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ABSTRACT

Reframing is key to mitigating the risks of implicit and inaccurate assumptions when dealing with complex, open-ended problems. While behavioural designers regularly face such problems, reframing is overlooked in current behavioural design guidance. Therefore, there is a need to better understand and demonstrate reframing's potential impact in behavioural design. We address this need via an exploratory, controlled experiment with design-engineering students responding to a complex, open-ended problem with a significant behavioural component. We evaluate the impact of three reframing stimuli against a control, measured with respect to behavioural design quality. The three stimuli included a *structure*-only stimulus (sequential steps of actions), a *content*-only stimulus (unordered prompting questions), and a combined *structure-content* stimulus. To evaluate behavioural design quality, we conduct a mixed-methods assessment of design outputs at different points in the design task: ideation of possible problem-solution perspectives, mindmapping, and proposition of a final solution (concept). Our findings confirm that all three stimuli are effective in increasing behavioural design quality, with increased emphasis on behavioural aspects and enhanced integration of behavioural and technical aspects of problems and solutions. This contributes to understanding the importance of reframing in developing problem-solution understanding in behavioural design, with significant implications for theory and practice.

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Behavioural design; reframing; framing; experiment; problem-solution space

1. Introduction

Behavioural designers deal with complex problems and solutions as they aim to tackle challenges such as health and sustainability (Niedderer, Clune, and Ludden 2017; Nielsen et al. 2024; OECD 2017). Observable symptomatic behaviour is often rooted in a network of intertwined problem causes and diverse perspectives and interests of actors, all embedded in complex physical and social contexts (Schmidt 2024; Tromp and Hekkert 2018). Further, as interventions often comprise multiple elements that must work together to facilitate

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behaviour change (Maier and Cash 2022; Tromp, Hekkert, and Verbeek 2011), the number of possibilities increases substantially across the entire behavioural design space (Nielsen et al. 2021). Hence, problem scoping and navigation in the problem-solution space are challenging and cannot be easily handled by a single approach (Funke 2021). Behavioural designers often need to consider and combine diverse guidance when designing interventions. For example, this includes overall strategies and Behavioural Change Techniques (BCTs) (Michie, Atkins, and West 2014), as well as lists of conventional design resources such as materials, colours, and linguistics (Cash et al. 2017a; Doss et al. 2019; Folk and Remington 2008). However, despite a growing body of guidance, behavioural designers still face significant challenges when dealing with complex, open-ended problem-solution spaces, where neither space can be easily defined upfront, and with numerous possible routes to their alignment.

While traditional deductive approaches struggle to tackle complex, open-ended problems, design thinking has been widely recognised as having potential in such contexts (Bender-Salazar 2023; Buchanan 1992). At the core of design thinking, framing and reframing form fundamental principles, which help designers to challenge, reformulate, and iterate current problem-solution understanding (Dorst 2011). Hence, reframing is a critical means of exploring and aligning problem and solution spaces. Yet, reframing is neither widely reported in behavioural design projects (Nielsen et al. 2024) nor explicitly incorporated in prominent behavioural design guidance where prior design research might suggest it would be relevant, such as the Medical Research Council's intervention development framework (Shahsavari et al. 2020) and Michie, Atkins, and West's (2014) eight step intervention development process. Thus, there is a need to understand and demonstrate the potential of integrating reframing as an explicit component in behavioural design.

While design research in recent years has moved towards more strict reporting of methodological decisions to increase the robustness of research findings (Cash, Daalhuizen, and Hay 2022a), behavioural science builds on a strong tradition of traceability and transparency achieved through theory-based clinical trials. Behavioural science focuses on internal validity and replicability, and is often conducted and reported following relatively inflexible, standardised, deductive processes, methods, and checklists such as TiDiEr (Hoffmann et al. 2014) and APEASE (Michie, Atkins, and West 2014). This overall deductive approach is often at the expense of applying and reporting more flexible aspects of design abduction, which – none-the-less – are required in behavioural design projects (Nielsen et al. 2024). At the same time, behavioural design work is often conducted in teams of domain specialists, e.g. clinical therapists, with design as an outsourced task (Nielsen et al. 2024). As a result, design work is often treated as a separate ecosystem, primarily concerned with technical functionality and form giving–i.e. embodied design–first relevant after deductive treatment of behavioural problems and solutions. However, a substantial number of studies report that embodied design features have a direct impact on human behaviour (Cash et al. 2017a; Doss et al. 2019; Folk and Remington 2008), highlighting the relevance of considering potential design features and their behavioural impact as part of a more abductive treatment of behavioural problems and solutions. Thus, while it is relevant to integrate reframing as an explicit component of behavioural design, first, it is necessary to understand the abductive and flexible features of reframing in a systematic and transparent way.

While research has already shown the importance of framing in conceptual development (Valkenburg and Dorst 1998), numerous authors call for further research into framing and reframing (Pee, Dorst, and van der Bijl-Brouwer 2015) in relation to creativity and abductive reasoning in intervention design (Hoddinott 2015), as well as method usage across design in general (Lavrsen, Carbon, and Daalhuizen 2025) and in the early stages of behavioural design (Duarte and Daalhuizen 2023). In responding to this need, we evaluate the impact of reframing in behavioural design via an exploratory experiment. In this experiment, we apply a complex, open-ended design brief on the consequences of high meat intake with a significant behavioural component. Through the experiment, we show that two essential reframing elements, *structure* and *content*, both combined and individually, increase behavioural design quality. These results have implications for behavioural design as well as wider design and behavioural science research.

2. Theoretical background

Before we can evaluate the impact of reframing, we must first outline how we define frames, framing, and reframing, as well as their relevance to behavioural design.

2.1. Frames, framing, and reframing

Initial problem scoping frames the first implicit and explicit directions for solutions (Peters 2005). As such, there is an increased risk of framing the problem superficially when scoping is done early in the design process and not updated or done independent from exploration of the solution space (Dorst and Cross 2001). This leads to predetermined implications and solution directions, with a large portion of the solution space cut-off from subsequent exploration. However, as complex, open-ended problems cannot be easily defined upfront (Fischer, Greiff, and Funke 2011) initial problem framing typically only provides a fraction of possible perspectives. As such – especially when working with complex, open-ended problems – designers risk compromising solution quality if they base their problem-solving on superficial problem understanding. To decrease this risk, designers actively reframe through making abductive inferences. This has been demonstrated to be especially relevant to tackling ill-defined, open-ended, and ambiguous problems across fields including design, innovation, business, and society (Paton and Dorst 2011; Pee, Dorst, and van der Bijl-Brouwer 2015). In this context, multiple authors have sought to present a nuanced understanding of framing processes linked to both tacit knowledge and abductive inference types (Dorst and Cross 2001; Koskela, Paavola, and Kroll 2018; Roozenburg 1993; Valkenburg and Dorst 1998). While Goffman primarily defined frames as cognitive shortcuts to make sense of complex situations (Goffman 1981), frames have since been described as implicit assumptions that influence the relevance of problem elements, values, and goals as well as evaluation criteria to assess solution meaningfulness (Hey, Joyce, and Beckman 2007). As such, frames, framing, and reframing link to sense-making, which is concerned with exploring underlying problem causes, and to making abductive leaps (Roozenburg 1993), which is itself concerned with creating value in original and fruitful ways.

