases where a conclusion "makes sense" to the participant, because the syllogism is logically valid.

Experiment 3a: Latent causality

This experiment was inspired by Bartlett's (1932) early study "The War of the Ghosts" in which he illustrated his concept of constructive memory. Presenting Western participants with a text taken from a foreign culture, which did not make immediate sense to them, Bartlett (1932) asked them to re-tell the content of this text to one another in a chain fashion, with a first participant being read out the original text, and each participant afterwards listening to a rendition by an earlier participant in the chain. With the chain length of six, the renditions of the text had become shorter than the original, and more coherent from the perspective of typical Western concepts and story lines. Bartlett's (1932) point was to argue that the main functional quality of memory consisted in the active reconstruction of materials at any point in time, using available schemata. In the present paper so far, we have argued that coherence is the degree to which a set of propositions makes sense as a whole, considered together. In the present experiment, we extend this idea to causality. But unlike Buehner & Humphreys (2010) or Haggard et al. (2002), we did not present participants with discrete events which could be constructed as cause and effect. Rather, we used small scenarios consisting of three propositions. These propositions were not explicitly linked in a cause-effect sort of way. However, when referring to applicable causal schemata, similar to Bartlett's (1932) approach, it was possible to *in*directly link them. One is likely to create a mental representation that "makes sense", constructing the scenario in a more coherent way, assuming an "unseen", or latent, causality as derived from the schema. As an example, consider the two words "knife" and "hospital", and note how the two propositions in which they are mentioned (the first and the third) can be linked by latent causality in the first, but not in the second scenario.

Scenario (1). 1. Johnny brought a *knife* to his workplace on a Wednesday. 2. He had always quarrelled with another man at work, Bill. 3. Bill had to be admitted to the *hospital* on the same day.

Scenario (2). 1. Johnny brought a *knife* to his workplace on a Wednesday. 2. His friend Bill's mother was very unwell. 3. Bill had to be admitted to *hospital* on the same day.

Note that the two words can only be linked by latent causality when constructing a whole, coherent representation of *all three* propositions; just propositions 1 and 3 alone are not sufficient. If such constructive mental activity takes place, we argue, then the resulting "latent causality" in the scenario, which makes it coherent, should translate into spatial effects, such that target words from different propositions within the scenario (such as the word pair *knife* and *hospital*) may be seen as closer together than analog target words from a scenario for which no such reconstruction was applicable.

Method

Participants.

Thirty-three participants from a residential area in Cornwall, UK (16 female, 17 male, mean age 31.2 years), all of them young or middle-aged adults of varied employment, took part in this study. Participants were volunteers from the public, and, volunteering, did not receive remuneration.

Materials

Ten scenarios were written, each in two versions, allowing construction of latent causality, or not. See Appendix D for a complete list of the scenarios. For each participant, a list of scenarios was assembled by randomly choosing a set of five scenarios to be presented in their coherent version, with the rest presented in their incoherent version. This way, all participants worked on the same content scenarios, only in different compositions as to which of them were causal or non-causal.

Procedure

Participants were seated in front of a laptop. An instruction and training sheet was presented on paper, explaining the concepts involved in this experiment and giving two example scenarios. Participants were asked to mark on the training sheet which scenario could be seen as containing latent causality. They were given feedback to ensure everybody had a good level of understanding. Next, participants were given on-screen instructions for the computer programme. There were four blocks of main trials, each block presenting all 10 scenarios. For each scenario, participants were given as much time as required to study it and decide whether they thought the scenario was "causal" or "non-causal". Once a participant terminated this stage by pressing the space bar, the two target words from the just-seen scenario were presented in the same way as the spatial task was delivered in all experiments so far. Participants made a 'fit' and 'no fit' response using the left and right arrow keys on the keyboard. On the following screen the scenario was shown once more, for the participant to provide a judgement of "causal" (using the left arrow key) or "non-casual" (right arrow key). Feedback was given on whether this decision was correct. The experiment lasted about 25 minutes, including debriefing.

Results

Accuracy for *causal*/*non-causal* responses

For each participant, accuracy scores were calculated as the proportions of correct responses in identifying scenarios with and without latent causality across all 4 trials. Causal scenarios were judged as "causal" in line with the experimenter intended classification more often than the (intended) non-causal scenarios were judged as "non-causal" (causal: $M = .89$, SD = .11; non-causal: $M = .82$, SD = .19), $t(32) = 2.76$, $p = .009$.

Fit – no fit responses

As to the perceptual task, it was first examined to what extent participants judged correctly whether or not a given string of x's would fit into the gap. Across all 4 blocks, the proportion of correct responses in terms of *fit* vs. *no fit* was calculated for causal and non-causal scenarios which the target words were taken from. Performance was again in the previous range for causal ($M = .64$, SD = .12) and non-causal scenarios ($M = .62$, SD = .11). They were, for both types of scenario, significantly better than chance (both t's (32) > 6.40, $p < .001$), but not different from each other, $t(32) = .89$, $p = .38$.

Crucially, we again looked at the proportion of *fit* responses across all 4 blocks, irrespective of accuracy, as a function of latent causality in the scenario. In line with the main hypothesis, participants were more likely to say the string of x's would fit into the gap when the target word pair came from an non-causal scenario (*M* = .33, SD = .20) than when it came from a causal scenario (*M* = .24, SD = .19), $t(32) = -2.23$, $p = .03$.

Additional SDT analyses

Once again, these data were also submitted to a signal detection analysis. Hit rates (the proportion amongst all trials in which the string of x's actually fitted in the gap, when the response was "fit") and false alarm rates (the proportion of trials in which the string of x's did not fit in the gap, but the response was "fit") were computed for each participant for causal and non-causal scenarios. Higher (conservative) values of response tendency parameters *C* meant greater tendencies to say "no fit" in general, that is, independent of discriminative performance. Discrimination parameters did not differ between causal (*d'* = 1.79, SD = 1.82) and non-causal scenarios (*d'* = 1.23, SD = 1.50), $t(32) = 1.57$, $p = .13$, but response tendencies were more conservative for causal (*C* = 1.57, SD = 1.54) than for non-causal scenarios (*C* = .95, SD = 1.23), $t(32) = 2.67$, $p < .01$. This replicates the findings of the previous experiments.