While framing happens both implicitly and explicitly, reframing can be used as an explicit strategy to mitigate the risk of implicit, inaccurate, or unfruitful assumptions and directions (Dorst 2015; Maher, Poon, and Boulanger 1996). One of the most well-known conceptual frameworks for this is Dorst's (2011) frame creation equation: WHAT (thing) + HOW (working principle) leads to VALUE (aspired), with the frame coupling HOW and VALUE. Here, frame creation serves as a way of understanding the problem by developing a novel standpoint from which it can be solved (Dorst 2015). To this end, Dorst (2015) proposes a nine-step frame creation method for moving beyond implicit or premature frames. These are often taken for granted in conventional problem-solving approaches as they typically prescribe deductive approaches and inflexible problem frames (Dorst 2015). While there is an ongoing discussion on how to define, categorise and differentiate frame types, scholars typically recognise 'problem' and 'solution' frames (Dong, Kleinsmann, and Deken 2013; Stompff, Smulders, and Henze 2016) as distinct yet interconnected (Dorst and Cross 2001). As such, problems and solutions can conceptually be treated as 'two sides of the same coin', where frames can be understood as a perspective on problems, solutions, or both.

2.2. Reframing in behavioural design

As behavioural design deals with complex, situated, and open-ended problems, the problem-solution space is closely linked, with numerous routes to potentially effective solutions (Cash et al. 2020). Here, the first observable symptomatic behaviour informing initial problem framing is often rooted in a network of intertwined potential problem causes (Khadilkar and Cash 2020; Tromp and Hekkert, 2018). For example, inadequate sorting of waste is possibly rooted in inadequate sorting facilities, confusing sorting rules, low willingness to adhere to sorting guidelines when neighbours do not adhere, a mix of the above, or other unknown sources. While all can potentially cause inadequate sorting, they imply different logical routes to meaningful solutions. Thus, superficial exploration of the problem-solution space increases the risk of ineffective intervention. In response to the risk of superficiality, design thinking comprises cyclic and iterative problem-solving principles emphasising abductive approaches with continuous incorporation of new knowledge, prototyping and testing of assumptions, and explicit reframing (Dorst 2011; Dorst and Cross 2001). As such, reframing is essential to creating effective interventions through meaningful interaction between people, physical contexts, and interventions (Khadilkar and Cash 2020; Nielsen et al. 2021; Tromp and Hekkert, 2018).

However, while framing is inherent in all problem scoping, and implicit reframing does happen in behavioural design projects (Cash, Hartlev, and Durazo 2017b; Khadilkar and Cash 2020), it is rarely explicitly documented (Nielsen et al. 2024). While behavioural design traditionally applies problem-first, linear problem-solving approaches (Cash et al. 2022c; Nielsen et al. 2024), reframing requires a dynamic problem-solving approach (Van Boeijen, Daalhuizen, and Zijlstra 2020) allowing co-evolution of problem- and solution-understanding through dynamic and systematic exploration (Dorst and Cross 2001). At the same time, behavioural design projects often apply and report standardised problem→solution approaches emphasising traceability and internal validity with the consequence of black boxing design decisions and initiatives dealing with the inevitable uncertainty linked to behavioural design problem-solving (Nielsen et al. 2024; Patton, Ryan, and Hughes 2020). Thus, while reframing is a generally established and accepted design approach (Paton and Dorst 2011; Stompff, Smulders, and Henze 2016), there is a specific need to evaluate the impact of reframing on problem and solution understanding in behavioural design.

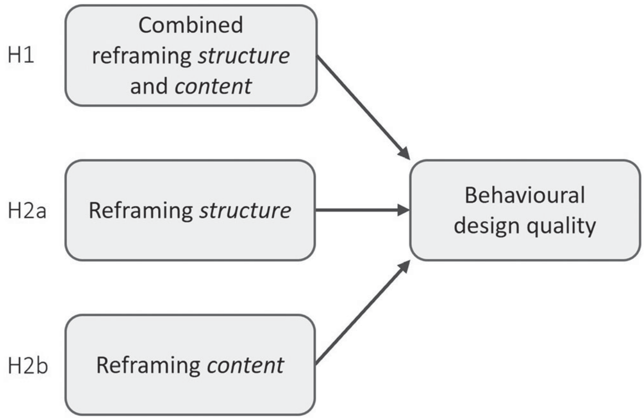

Figure 1. Conceptual framework of reframing constituting two individual and combined key elements, *structure* and *content,* informed by 'procedure' and 'mindset' from method content theory (Daalhuizen and Cash 2021) and its logical impact on behavioural design quality.

3. Conceptual framework and hypothesis

Following prior literature, we operationally define frames as perspectives on problems, solutions, or both and reframing as the process of changing perspective. Here, comparing shifts in perspective provides a means to evaluate the impact of reframing (Paton and Dorst 2011; Stompff, Smulders, and Henze 2016). However, given the current lack of strong reframing theory or extensive empirical work in theory testing mode, we are constrained to logically deriving hypotheses based on existing conceptual reframing research (Dorst 2011; Dorst and Cross 2001; Koskela, Paavola, and Kroll 2018; Stompff, Smulders, and Henze 2016) and method content theory (Daalhuizen and Cash 2021). Thus, we adopt an exploratory approach.

Following method content theory, the basis for the internal logic of methods relies on three elements: method 'mindset' and a combined method 'procedure' and 'goal'. Here, mindset is defined as '*the set of described values, principles, underlying beliefs, and logic that inform a method and its use*' (Daalhuizen and Cash 2021, 8). As such, mindset refers to how designers interpret and respond to the content of the method. Next, procedure is defined as '*the structural activities described in the method and their relative chronological and logical ordering*' (Daalhuizen and Cash 2021, 9). Lastly, goal is defined as '*the described goals and the prioritisation of those goals a method aims to help achieve through its use*' (Daalhuizen and Cash 2021, 8). Hence, to operationalise reframing (i.e. changing perspective on problem/solution) in a testable way in relation to behavioural design, mindset and procedure form key elements informing our conceptual framework (Figure 1). For simplicity, we refer to mindset as '*content*' and procedure as '*structure*'.

Most reframing guidance combines both content and structure (Dorst 2015; Hekkert and Van Dijk 2011), i.e. offering both suggested stages and steps as well as, for example, beliefs about what constitutes a good frame. Yet most of the evidence for the impact of this guidance is based on conceptual work, or anecdotal or case-based accounts of application that typically reflect holistic implementation (Tromp and Hekkert 2016). As a result, there is little reliable insight into these mechanisms of reframing. Notably, there are almost no theory testing mode (Cash 2018) studies of reframing or interventions targeting reframing based on strong theoretical explanations as opposed to general heuristics. Hence, while we might expect guidance that combines structure and content to elicit more effective reframing and consequently to lead to more appropriate behavioural interventions (van Arkel and Tromp 2022), it is far from given. This is especially salient considering the generally abstract nature of current theory, where the relationship between reframing and improved outcomes is often characterised at the overall level and hence does not explain causal mechanisms related to the embodiment of the reframing guidance itself, let alone to its effect on the designed interventions inspired by it. Thus, the essential, central hypotheses of our experiment is that combined guidance on structure and content will increase behavioural design quality:

H1: A *combined structure-content* reframing stimulus will increase behavioural design quality

While there is at least a credible empirical foundation for our expectations of increasing behavioural design quality via a combined structure-content stimulus (although this has not been widely tested or strongly theorised as outlined above), the effects of individual elements are much less well understood. That is, both *structure* and *content* are known to form essential elements of design guidance in general (Lewrick, Link, and Leifer 2018; Van Boeijen, Daalhuizen, and Zijlstra 2020), yet their individual impact on or mechanism of action remains little theorised, particularly in the context of behavioural design. Similarly, in the more general context of design, it has been argued that both design mindset and design process offer critical guidance on impactful and effective design work (Daalhuizen et al. 2019; Hay et al. 2017). Reframing guidance is conceptually similar to many other types of creative or design thinking guidance, so it is plausible that *structure* and *content* will both play distinct individual roles here also. Hence, in our experiment, we treat structure and content as two separate elements, each of which contributes to reframing individually and in combination. Following this, we introduce two secondary, supporting hypotheses, H2a and H2b, to evaluate the separate effects of *content* and *structure*:

H2a: A *structure* reframing stimulus will increase behavioural design quality

H2b: A *content* reframing stimulus will increase behavioural design quality

Our three hypotheses enable us to understand how these different elements constituting reframing guidance, individually and combined, contribute to reframing's impact on behavioural design quality. Here, we refer to behavioural design quality as the level to which behavioural aspects of complex, open-ended design problems are reflected and integrated with non-behavioural aspects such as technical mechanisms. The direct and indirect behavioural impact of both behavioural and non-behavioural aspects affects the intervention appropriateness, i.e. the experiential variable moderating intervention effectiveness through aesthetic, moral, and contextual fitness between people and solutions (van Arkel and Tromp 2022). Thus, in evaluating reframing's impact on behavioural design quality, we consider a high emphasis on behavioural aspects and a high integration of behavioural and non-behavioural (e.g. technical) aspects to indicate high behavioural design quality.

4. Method

Due to the limited scope of prior theory, but the extent of empirical accounts of design guidance, we used an exploratory controlled experiment to evaluate the impact of reframing stimuli on behavioural design quality. The following sub-sections present considerations concerning method choice, sample characteristics, experimental conditions, setup, and data collection.

To reduce complexity and increase robustness, we have drawn on creativity and facilitation studies (Sääksjärvi and Gonçalves 2018; Wróbel, Lomberg, and Cash 2021) and adapted approaches and measures according to relevant reframing and design theory. The overall experimental design was based on best practice guidance for exploratory studies and double-blinded controlled trials. In this we prioritised internal validity in sampling and study design, whilst also considering external validity issues in the formulation of the brief, design tasks, and measures. Thus, the participants did not know the direct purpose of the experiment (evaluating reframing in behavioural design), and the first author present during data collection did not know which participant was assigned to which condition.

4.1. Sample considerations

To delineate our sample, we use the sampling considerations described by Cash et al. (2022b). To evaluate general hypotheses on the impact of reframing on behavioural design quality, where our main aim is contributing to knowledge, we prioritise scientific concerns aiming at internal validity within the sample group. Here, to increase internal validity and comparability of assessment, we prioritise a homogenous sample with similarity of participant background, experience level, and ability to follow design task instructions. To these ends, students are ideal for isolating causal effects, as they are both relatively homogeneous and capable of following complex study designs (Bello et al. 2009). Further, we prioritise internal statistical validity, where best practice for social research recommends a sample size of 20–40 participants (Cash et al. 2022b; Onwuegbuzie and Collins 2007). Based on these criteria, we used design-engineering students with a minimum mid-bachelor educational level as experiment participants and aimed at least 20 participants in each condition.

4.2. Sample characteristics

In total, 70 design-engineering students participated in the experiment. To ensure sample quality, the first author screened and excluded seven sample units using the inclusion criteria in Table 1. The seven exclusions include three participants arriving after the experiment had started, one participant using a self-chosen design brief, two participants using 'plant growth' provided as the example in the experiment material instead of using the provided design brief on meat intake, and one participant leaving an entire design task section blank.

After exclusion, our sample consisted of 63 participants, of which 33 were female. The participants reported an average age of 23 and 3–4 years of design-engineering education. 64% (= 40 participants) reported being enrolled at the Design & Innovation engineering education, 20% (= 13 participants) reported being enrolled in related engineering educations, and 16% (= 10 participants) did not specify a specific engineering education. However, at the time of participation all 63 participants was enrolled in a Design & Innovation engineering course. The 63 participants were distributed under the different conditions as follows:

Table 1. Inclusion criteria.

No.	Inclusion Criteria (IC)	Excluded
IC1	Student must participate in the entire experiment from start to end	3
IC2	Student must use the provided design brief as the starting point	3
IC3	Student must actively generate data in each design task in the booklet	1

Table 2. Overview of the three reframing stimulus conditions.

Reframing stimuli conditions		Reframing elements	
	Structure	Content	
H2a	Structure stimulus	Yes	No
H2b	Content stimulus	No	Yes
H1	Structure-content stimulus	Yes	Yes

Table 3. Reframing stimuli elements (*structure* and *content*) overview.