Re-analysis on the basis of subjective validity

Once again, we also conducted a supplementary analysis using participants' perceptions of each scenario's latent causality as an alternative criterion for analysis. Across all four experimental blocks, a spatial response to a pair of target words was counted for the respective causality category (*causal* vs. *non-causal*) that corresponded to the participant's own judgment, irrespective of their designed latent causality. Proportions of *fit* responses were higher when the targets came from a scenario that participants had found to be non-causal (*M* = .33) than when they came from one they had found to be causal (*M* = .25), $p = .03$. Whilst average *d'* parameters were not different between conditions, $t(32) = 1.06$, $p = .29$, response tendency parameters *C* were significantly higher for causal (*M* = 1.53) than non-causal scenarios (*M* = 1.02), $t(32) = 1.88$, $p = .03$, *one-tailed*.

Discussion

Although in the great majority of cases, participants saw latent causality in the presented scenarios as intended, correspondence with the experimenter-intended classification was slightly lower for non-causal than for causal scenarios. Possibly participants wondered whether they might have missed a hidden causal link because the three propositions together did not readily make sense (a possibility supported by post-experimental interviews) or they created some elaborate, underlying story.

Overall, for those scenarios that lent themselves readily to a causal schema linking the three propositions and thus rendering make them coherent, participants showed a tendency to be more conservative; that is, they were more likely to see the gap as too narrow for the X's to fit in, in keeping with the findings of the previous experiments. The present experiment thus replicates the basic spatial effect of coherence, and demonstrates it in a third domain.

Experiment 3b: Latent causality, replication

The manipulation of latent causality in the previous experiment differs from our other two domains (Balance and logical validity) in that there is no objective 'right' or 'wrong'. As just discussed, participants may posit a (non-obvious) causal link even in the designated 'non-causal' scenarios, or simply assume that such a link exists even if they cannot think of one. At the same time, causal links may be perceived to be more or less strong and more or less direct. Coherence in this case is thus clearly scalable. We thus sought to replicate Experiment 3a with a response scale that moved from a binary response of causal/non-causal to a graded scale from 1 ("definitely non-causal") to 9 ("definitely causal"). We also removed feedback on causality judgments, that is, there was no attempt to bring participants' judgments of the scenarios in line with experimenter-intended classifications.

If there is a spatial correlate of the mental activity that constructs latent causality from the three sentences in the scenario, then the observable spatial effect should be more pronounced in cases of clear, rather than ambiguous, mental representations of such causality. In other words, the spatial effect should appear more pronounced for scenarios receiving more extreme ratings on the scale as either non-causal or causal, as compared to scenarios that would receive more intermediate judgments on that scale.

Method

Participants.

Following a power analysis (Faul, Erdfelder, Lang, & Buchner, 2007) based on the proportion of *fit* responses in the previous experiment (3a), and on a required power of .70, thirty undergraduate students from Cardiff University (25 female, 5 male, mean age = 19.1 years) took part in the experiment. Participants received course credit for their participation.

Materials and Procedure

All methods used here were the same as in Experiment 3a, except for the screen display immediately after participants had made their 'fit' and 'no fit' response. On that screen, participants were now asked to provide a judgement of perceived causality in the scenario by indicating a number on a scale from 1 ("definitely non-causal") to 9 ("definitely causal"), using the numerical keys above the letters on the keypad. In contrast to Experiment 3a, they received no feedback on causality judgments. The experiment lasted about 25 minutes, including debriefing

Results

Two participants were excluded because in post-experimental interviews they revealed a strongly biased understanding of the spatial task, assuming that more than 80% of all spatial trials were a *no fit*. So the actual sample was reduced to N=28.

Range of *causal/non-causal* responses

For each participant, the two mean ratings for all trials on designed non-causal scenarios and designed causal scenarios were calculated. On average, causal ratings on the 9-point scale for designed non-causal trials (M = 3.54, SD = .90) were significantly lower than for designed causal trials (M = 7.16, SD = .92), $t(27) = 15.85$, p = .001, indicating greater perceived causality in those scenarios that had been designed as causal, as compared to the ones designed as non-causal. As the maximum for designed non-causal trials (5.75) as well as the minimum for designed causal trials (4.63) both overshot the scale's neutrality point of 5, it is notable that 50% of the data in both distributions partly covered areas of the scale that can be seen as expressing ambiguity. The Interquartile Range for designed non-causal trials was 1.43, ranging from 2.82 to 4.25, whereas for designed causal trials it was 1.30, ranging from 6.70 to 8.00. Thus, there is reason to believe that the used scenarios, whilst designed with a discrete state of latent causality (as present or absent) in mind, were indeed often understood in a more ambiguous way by participants. This holds for both types of scenarios designed as having or not having an underlying thread of latent causality.

Fit – no fit responses

As to the perceptual task, it was first examined to what extent participants judged correctly whether or not a given string of x's would fit into the gap. Across all 4 blocks, the proportion of correct responses in terms of *fit* vs. *no fit* was calculated for designed causal and non-causal scenarios which the target words were taken from. Performance was in the previous range for causal (M = .63, SD = .10) and non-causal scenarios (M = .65, SD = .09). They were, for both types of scenario, significantly better than chance (both t's (27) > 6.95, p < .001), but not different from each other, $t(27) = -.49$, $p = .62$.

Concerning the main hypothesis, we first looked at the proportion of *fit* responses across all 4 blocks as a function of designed latent causality in the scenario. Participants were equally likely to say the string of x's would fit into the gap when the target word pair came from an designed non-causal scenario (M = .39, SD = .14) as when it came from a designed causal scenario (M = .38, SD = .12), $t(27) = -.32$, *ns*. Secondly, we recalculated that same proportion again, but now only considering those trials in which participants had rated the pertaining scenario in a more extreme way. That is, we investigated such trials in which the scenario received a rating as either clearly non-causal (corresponding to ratings between 1 and 3 on the scale) or clearly causal (corresponding to ratings between 7 and 9 on the scale). This led to an exclusion of 333 trials out of an initial total of 1360 trials, corresponding to 24% trials that had received more ambiguous ratings in the midrange of the scale (between 4 and 6). Thus, analyzing only those 76% of the data where participants were likely to have constructed a clear mental representation of the scenario's causality status, they were more likely to say the string of x's would fit into the gap when the target word pair came from a subjectively non-causal scenario ($M = .45$, SD = .19) than when it came from a subjectively causal scenario ($M = .37$, SD = .14), $t(27) = -2.27$, $p = .03$.