Reframing stimuli elements	
Structure: three process steps	*Content*: six questions
1) Generate frame – come up with frame	What might different underlying causes of the problem be? What different solution principles might realise an effect?
2) Detail frame – elaborate frame	What different overall perspectives (e.g. metaphors, analogies) might help to understand the problem? What different perspectives (e.g. metaphors, analogies) might inspire solutions and/or solution principles?
3) Use frame – come up with solution	What might different desired effects be? What might different solution ideas be?

- Structure stimulus: 14 participants (= 22%)
- Content stimulus: 15 participants (= 24%)
- Structure-content stimulus: 7 participants (= 27%)
- Control: 17 participants (= 27%)

4.3. Conditions

In testing our hypotheses (Section 3), following method content theory (Daalhuizen and Cash 2021), we built three stimuli conditions comprising essential reframing elements: *structure*, *content*, as well as a combination of both in a *structure-content* stimulus as described in Table 2.

Our structure stimulus was informed by generally accepted studies of reframing processes (Stompff, Smulders, and Henze 2016) and Dorst's (2011) frame equation, while our content stimulus was informed by generally accepted reframing questions–that is–questions aimed at prompting abductive reasoning (Dorst and Cross 2001; Koskela, Paavola, and Kroll 2018), see Table 3.

We also designed a control condition based on general design thinking theory comprising both generic structure and content elements using the first three steps of the double diamond combined with generic design thinking questions (Table 4). As such, the control condition serves as a suitable baseline for the main hypothesis. Following best practice in this context, we elected to use this 'placebo' type control as opposed to a simple 'no-stimuli'

Table 4. Control condition overview.

Control condition	
Structure: first three double diamond steps	Content: six generic design questions
1) Discover	What might explain the problem? What might the challenge be?
2) Define	How might the problem be defined? What opportunities might exist?
3) Develop	What might solutions be? How could the problem be solved?

control in order to account for experimental affects derived from attention, workload, expectancy of success etc. (Adair, Sharpe, and Huynh 1990; Gephart and Antonoplos 1969).

See appendix A1 for the final design of the three reframing stimuli and the control condition. Note, that while Table 1 shows the six questions in logical order, they are deliberately presented unordered in the content-only stimulus design.

4.4. Experiment setup and data collection

The first author collected the data on February 15th and March 1st 2023, at the Technical University of Denmark. To maintain consistency and minimise confounding experimental effects the study was conducted in a lecture room. Here, we asked the participants to sit individually and work with a sequence of consecutive design tasks using the instructions in a provided booklet. Before the experiment started, the first author welcomed the participants and introduced the formal experiment setup. To ensure proper ethical conduct, the introduction explicitly emphasised voluntary participation, anonymising data, and that the data was collected for research purposes only, participants then gave consent to join the study.

In the experiment, we asked the participants to think of themselves as designers hired to work on a design assignment presented as an initial design brief. To create a fitting–that is–relevant, relatable and motivational design brief, in designing the experiment, we did a pre-test with five students to examine the students' responses to three different briefs, including cigarette waste, alcohol intake, and meat consumption. Based on their responses and advice, we chose to apply the meat intake brief to our final experiment design. As such, all participants in the experiment were provided with an identical design brief regarding meat intake (Figure 2) formulated as an ill-defined, open-ended problem statement with an obvious behavioural component reinforced by including a picture of people eating meat in a social setting. The problem elements provided are based on facts from real-world data and case examples to increase ecological validity and relevance.

After consent, we asked the participants to follow the general instructions for finishing the design tasks provided in a booklet from start to end, working individually in silence without leaving the room. Each student got a booklet (Appendix A2), supported by verbal and visible time management instructions in a timed power point slide deck. The booklet and script for the verbal and visible instructions had undergone extensive pre-testing to minimise possible facilitation bias and improve internal validity. Further, we informed the participants that only 'technical' questions such as 'Shall I continue to the next page or wait' but no content related questions such as 'Is this good enough' would be answered during

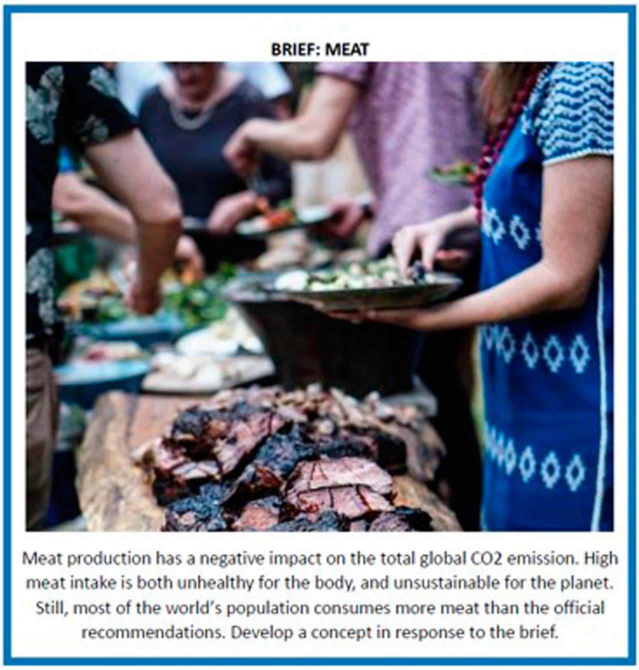

Figure 2. Ill-defined, open-ended problem brief on consequences of high meat intake provided as the starting point for the design tasks in the experiment.

the experiment. Lastly, we emphasised that there were no right or wrong answers to any of the design tasks, and participants were encouraged to answer the tasks to the best of their ability and to follow the instructions provided in the experiment material. All materials were identical across the entire sample except for the stimuli/control conditions conveyed as 'a model'. We instructed the participants to use the model actively to help them respond to the design tasks. Each booklet contained one of four models comprising either one of the three reframing stimuli or the control condition.

The experiment started when the first author pressed play on the power point. To ensure that the participants had actively engaged with the provided stimuli/control, first in the booklet, we instructed the participants to confirm reading the provided model in the booklet. Next, to help the participants get into problem-solving mode, we instructed them to read the initial design brief and reformulate it into a design goal using their own words. Then, following these two initial tasks, the main experiment comprised a sequence of five individual design tasks, including frame generation, frame arrangement, concept development, concept elaboration, and self-assessment (Figure 3). To complete each task, we gave the participants a fixed number of minutes, between 7 and 10 min. The experiment took 55 min in total, excluding a short introduction and debriefing.