<u>Additional SDT analyses</u>

The subset of data trimmed as above for clear representation of latent causality, was analysed in terms of SDT. Hit rates (proportion amongst all trials in which the string of x's actually fitted in the gap, when the response was "fit") and false alarm rates (proportion of trials in which the string of x's did not fit in the gap, but the response was "fit") were computed for each participant for scenarios judged as clearly causal and non-causal. Higher (conservative) values of response tendency parameters *C* meant greater tendencies to say "no fit" in general, that is, independent of discriminative performance. Discrimination parameters did not significantly differ between scenarios that were seen as causal ($d' = 1.11$, SD = 1.72) and non-causal ($d' = 1.65$, SD = 2.13), $t(27) = 1.57$, $p = .13$, but response tendencies were more conservative for scenarios clearly seen as causal ($C = .58$, SD = .99) than for scenarios seen as non-causal ($C = .03$, SD = 1.36), $t(27) = 2.21$, $p = .03$. This replicates the findings of the previous experiments.

Discussion

The present experiment provided evidence for the spatial effect of coherence in a particular, though rigorous sense. The effect was not present when all trials were considered, that is, including those trials in which the scenario had received intermediate, ambiguous judgments on the scale of perceived latent causality. However, when the subset of only those scenarios was considered which were seen, according to participants' ratings, as clearly non-causal or causal, the spatial coherence effect replicated. This in itself is a stringent test of the present basic tenet, that is, mental representations of distances between propositions may be a correlate for the subjective experience of coherence, and should therefore be the more pronounced, the clearer the mental representation of coherence (as being present or absent). However, in comparison to the data from Experiment 3a the question arises, why in this experiment, the analysis just according to designed causality status did not show the effect, whereas the same type of analysis in Experiment 3a did. One possibility is the use of the continuous response measure. This helped, as we think, to make participants feel they could "rightfully" assign intermediate states of latent causality to any of the scenarios. This again, we believe, would enhance the value of spatial representation of coherence as a tool for a better discrimination between those scenarios that clearly made sense to participants and those that clearly didn't. If coherence maps onto distance, that mapping should then be more likely to happen in clear as compared to unclear cases. If there is justification in viewing a scenario as "maybe making sense or maybe not or to some degree", then it is not obvious how to represent such a view, within the logic of the mapping. Thus, propositions from such an ambiguous scenario could either be represented close to, or rather distant from each other, in an indeterminate way. We submit that in such ambiguous cases the spatial simulation would either proceed in a random way (creating noisy spatial judgments later on) or spatial simulation would not take place at all, in which case, again, later spatial judgments would be made in a noisy way, at random and spontaneously, that is, without reference to an existing mental representation. Consider, in contrast, Experiment 3a. In that experiment, participants were to assume by default that each scenario had to be either clearly causal or clearly non-causal. This may have led participants to spatially simulate coherence even in those cases in which they were a bit uncertain. If we further assume that such simulation was likely to happen with a tendency for designed "causal" scenarios to be simulated as coherent, and designed "non-causal" scenarios as incoherent, the prediction follows that an analysis of those data as "causal vs. non-causal by design" would indeed yield a significant result.

General Discussion

It has been repeatedly argued that reasoning about abstract domains makes use of, or is supported by, analog extensions from experience-based, physical domains, particularly space (Boroditsky & Ramscar, 2002; Lakoff & Johnson, 1980; von Hecker, Klauer, & Sankaran, 2013;

von Hecker, Klauer, Wolf, & Fazilat-Pour, in press). According to this view, abstract notions such as time (Boroditsky & Ramscar, 2002), social power (Schubert, 2005) or evaluation (good vs. bad, Meyer & Robinson, 2004; Casasanto, 2009) appear to be reasoned about by way of simulation in mental space. This raises the possibility that not only single propositions about abstract concepts may be spatially simulated as such, but that spatial simulation might also take place in order to represent the (logical) coherence amongst an entire set of propositions, thus extending the same argument to the next level of abstraction.

During the crucial trials in all five reported experiments, participants were facing a situation in which they fixed their gaze to a gap between two words that came from a more comprehensive set of propositions. The particular set of propositions could be regarded, or was regarded subjectively by participants, as either high or low in coherence. In the balance case (Experiment 1) we know from earlier research that participants usually perceive balanced social triads as making more sense than unbalanced (Brown & Van Kleeck, 1989; Crandall et al., 2007; Rudolph & von Hecker, 2006), whereas in the syllogism case (Experiments 2a and 2b) participants were given formal training to evaluate categorical syllogisms and were asked to think about the validity of each syllogism immediately prior to the spatial task, and immediately afterwards to make a judgment on validity (which was reasonably accurate on average, see Table 4). Finally, in the latent causality case coherence is thought to be achieved by a participant's own, schema-guided, re-constructive activity (see Bartlett, 1932). As the main finding in all experiments, participants were less likely to say that a string of x's fitted into the gap when flashed for 700 ms onto the presentation screen at an off-foveal position while they were fixating the gap, when the two words came from a set of propositions high in coherence as compared to low in coherence. It is important to note that the spatial task itself was relatively unconstrained; that is, it was difficult, given the gaze fixation to the gap, to clearly discern whether the string of x's actually fitted or was too long for the gap. Our results suggest that participants' responses, in lack of sufficient perceptual constraints, were substantially influenced by their spatial representations of the level of coherence within the set of propositions. As the consistent pattern in the SDT analyses across all five experiments reveals, this effect can be attributed to a response tendency that affects participants' judgments about the string of x's versus the gap in a general way, that is, irrespective of actual discriminative performance (*C* parameters, but not *d'*s, were affected by coherence). The effect was also not tied to particular response labels or directional assignments of response keys as these were changed between the experiments (*fit* vs. *overshoot*). What is simulated by the spatial representation of coherence is, as we believe, the configuration of locations of individual terms within the set of propositions. (e.g., fictitious people A, B, and C in the case of triads, or categories involved in the categorical syllogisms). What is simulated, then, is the spatial closeness of these discrete concepts within a situation model (O'Brien, Albrecht, Rizzella, & Halleran, 1998; Zwaan & Radvansky, 1998), with average inter-concept distances being smaller for scenarios that the participant thinks "makes sense", i.e., incorporates an implicit causal connection, as opposed to scenarios that would not "make sense" to the participant.