5. Data preparation and analysis

As mentioned in Section 4.2, our sample comprised 63 participants after exclusions. Even though our sample size was lower than the recommended minimum of 20 participants in

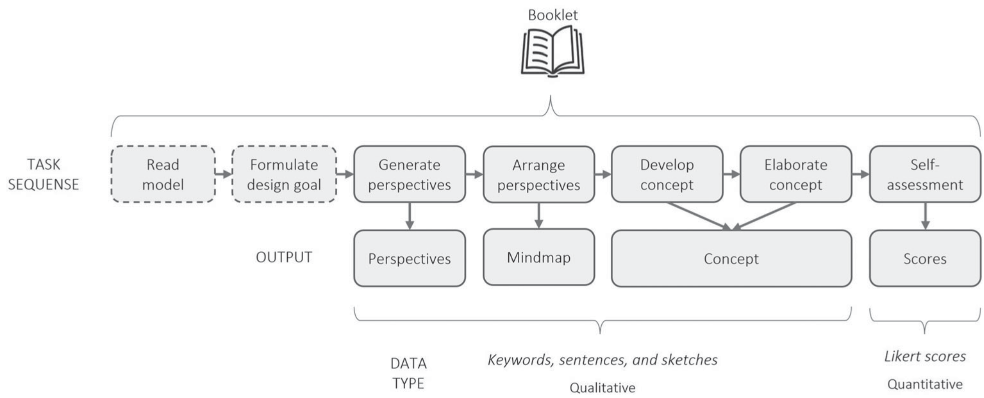

Figure 3. Overview of the sequence of design tasks and their outputs.

each condition, we chose not to risk the introduction of bias or compromise the integrity of the study by seeking additional participants at this point. This is in line with best practice and the sample is still appropriate for the exploratory analysis in this study. Future confirmatory work can use our results to conduct confirmatory power analysis and sample definition, but as this was not possible for this exploratory study, we proceeded with the sample as collected. In making this trade-off, and following best practice for exploratory studies, we pursued data preparation and analysis with a qual→quant approach with low expectations of significant statistical results (Onwuegbuzie and Collins 2007).

As a practical means, and to decrease handwriting bias to a minimum, the first author typed in the raw data into Excel, divided into four main data groups: (1) *perspectives*, (2) *mindmap*, (3) *concept*, and (4) *scores* (self-assessment) with the first three groups representing key intermediate design outcomes of a co-evolutionary process. Here, the first author grouped the raw data according to the output from each design task except for *concept*, which was informed by the collective output from concept development and concept elaboration (Figure 3). We first present data preparation and analysis of direct performance measures across the *perspectives, mindmap*, and *concept* data groups. Second, we present data preparation and analysis of indirect performance measures from the *score* data group.

5.1. Perspectives

From the first task, perspective generation, the total number of perspectives ranged from 102 in the process stimulus condition to 111 in the combined process-content stimulus condition. The perspectives included keywords, sentences, sketches, or a combination. All perspectives were included in the sample. Figure 4 shows an example of two generated perspectives.

5.2. Mindmap

From the second task, perspective arrangement, the total number of arranged perspectives ranged from 191 in the combined process-content stimulus condition to 212 in the control condition. As the participants were allowed to add any new perspectives coming to mind when arranging already generated ones, the total number of perspectives

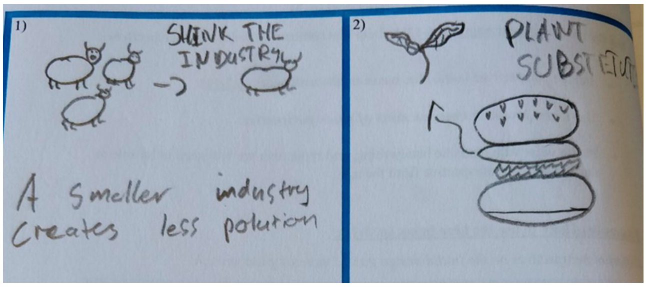

Figure 4. Example of two generated perspectives.

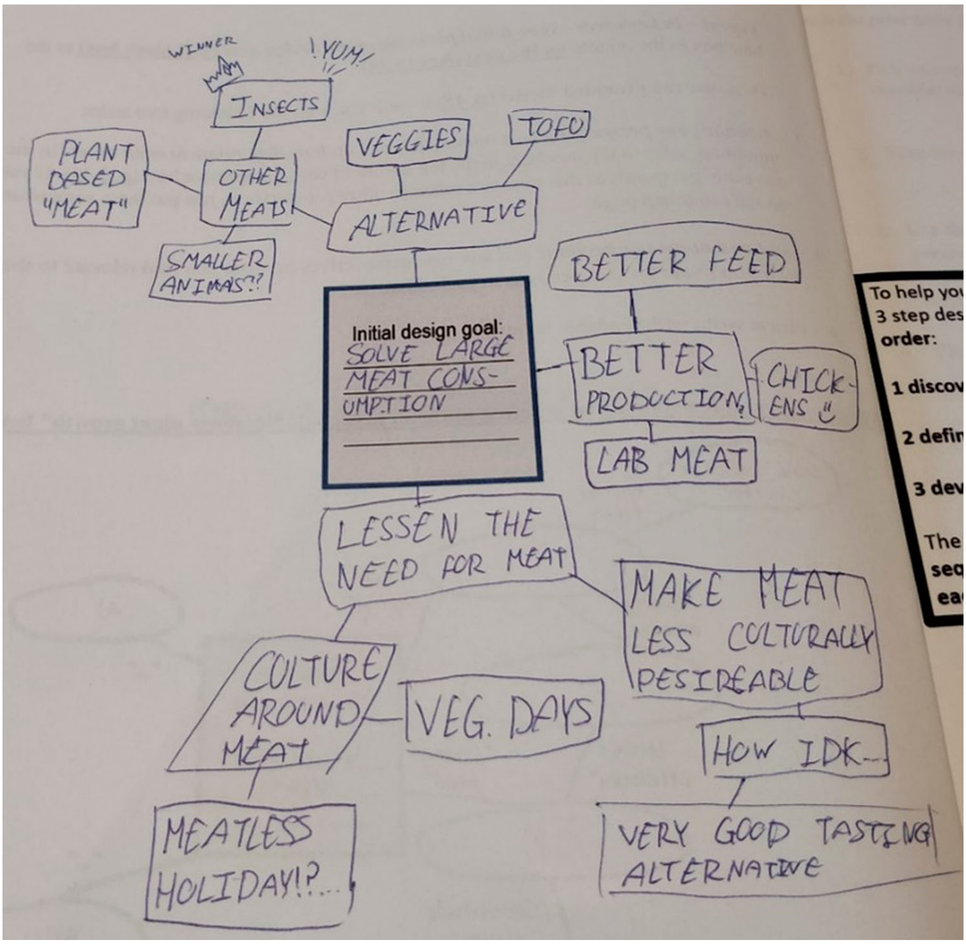

Figure 5. Example of perspectives arranged in a mindmap.

arranged in the mindmaps increased. In typing in the raw data, the first author excluded 82 words/sentences from the sample–that is, they were not identified as perspectives–as they comprised 'neutral' labels such as writing 'economy' next to a group of perspectives dealing with economic incentives, or sound words like 'yum' as seen in the provided example. Apart

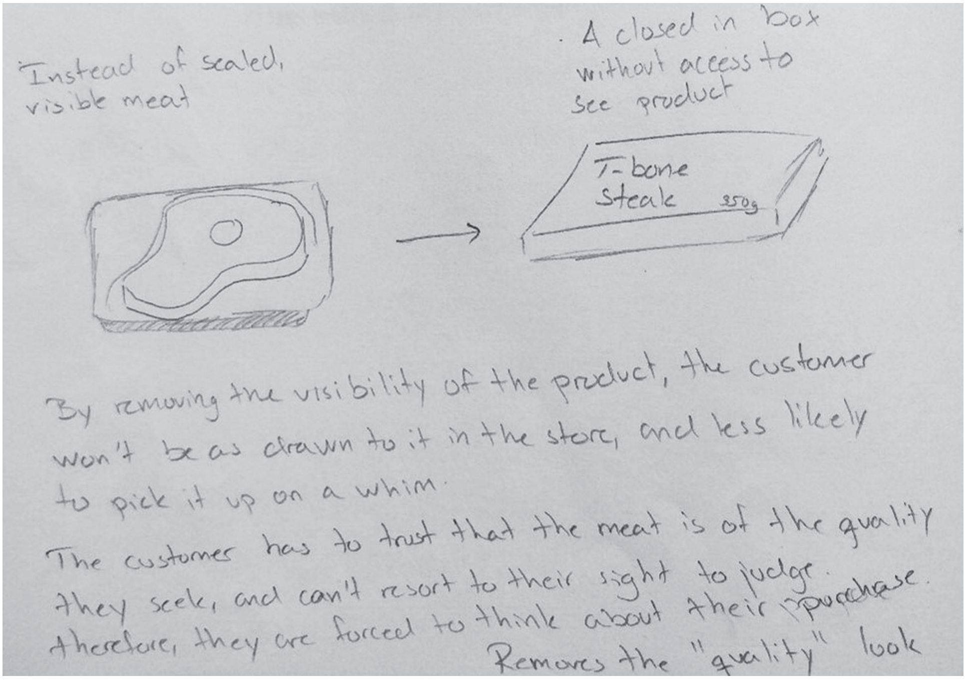

Figure 6. Example of concept.

Table 5. Overview of raw data from the three data groups *perspectives, mindmap*, and *concept*.

	Perspectives	*Mindmap (arranged perspectives)*		*Concept*
Stimuli/control condition	Total no. of perspectives	Total no. of perspectives arranged in mindmap	Total links between perspectives	Total no. of concepts
Control	103	212	178	17
Structure	102	195	176	14
Content	106	198	176	15
Structure-content	111	191	151	17

from the total count of arranged perspectives, we also identified the total number of links between perspectives. The total number of links ranges from 151 in the combined process-content stimulus condition to 178 in the control condition. Figure 5 shows an example of perspectives arranged in a mindmap.

5.3. Concept

From the third and fourth tasks, concept development and elaboration, the total number of concepts equals the total number of participants in each condition, as we instructed each participant to develop and elaborate only one concept. All concepts were included in the sample. Figure 6 shows an example of a concept.

Table 5 shows an overview of the raw data across the three groups: *perspectives, mindmap*, and *concept*.

After preparing the data for further analysis, the raw data in the first three groups, *perspectives, mindmap*, and *concept,* were qualitatively analysed through categorical analysis in

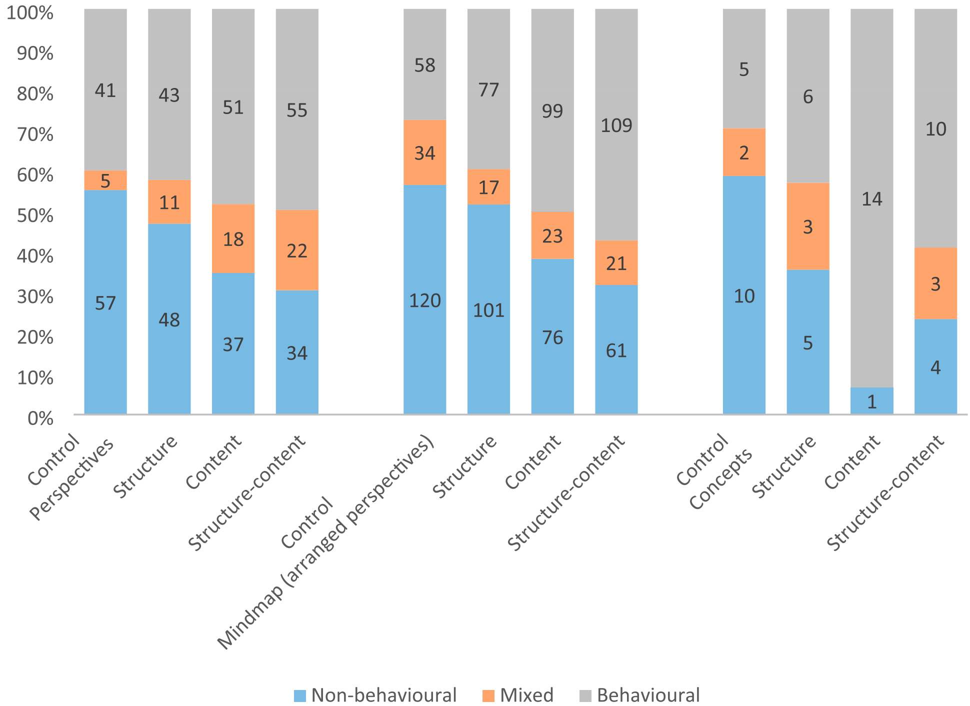

Figure 7. Overview of the total number of perspectives and concepts across there categories *non-behavioural*, *mixed*, and *behavioural* aspects.

multiple rounds. Here, the first author coded each perspective, each arranged perspective in mindmaps, and each concept on only one of four coding categories, *non-behavioural*, *behavioural*, or *mixed*, guided by the definitions below:

- *Non-behavioural:* emphasising STEM aspects, including science, technology, engineering, and mathematic mechanisms, or policy strategies directed at national or international meta-level.
- *Behavioural*: emphasising psychological, behavioural aspects such as individual or interpersonal mechanisms.
- *Mixed*: emphasising aspects with equal interplay between *non-behavioural* and *behavioural* mechanisms.