The argument proposed here is germane to, but distinct from the one presented in Buehner & Humphreys (2010). These authors looked at perceptual judgments with respect to the distance between two concrete objects A and B within an animated display. Their task involved viewing a dynamically unfolding situation, as equivalent to a single proposition; for example, "moving object A causes the start of movement in object B". Thus, causality is linked to the observed sequence of events, that is, causality relies on the relations between the two terms A and B within one and the same proposition. In contrast, in our setting, perception of coherence is not linked to just one relation between two terms within a single proposition, but relies entirely on the joint assessment of *all three* propositions simultaneously, as *balanced*, *valid*, or *causal*, as opposed to *unbalanced*, *invalid,* or *non-causal*. In this sense, our investigation of coherence taps into the more abstract domain of *networks of propositions*, as opposed to relations between terms in one and the same proposition. Our results do dovetail with Buehner & Humphrey's (2010) in one important aspect. As discussed in the Introduction, their results provided stronger evidence than before to support the notion that binding occurs as a function of the mere perception of causality, and not only as a result of intentional acting. The spatial binding phenomena reported here support a similar notion. In particular, we emphasize the fact that in Experiments 2a and 2b, the spatial effect occurred only as a function of logical validity, again (if indirectly) suggesting the possibility that binding phenomena might indeed be more closely related to abstract reasoning (causal or logical) rather than to intentional or perceptual tendencies. With respect to perceptual processes, it is important to note that the latter statement is made in the light of equal spatial discrimination performance (SDT analysis) for both valid and invalid syllogisms, the spatial coherence effect being modelled as an independent response tendency.

The finding from Experiment 1 that participants were not aware of the triadic configurations amongst the learnt relations (see for a similar finding von Hecker, 1994) raises the question to what extent the spatial binding processes should be conceptualized as conscious or unconscious. The possibility that coherence might be cognitively represented, with the larger propositional structure that gave rise to it remaining as such unnoticed, is an intriguing one but cannot be exhaustively answered on the basis of the present data. The present data are ambiguous in this respect because there is evidence for a spatial coherence effect for balance, with perception of balance being mostly seen as Gestalt-like automatic, and effortless (see Crockett, 1982; Heider, 1958; Zajonc & Burnstein, 1965); but there is also evidence for spatial coherence effects in the case of categorical syllogisms, with coherence being a result of effortful logical reasoning, and in the case of latent causality where experiences of coherence arise from an effortful re-construction of script-related, implicit causal connections within the scenarios. As an additional research strategy, it appears promising to investigate the conscious-vs.-unconscious issue in connection with recent approaches to spatial representations in different areas of the brain. Neurocognitive results have accrued to suggest that brain activation during reasoning tasks, in particular the establishment of consistent, contradiction-free rank orders (based on sets of learned individual propositions such as *A is older than B*, *C is younger than B*, etc.), involve not only prefrontal areas but also areas in the parietal cortex areas which have been known to support spatial processing (Acuna et al., 2002; Christoff et al., 2001; Goel & Dolan, 2001; Heckers, Zalesak, Weiss, Ditman, & Titone, 2004; van Opstal, Fias, Peigneux, & Verguts, 2009). Therefore, in very general terms, space can be hypothesised as providing a simulation scaffold for the construction of an integrated set of propositions into a common, unified structure which, as such, may or may not be accessible at a conscious level.

An important question for future examination in this context is whether the spatial processing steps which our observed phenomena give a hint of are themselves correlates of the experience of coherence, or have a causal or constitutive role. Whatever the neural basis, however, our results suggest that if a set of propositions is found to be coherent there will be an association of “close distance” between pairs of elements as part of that representation. There is no reason to expect effects to be limited to Good Gestalt, logical truth, and causality, as examined here, and future work should explore other criteria of coherence as mentioned by Thagard (2000), for example, analogy, entailment, or justification. Future research should also address the question of whether spatial effect of coherence is limited to a situation (as typical for the present research) in which model representation of similar types are generated, some of which “make sense” whereas others do not. It is possible that the use of space as a metaphor for “what makes sense” depends on the existence of contrast between exemplar representations of both kinds.

Exploration of other forms of ‘coherence’ seem important, last but not least, because we see not just a theoretical relevance in the spatial effect of coherence but also a potentially invaluable methodological tool. Studies of human reasoning are hampered by the fact that they often involve technical notions that either deviate from everyday usage of terms or are not that familiar to participants (e.g., ‘logical validity’). It is nevertheless of interest whether participants possess intuitive, implicit versions of these concepts (as has been the focus of, for example, research on logical reasoning). Our spatial judgment task couldn’t be easier to explain to participants, and, could potentially be used to probe underlying perceptions of coherence without having to explicitly identify to participants the theoretical notion of interest (e.g., 'logical validity', 'causality'). Future work should thus probe in more detail the suitability of spatial judgments as an implicit measure[6].

In conclusion, it appears from the present results that people have a tendency to reason spatially about the degree to which something "makes sense" and to represent things that "hang together" in their understanding as likewise close to each other in their mental representation. It will be on the future research agenda to explore more fully the domains in which this tendency can be observed, and what further correlates it has. One way of colloquially expressing the notion of coherence is to say that something "makes sense". However, from this broad-sounding colloquial understanding it becomes clear that different people may have different concepts and criteria for how and when a set of propositions would indeed "make sense" to them, personally. Thus, future research might be directed more generally to the facets of lay understanding of coherence and how these different concepts might relate to the phenomena observed here. Obviously, all areas in which logical understanding and the mastery of formal logic is crucial are good candidates for such investigations. But likewise, the perception of causality and the forensic domain will hopefully benefit from the present approach.

References

Acuna, B. D., Eliassen, J. C., Donoghue, J. P., & Sanes, J. N. (2002). Frontal and parietal lobe activation during transitive inference in humans. *Cerebral Cortex 12,* 1312–1321.

Barsalou, L. W. (1999). Perceptions of perceptual symbols. *Behavioral and Brain Sciences, 22*, 637-660.

Barsalou, L. W. (2008). Grounded cognition. *Annual Review of Psychology, 59*, 617-645.

Bartlett, F. C. (1932). *Remembering: An experimental and social study.* Cambridge, UK: Cambridge University Press.

Baxter, L. A. (1984). An investigation of compliance-gaining as politeness. *Human Communication Research, 10,* 427-456.

Berman, G. L., & Cutler, B. L. (1996). Effects of inconsistencies in eyewitness testimony and mock-juror decision making. *Journal of Applied Psychology, 81,* 170–177.

Berman, G. L., Narby, D. J., & Cutler, B. L. (1995). Effects of inconsistent eyewitness statements on mock-juror's evaluations of the eyewitness, perceptions of defendant culpability and verdicts. *Law and Human Behavior, 19,* 79–88.