The research team discussed the coding until they reached a collective agreement. Figure 7 presents the total number of perspectives, arranged perspectives, and concepts in each category, and Figure 8 presents the total number of links between non-behavioural (NoBe), Behavioural (Be), and Mixed (Mix) perspectives arranged in mindmaps.

5.4. Scores

In the self-assessment task, we instructed participants to assess their generated outcome by scoring multiple statements on a 7-point Likert scale, as this is a convenient way to measure

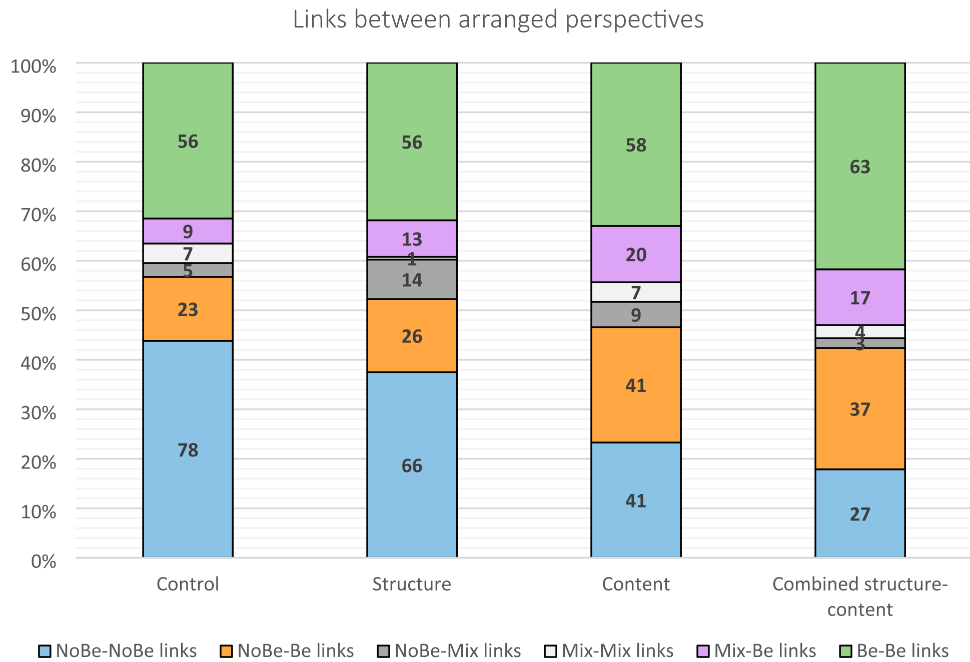

Figure 8. Overview of the total links between non-behavioural, mixed, and behavioural perspectives arranged in mindmaps.

unobservable constructs like perception of performance and is easy for participants to complete (Jebb, Ng, and Tay 2021). As there–to our knowledge–are no validated Likert scales for evaluating creative output in connection to reframing, we built our scales from three categories following Sääksjärvi and Gonçalves (2018) including novelty, meaningfulness, and usefulness. Each category comprises 3–5 questions targeting self-assessment on generated output on a scale of 1 (low agreement) to 7 (high agreement). See the booklet in Appendix A2 containing the questions.

The first author manually inputted all scores into Excel. Across our entire sample, only four individual self-assessment questions were unanswered. These comprise three cases of novelty questions and one case of a meaningfulness question. The absent scores occurred across the stimuli/control conditions except for the process stimulus condition, where all individual self-assessment questions were answered. In cases of missing answers, we followed standard Likert scale practice for absent data, calculating the absent score as the average of the remaining inputted scores within the specific category. For example, one of our missing data cases was in the novelty category comprising three questions: question one scored 5, question two scored 4, and the missing score was calculated as the average of 4.5. In preparing the data for analysis, before calculating each participant's mean novelty, meaningfulness, and usefulness score, we checked the correlation between self-assessment questions within each category across the entire sample using Pearson's Correlation suitable for interval data (see Appendix A3). The average correlation of the three novelty questions, four meaningfulness questions, and five usefulness questions were respectively calculated to $r(61)_{Novelty} = .56$, $p = .002$, $r(61)_{Meaningfulness} = .42$,

$p = .011$, and $r(61)_{usefulness} = .39$, $p = .028$. As such, the questions correlate, and we can apply the means for the findings elaborated in the findings section.

6. Findings

We present the findings at three levels. First, we present the findings in connection with our main hypothesis concerning the impact of the combined *structure-content* reframing stimulus on behavioural design quality. Second, we elaborate on these findings by unpacking them in connection to our two sub-hypothesis concerning the impact of the individual *structure* and *content* reframing stimuli on behavioural design quality. On both the first and second levels, we assess the distribution of *non-behavioural*, *behavioural*, and *mixed* aspects across the three data groups: *perspectives*, *mindmap*, and *concept*. We present the findings in percentages to account for the uneven number of participants in each condition. Lastly, we provide nuance to our main findings by comparing them to the participants' self-assessment scores.

6.1. Main findings

We present our combined structure-content stimulus results to answer our main hypothesis, 'H1: A *combined structure-content* reframing stimulus will increase behavioural design quality' (Section 3). Here, the findings show that the participants receiving the combined *structure-content* reframing stimulus emphasise behavioural (Be) aspects over non-behavioural (NoBe) aspects across all three data groups (Table 6). In contrast, in the control condition non-behavioural (NoBe) aspects dominate across all three data groups. These findings are supported by the distribution of link types in the mindmap data group (Figure 8), where the proportion of NoBe-NoBe links decreases and the proportion of Be-Be increases in the combined structure-content stimulus condition. At the same time, the proportion of any mixed links–here referring to the pool of NoBe-Be, NoBe-Mix, Mix-Mix, and Mix-Be–increases in the combined structure-content stimulus condition. As such, the findings show that applying the combined structure-content stimulus increased behavioural design quality, measured by a shift in perspective from emphasising *non-behavioural* aspects not reflecting behavioural impact to emphasising *behavioural* aspects reflecting behavioural impact. This is also shown in higher integration of *non-behavioural* and *behavioural* aspects measured by an increase of any mixed links in the mindmap data group (Figure 8).

To determine the significance of our findings, we used a Chi Square Test on the perspectives and mindmap data groups and a Fischer Exact test on the concept data group.

Table 6. Distribution of *non-behavioural*, *behavioural*, and *mixed* perspectives and concepts in the combined process-content stimulus and control condition.