Blum-Kulka, S., & House, J. (1989). Cross-cultural and situational variation in requesting behavior. In: S. Blum-Kulka, J. House, & G. Kasper (Eds.), Cross-Cultural Pragmatics: Requests & Apologies (pp. 123-154). Norwood, N.J.: Ablex.

Boroditsky, L., & Ramscar, M. (2002). The roles of body and mind in abstract thought. *Psychological Science, 13,* 185-189.

Bovens, L., & Hartmann, S. (2003). *Bayesian epistemology*. Oxford, England: Oxford University Press.

Brewer, N., Potter, R., Fisher, R. P., Bond, N., & Luszcz, M. A. (1999). Beliefs and data on the relationship between consistency and accuracy of eyewitness testimony. *Applied Cognitive Psychology, 13,* 297–313.

Brown, R., & Van Kleeck, M. H. (1989). Enough said: Three principles of explanation. *Journal of Personality and Social Psychology, 57,* 590-604.

Buehner, M. J., & Humphreys, G. R. (2010). Causal contraction: Spatial binding in the perception of collision events. *Psychological Science, 21,* 44-48.

Casasanto, D. (2009). Embodiment of abstract concepts: Good and bad in right- and left-handers. *Journal of Experimental Psychology: General, 138,* 351-367.

Christoff, K., Prabhakaran, V., Dorfman, J., Zhao, Z., Kroger, J.K., Holyoak, K.J., et al. (2001). Rostrolateral prefrontal cortex involvement in relational integration during reasoning. *Neuroimage 14*, 1136–1149.

Crandall, C. S., Silvia, P. J., N'Gbala, A. N., Tsang, J., & Dawson, K. (2007). Balance theory, unit relations, and attribution: The underlying integrity of Heiderian theory. *Review of General Psychology, 11,* 12-30.

Crockett, W. H. (1982). Balance, agreement and positivity in the cognition of small social structures. In L. Berkowitz (Ed.), *Advances in experimental social psychology* (Vol. 15, pp. 1-57). New York: Academic Press.

DeSoto, C. B. (1960). Learning a social structure. *Journal of Abnormal and Social Psychology, 60,* 417-421.

Eagleman, D.M., & Holcombe, A.O. (2002). Causality and the perception of time. *Trends in Cognitive Sciences, 6,* 323–325.

Evans, J. S. B. (2005). Deductive reasoning. In K. J. Holyoak, & R. G. Morrison (Eds.), The Cambridge Handbook of Thinking and Reasoning (pp. 169-184). Cambridge, UK: Cambridge University Press.

Evans, J. S. B., Barston, J. L., & Pollard, P. (1983). On the conflict between logic and belief in syllogistic reasoning. *Memory & Cognition, 11,* 295-306.

Faul, F., Erdfelder, E., Lang, A.-G., & Buchner, A. (2007). G*Power 3: A flexible statistical power analysis program for the social, behavioral, and biomedical sciences. *Behavior Research Methods*, *39*, 175-191.

Fitelson, B. (2003). A Probabilistic Theory of Coherence. *Analysis, 63*, 194–99.

Glass, D. H. (2007). Coherence measures and inference to the best explanation. *Synthese, 157,* 275-296.

Goel, V., Dolan, R. J. (2001). Functional neuroanatomy of three-term relational reasoning. *Neuropsychologia, 39,* 901–909.

Haggard, P., Clark, S., & Kalogeras, J. (2002). Voluntary action and conscious awareness. *Nature Neuroscience, 5,* 382–385.

Harris, A. J. L., & Hahn, U. (2009). Bayesian rationality in evaluating multiple testimonies: Incorporating the role of coherence. *Journal of Experimental Psychology: Learning, Memory, and Cognition, 35,* 1366-1373.

Heckers, S., Zalesak, M., Weiss, A. P., Ditman, T., & Titone, D. (2004). Hippocampal activation during transitive inference in humans. *Hippocampus, 14,* 153–162.

Heider, F. (1958). *The psychology of interpersonal relations.* New York:Wiley.

Heit, E., & Rotello, C. M. (2014). Traditional difference-score analyses of reasoning are flawed. *Cognition, 131,* 75-91.

Holyoak, K. J., & Patterson, K. K. (1981). A positional discriminability model of linear-order judgments. *Journal of Experimental Psychology: Human Perception and Performance, 7,* 1283-1302.

Johnson-Laird, P. N. (1983). *Mental models: Towards a cognitive science of language, inference and consciousness.* Cambridge: Cambridge University Press.

Johnson-Laird, P. N., & Byrne, R. M. J. (1991). *Deduction*. Hove (UK): Erlbaum.

Kant, I. (1999). *Critique of pure reason* (translated by P. Guyer & A.Wood). Cambridge: Cambridge University Press.Kintsch, W. (1988). *Comprehension: A paradigm for cognition*. Cambridge: Cambridge University Press.

Klauer, K. C., & Singmann, H. (2013). Does logic feel good? Testing for intuitive detection of logicality in syllogistic reasoning. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, 39, 1265-1273.

Klauer, K. C., Musch, J., & Naumer, B. (2000). On belief bias in syllogistic reasoning. *Psychological review*, *107*, 852.

Kunda, Z., & Thagard, P. (1996). Forming impressions from stereotypes, traits, and behaviors: A parallel-constraint-satisfaction theory. *Psychological Review, 103,* 284-308.

Lakoff, G., & Johnson, M. (1980). *Metaphors we live by.* Chicago: University of Chicago Press.

Leth-Steensen, C., & Marley, A. A. J. (2000). A model of response time effect in symbolic comparison. *Psychological Review, 107,* 62–100.MacDonald, M. C., Pearlmutter, N. J., & Seidenberg, M. S. (1994). Lexical nature of syntactic ambiguity resolution. *Psychological Review, 101,* 676-703.

Macmillan, N. A., & Creelman, C. D. (1991). *Detection theory: A user's guide.* Cambridge: Cambridge University Press.

Meier, B. P., & Robinson, M. D. (2004). Why the sunny side is up. *Psychological Science, 15,* 243-247.

Newstead, S. E., Pollard, P., Evans, J. S. B., & Allen, J. L. (1992). The source of belief bias effects in syllogistic reasoning. *Cognition, 45*, 257-284.

Niedenthal, P., Barsalou, L. W., Winkielman, P., Krauth-Gruber, S., & Ric, F. (2005). Embodiment in attitudes, social perception, and emotion. *Personality and Social Psychology, Review, 9,* 184-211.

Nilsson, I., & Ekehammar, B. (1987). Person-positivity bias in political perception? *European Journal of Social Psychology, 17,* 247-252.

Oakhill, J., Johnson-Laird, P. N., & Garnham, A. (1989). Believability and syllogistic reasoning. *Cognition, 31,* 117-140.

O'Brien, E. J., Albrecht, J. E., Rizzella, M. L., & Halleran, J. G. (1998). Updating a situation model: A memory-based text processing view. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *24*, 1200-1210. doi:10.1037/0278-7393.24.5.1200

Olsson, E.J. (2002b). What is the Problem of Coherence and Truth? Journal of Philosophy, 94, 246–72.

Olsson, E. J. (2005). *Against coherence: Truth, probability, and justification*. Oxford: Oxford University Press.

Olsson, E. J., & Schubert, S. (2007). Reliability conducive measures of coherence. *Synthese, 157,* 297-308.

Picek, J. S., Sherman, S. J., & Shiffrin, R. M. (1975). Cognitive organization and coding of social structures. *Journal of Personality and Social Psychology, 31,* 758-768.

Press, A. N., Crockett, W. H., & Rosenkrantz, P. S. (1969). Cognitive complexity and the learning of balanced and unbalanced social structures. *Journal of Personality, 37,* 541-553.

Quine, W.V., &| Ullian, J.S. (1978). *The web of belief* (2nd edition). New York: Random House.

Rauh, R., Hagen, C., Knauff, M., Kuss, T., Schlieder, C., & Strube, G. (2005). Preferred and alternative mental models in spatial reasoning. *Spatial Cognition & Computation, 2-3,* 239-269, DOI: 10.1080/13875868.2005.9683805.

Rozin, P. & Royzman, E. B. (2001). Negativity bias, negativity dominance, and contagion. *Personality and Social Psychology Review, 5,* 296-320.

Rudolph, U., & von Hecker, U. (2006). Three principles of explanation: Verb schemas, balance, and imbalance repair. *Journal of Language and Social Psychology, 25,* 377-405.

Shogenji, T. (1999). Is Coherence Truth-Conducive? *Analysis, 59*, 338–45.

Schubert, T. (2005). Your highness: Vertical positions as perceptual symbols of power. *Journal of Personality and Social Psychology, 89*, 1-21.

Sears, D. O. (1983). The person-positivity bias. *Journal of Personality and Social Psychology, 44,* 233-240.

Spencer-Oatey, H. (1996). Reconsidering power and distance. *Journal of Pragmatics, 26,* 1-24.

Stanovich, K. E. (1999). *Who Is Rational?: Studies of individual Differences in Reasoning.* Mahwah, NJ: Lawrence Erlbaum.

Stetson, C., Cui, X., Montague, P. R., & Eagleman, D. M. (2006). Motor-Sensory Recalibration Leads to an Illusory Reversal of Action and Sensation. *Neuron, 51*, 651–659. doi:10.1016/j.neuron.2006.08.006

Thagard, P. (2000). Coherence in thought and action. Cambridge, MA: MIT Press.

Thagard, P., & Millgram, E. (1995). Inference to the best plan: A coherence theory of decision. In A. Ram, & D. B. Leake (Eds.), *Goal-driven learning* (pp. 439-454). Cambridge, MA: MIT Press.

van Opstal, F., Fias, W., Peigneux, P., & Verguts, T. (2009). The neural representation of extensively trained ordered sequences. *Neuroimage, 47,* 367–275.

von Hecker, U. (1994). The significance of balance, generalization, and positivity as cognitive rules. *The German Journal of Psychology, 18, 3,* 234 - 235.

von Hecker, U., Klauer, K. C., & Sankaran, S. (2013). Embodiment of social status: Verticality effects in multi-level rank-orders. *Social Cognition, 31,* 387-402.

von Hecker, U., Klauer, K. C., Wolf, L. and Fazilat-Pour, M. (in press). Spatial processes in linear ordering. *Journal of Experimental Psychology: Learning, Memory, and Cognition.*

Zajonc, R. B., & Burnstein, E. (1965). The learning of balanced and unbalanced social structures. *Journal of Personality, 33,* 153-163.

Zwaan, R. A., & Radvansky, G. A. (1998). Situation models in language comprehension and memory. *Psychological Bulletin*, *123*, 162-185. doi:10.1037/0033-2909.123.2.162

Author Note

Correspondence should be addressed to Ulrich von Hecker, School of Psychology, Cardiff University, Tower Building, Park Place, Cardiff, CF10 3AT, United Kingdom. Electronic mail may be sent to vonheckeru@cardiff.ac.uk.

We would like to thank Laura Guthof, Ana Krilanović, Mythri Kukkaje Ramakrishna, and Natalia Zarzeczna for help with data collection, as well as Isabel Gauthier, Karl Christoph Klauer and an anonymous reviewer for helpful comments to an earlier version.

Footnotes

1
Note that the task had the same physical specifications (the gap being of equal width, and the fitting as well as the non-fitting string of x's being of equal length) for all types of materials. As shown above, the task proved to be more difficult for negative than positive relations, but proved of equal difficulty for balanced and unbalanced materials.

2
As another reason for conducting this type of reanalysis including subjective triadic representation lies in the fact that the twelve stimulus-response pairs for the PAL task contain a confound (see Appendix A): For the balanced triads, the first name in each pair is mapped in a one-to-one fashion with either + or - as response, whereas for unbalanced triads, there is no such simple relationship between the first names and -, the symbol for dislike. Participants might capitalize on such contingencies by focusing on first names and the response, which would be a successful and unambiguous strategy supporting learning for balanced, but not for unbalanced trials. In particular, it would provide a straightforward explanation for the finding that learning is especially poor for the dislike relations in unbalanced triads. We are grateful to Karl Christoph Klauer for making us aware of this possibility.

3
The power analysis for Experiment 2a was based on the results of Experiment 2b, which, in fact, was run first. The two experiments are presented in (historically) reverse order to aid the exposition, as Experiment 2b contains an additional factor of interest.

4
We thank Klaus Oberauer for letting us have some syllogisms developed in his lab.

5
Earlier, we had run a previous version of Experiment 2b in which the response mode for participants was "fit" vs. "no fit", as opposed to what we now used in Experiments 2a and 2b, i.e., "overshoot" vs. "no overshoot." This earlier experiment did not yield any systematic results, but many participants told us that that, when responding to the spatial task, they were often tempted to conflate the notion of "fit" with the notion of logical validity. This was another reason for changing the response mode in the present experiment.

6
For example, the task might be used to test predictions of mental model theory in the context of logical reasoning (Johnson-Laird, 1983; Johnson-Laird & Byrne, 1991; more recently Rauh et al. (2005) claims a three-stage process for reasoning in the sense of drawing new inferences: (1) A phase of model construction where an integrated representation of the available information is generated, (2) a phase of model inspection where some inferences may already be drawn, and (3) a phase of model variation and search for counterexamples in case of more complex inferences. Elements that are included in the constructed model might be expected to be perceived as "closer together" to the extent that the resulting representation "makes sense" to the perceiver.

Appendix A: List of the triads used in Experiment 1

Legend: "+" means the two fictitious people like each other; "-" means they do not like each other.

Balanced:

Brian and James	-
Brian and Ken	-
James and Ken	+

Trisha and Carol	-
Trisha and Helen	-
Carol and Helen	+

Unbalanced:

Mark and Steven	-
Mark and Tom	+

Steven and Tom	+
Linda and Mary	-
Linda and Ruth	+
Mary and Ruth	+

Appendix B: List of the syllogisms used in Experiment 2a

Valid:

All pears are sappy
All sappy fruits are aggressive
→ All pears are aggressive

All apple-trees have a red mark
All trees with a red mark are conifers
→ All apple-trees are conifers

All mushrooms grow on a bush
All things that grow on a bush are sour fruits
→ All mushrooms are sour fruits

Some meat-eating animals are camels
All camels have humps
→ Some meat-eating animals have humps

Some plums are sweet fruits
All sweet fruits have sticky legs
→ Some plums have sticky legs

Some mice live in holes
All animals living in holes have green noses
→ Some mice have green noses

Invalid:

All fruits are ripe
All ripe fruits are cubic eggs
→ All cubic eggs are fruits

All healthy fruits are tiny fruits
All tiny fruits are mammals
→ All mammals are healthy fruits

All talking fruits are in the box
All things in the box are sweet cherries
→ All sweet cherries are talking fruits

All birds with four wings are at the lake
Some birds at the lake are geese
→ Some birds with four wings are geese

All peaches are soft
Some soft fruits are warm-blooded
→ Some peaches are warm-blooded

All fish live in the lake
Some animals living in the lake purr
→ Some fish purr

Appendix C: List of the syllogisms used in Experiment 2b

Valid:

Believable:

All mortals have to die
All humans are mortals
→ All humans have to die

All mothers at the party wear hats.
All people who wear hats are women.
→ All mothers at the party are women

All fruits in the crate are sour fruits
All lemons are in the crate
→ All lemons are sour fruits

Some French are members of the Pigalle Club
All members of the Pigalle Club are wine drinkers
→ Some French are wine drinkers

Some men in the meeting wear suits
All men in the meeting smoke cigars
→ Some men wearing suits smoke cigar

Unbelievable:

All things made of glass are transparent
All transparent things are colourless
→ All things made of glass are colourless

All things in Bill’s refrigerator can be eaten.
Some lightbulbs are in Bill’s refrigerator.
→ Some lightbulbs can be eaten.

No adults are in the zoo
Some doctors are in the zoo
→ Some doctors are not adults

Some magicians can conjure
No human can conjure
→ Some magicians are not humans

All old people like to go to Mt Everest.
All people who l like to go to Mt Everest are adventurous
→ All old people are adventurous.

Invalid:

Believable:

All musical instruments are in the music room
All things in the music room are trumpets
→ All trumpets are musical instruments

Some things that taste good are drinks
All liquors taste good
→ Some drinks are liquors

All forceps are tools
Some tools are metallic
→ Some forceps are metallic

All children like chocolates
Some dogs like chocolates
→ Some children like dogs

All computers are machines
Some machines have monitors

→Some computers have monitors

Unbelievable:

All violins are in the music room
Some things in the music room are drums
→ Some violins are drums

All cobras are snakes
All snakes are poisonous creatures
→ All poisonous creatures are cobras

All things with red handle are hammers
All hammers are tools
→ All tools have a red handle

All glasses are in the cupboard
Some things in the cupboard are plates
→ Some glasses are plates

All things in the box are sweet things
All lipsticks are in the box

→ All sweet things are lipsticks

Appendix D: List of the instructions, target words, and scenarios used in Experiment 3a and 3b

INSTRUCTION:
Read these groups of 3 sentences, each set describing a mini-story. Say "yes" or "no" whether you can detect a causal chain in these mini-stories. A causal chain is a sequence of ideas whereby one idea is the cause of the next one, or, a certain action leads one state into the next.

LATENT CAUSALITY (COHERENT):

1 KNIFE HOSPITAL
1. Johnny brought a knife to his workplace on a Wednesday.
2. He had always quarrelled with another man at work, Bill.
3. Bill had to be admitted to the hospital on the same day.

2 DRESS MISSING
1. Sarah was looking forward to wearing a blue dress to her school ball.
2. She couldn't afford a new dress, so decided to steal one.
3. A dress was reported missing from a department store.

3 ARGUE GLASS
1. James and Amy argued near a phone booth.
2. James became very angry and started to break things.
3. There was glass near the phone booth.

4 LOUIE FIGHT
1. Louie enjoyed the occasional alcoholic beverage.
2. He sometimes had a few too many beers at the pub.
3. There were often fights outside the pub.

5 CONCERNED THEFT
1. Rachel was concerned about a meeting with her boss.
2. She feared he knew she had stolen money from the register.
3. The next day, she confessed to recurrent theft.

6 MILLIE CRASH
1. Millie was given a car for Christmas.
2. She was a terrible driver.
3. There was a car crash on Boxing Day.

7 WOODS TRESPASS

1. One day, Richard went for a walk.
2. He climbed over a gate in the woods, that read ‘no entry’.
3. He was charged with trespassing.

8 STEVEN POLICE

1. Lisa and Steven went to a hotel for their anniversary.
2. Steven was often abusive towards Lisa.
3. The police received a call from the hotel.

9 TARA PLAGIARISM

1. Tara was struggling to finish an essay.
2. She copied work from a journal to finish in time.
3. She was suspended for plagiarism.

10 LUKE STOLEN

1. Tommy left his new bike in his driveway.
2. Tommy’s friend Luke was jealous of the bike after seeing it in the driveway.
3. The bike was stolen from a driveway.

NO LATENT CAUSALITY (INCOHERENT):

1 KNIFE HOSPITAL

1. Johnny brought a knife to his workplace on a Wednesday.
2. His friend Bill’s mother was very unwell.
3. Bill had to be admitted to hospital on the same day.

2 DRESS MISSING

1. Sarah was looking forward to wearing a blue dress to her school ball.
2. She couldn’t afford a dress, so her mother bought one for her.
3. An item was reported missing from a department store.

3 ARGUE GLASS

1. James and Amy argued near a phone booth.
2. James became angry and walked home without Amy.
3. There was glass near the phone booth.

4 LOUIE FIGHT

1. Louie enjoyed the occasional alcoholic beverage.
2. Such as the fine port he and his wife shared.
3. There were often fights outside the pub.

5 CONCERNED THEFT

1. Rachel was concerned about a meeting with her boss.
2. She was anxiously hoping for an important promotion.
3. The next day, she confessed to recurrent theft.

6 MILLIE CRASH
1. Millie was given a car for Christmas.
2. But she did not drive it for another week.
3. There was a car crash on Boxing Day.

7 WOODS TRESPASS
1. One day, Richard went for a walk.
2. He helped his daughter pick flowers afterwards.
3. He was charged with trespassing.

8 STEVEN POLICE
1. Lisa and Steven went to a hotel for their anniversary.
2. Steven proposed that weekend.
3. The police received a call from the hotel.

9 TARA PLAGIARISM
1. Tara was struggling to finish an essay.
2. But in the end, she got all the citations correct.
3. A student was suspended for plagiarism.

10 LUKE STOLEN
1. Tommy left his new bike in his driveway.
2. His father Luke nearly drove over it when he got home so Tommy was sent to bed early.
3. A bike was stolen from a driveway.

Table 1 Experiment 1: Accuracy for relational signs (+ or –), across 13 PAL-learning trials.

Type of triad

Type of

relation	balanced		unbalanced	
positive	.79	(.25)	.76	(.23)
negative	.75	(.26)	.52	(.27)

Legend:

Proportions correct, standard deviations in brackets.

Table 2 Experiment 1: Accuracy at spatial task (*fit* vs. *no fit* responses), across 10 PAL-learning trials with spatial component.

	Type of triad			
Type of relation	balanced		unbalanced	
positive	.63	(.12)	.62	(.09)
negative	.59	(.07)	.57	(.10)

Legend:

Proportions correct, standard deviations in brackets.

Table 3 Experiment 1: Tendency to say *fit* vs. *no fit*, across 10 PAL-learning trials with spatial component.

	Type of triad			
Type of relation	balanced		unbalanced	
positive	.43	(.19)	.48	(.20)
negative	.39	(.16)	.41	(.17)

Legend:

Proportions of *fit* responses, standard deviations in brackets.

Table 4. Experiment 2b: Accuracy at validity task (*valid* vs. *invalid* responses), across 4 evaluation trials (20 syllogisms, 5 in each category).

Conclusion	Validity			
	valid		invalid	
believable	.89	(.09)	.69	(.18)
unbelievable	.86	(.14)	.84	(.19)

Legend:

Proportions correct, standard deviations in brackets.

Table 5 Experiment 2b: Accuracy at spatial task (*overshoot* vs. *no overshoot* responses), across 4 evaluation trials (20 syllogisms, 5 in each category).

Conclusion	Validity			
	valid		Invalid	
believable	.59	(.15)	.61	(.13)
unbelievable	.58	(.15)	.61	(.14)

Legend:

Proportions correct, standard deviations in brackets.

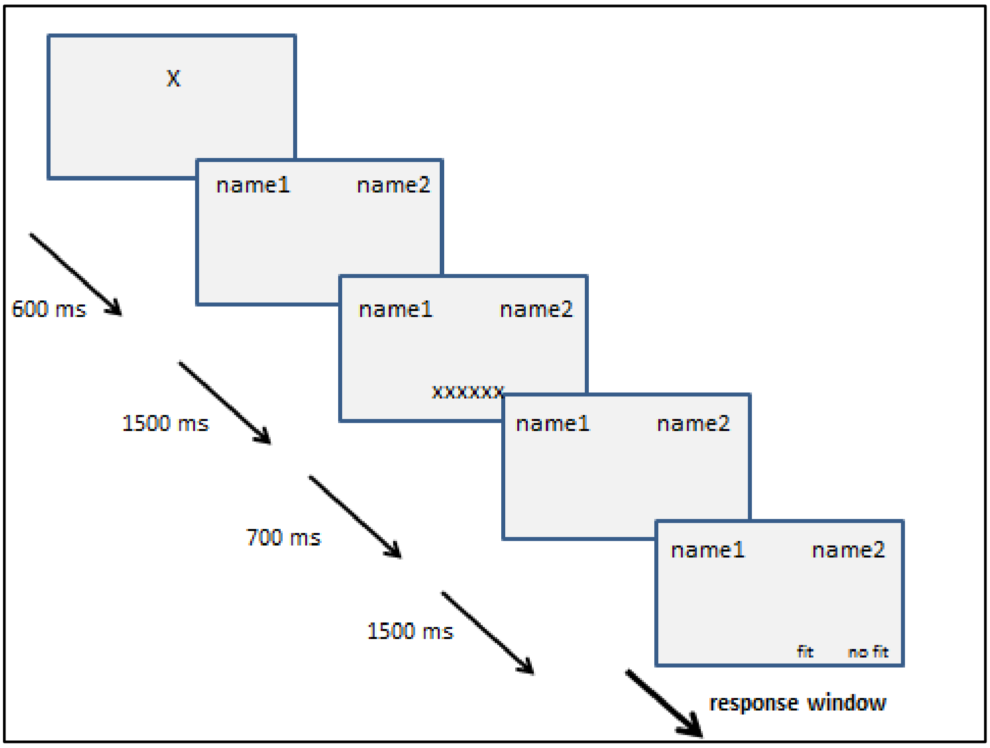
Table 6 Experiment 2b: Tendency to say *overshoot* vs. *no overshoot*, across 4 evaluation trials (20 syllogisms, 5 in each category).

Conclusion	Validity			
	valid		invalid	
believable	.44	(.20)	.48	(.20)
unbelievable	.42	(.18)	.49	(.16)

Legend:

Proportions of *no overshoot* responses, standard deviations in brackets.

Figure 1



Legend: Experiment 1, sequence of screen presentations as part of the spatial task.

Response keys were the left and right arrow keys (see text)