	Perspectives			Mindmap (arranged perspectives)			Concept		
	NoBe = Non-behavioural, Be = Behavioural, Mix = Mixed								
Conditions	NoBe	Be	Mix	NoBe	Be	Mix	NoBe	Be	Mix
Control	**55%**	40%	5%	**57%**	27%	16%	**59%**	29%	12%
Structure-content	31%	**50%**	20%	32%	**57%**	11%	24%	**59%**	18%

The Fischer Exact test is better suited to the low frequencies found in this final data group. In comparing the combined structure-content stimulus to the control, the *p*-values are calculated to X^2 (2, $N = 422$) $= 18.29$, $p < .001$ on the perspectives data group, X^2 (2, $N = 796$) $= 36.89$, $p < .001$ on the mindmap data group, and $p = .129$ (Fischer Exact Test) on the concept group. As such, the findings are statistically significant in the perspectives and mindmap data groups while non-significant in the concept data group. With the low frequencies in the concept data group, effect size calculations are only suitable for the perspectives and mindmap data groups. We use Cramer's *V* to calculate effect size (*V*) for the Chi Square Tests with 2 degrees of freedom, with $V = .07$, $V = .21$, and $V = .35$ indicating small, medium, and large effect sizes. In comparing the combined structure-content stimulus to the control, the effect size is calculated to $V = .147$ on the perspectives data group, and $V = .152$ on the mindmap data group. As such, the effect sizes are in between small and medium. Together, these findings provide robust confirmation for H1.

6.2. Supporting findings

In answering our sub-hypotheses, 'H2a: A *structure* reframing stimulus will increase behavioural design quality', and 'H2b: A *content* reframing stimulus will increase behavioural design quality' (Section 3), we add the results from our individual *structure* and *content* stimuli.

For the *structure*-only stimulus, participants emphasised *non-behavioural* aspects in the perspectives and mindmap data groups. However, this emphasis shifts to *behavioural* aspects in the concept data group (Table 7). Also, the proportion of emphasising mixed aspects approximately doubles in the concept data group compared to the perspectives and mindmap data groups. For the *content*-only stimulus, participants emphasised *behavioural* aspects across all three groups, with a considerable increase to 93% in the concept group. Again, these findings are supported by the distribution of link types in the mindmap data group (Figure 8), where the proportion of NoBe-NoBe links decreases across all stimuli conditions. The proportion of especially any mixed links–here referring to the pool of NoBe-Be, NoBe-Mix, Mix-Mix, and Mix-Be–increases in the structure-only and content-only stimulus condition, whereas both any mixed links and Be-Be links increase in the combined structure stimulus. As such, the findings show behavioural design quality, measured by a shift in perspective from emphasising *non-behavioural* aspects to emphasising *behavioural* aspects, increased across all three data groups in the *content*-only stimulus condition. This is also reflected by the increase of integration of *non-behavioural* and *behavioural* aspects measured by an increase in mixed links in the mindmap data group (Figure 8). In the *structure*-only stimulus condition, while the emphasis from *non-behavioural* to *behavioural* aspects shifted in the concept data group, the proportion of *non-behavioural* aspects across the perspectives and mindset data groups decreased. Also, there was only a small increase in integration of *non-behavioural* and *behavioural* aspects measured by an increase of any mixed links in the mindmap data group (Figure 8).

We again used a Chi Square Test on the perspectives and mindmap data groups and a Fischer Exact test on the concept data group. Here, we calculated *p*-values by comparing all the conditions as in Table 8. The *p*-values range from $p > .001$ to $p = .411$, comprising a mix of significant ($p > .05$) and non-significant findings. Notably, the mindmap data group comprises the most significant results. Again, on the perspectives and mindmap data

Table 7. Distribution of *non-behavioural*, *behavioural*, and *mixed* perspectives and concepts across conditions.

	Perspectives			Mindmap (arranged perspectives)			Concept		
	NoBe = Non-behavioural, Be = Behavioural, Mix = Mixed								
Condition	NoBe	Be	Mix	NoBe	Be	Mix	NoBe	Be	Mix
Control	**55%**	40%	5%	**57%**	27%	16%	**59%**	29%	12%
Structure	**47%**	42%	11%	**52%**	39%	9%	36%	**43%**	21%
Content	35%	**48%**	17%	38%	**50%**	12%	7%	**93%**	0%
Structure-content	31%	**50%**	20%	32%	**57%**	11%	24%	**59%**	18%

groups we use Cramer's V to calculate effect size (V), but here comparing all the conditions as in Table 8. The effect sizes range from $V = .60$ to $V = .122$, comprising a mix of just below small ($V = 0.7$) to between small and medium ($V = .21$) effect sizes. Overall, these lend general confirmatory support to H2a and H2b.

6.3. *Self-assessment scores*

In providing nuance to our main findings, we elaborate on the self-assessment scores. Compared to our direct performance measures (perspectives, mindmap, and concept data groups), Likert scales reflect indirect measures relying on people's perceptions of performance. As such, while the findings provide nuance, the scores do not directly contribute to confirming or rejecting our hypothesis.

The findings were twofold in examining the participants' self-assessment scores on novelty, meaningfulness, and usefulness. First, the self-assessment scores were highest across all three categories in the *content*-only condition. Second, while the findings from our direct performance measures show substantial differences between the three stimuli conditions and the control, the differences from highest to lowest average self-assessments scores only range from 0.3 in the novelty category to 0.8 in meaningfulness and useful categories. As such, it is unsurprising that the p-value calculated using the Mann–Whitney U Test was not significant in any of the self-assessment categories ($p > .05$, see Appendix A4). Thus, even though the participants across the reframing stimulus conditions outperform the participants in the control group when measuring direct performance, these results do not translate into the self-assessment scores with low differences between conditions. These results align with other studies comparing direct and indirect measures of performance (Lauritsen, Cash, and Kreye 2023). See self-assessment score means and standard deviations in Table 9.

7. Discussion and implications

This paper set out to evaluate the impact of reframing on behavioural design quality via an exploratory controlled experiment. The findings confirm our main hypothesis 'H1: A *combined structure-content* reframing stimulus will increase behavioural design quality' (Section 3)'. Here, our findings show that in contrast to the participants in the control condition emphasising *non-behavioural* aspects, i.e. not reflecting behavioural impact, across the three data groups, the participants in the structure-content stimulus condition emphasised *behavioural* aspects. Hence, providing a stimulus combining sequential steps of actions

Table 8. *p*-values and effect sizes of distribution of non-behavioural (NoBe), behavioural (Be), and mixed (Mix) perspectives, arranged perspectives, and concepts across conditions.

	Perspectives (Chi Square Test) X^2 (2, N = 422) (Cramer's V)			Mindmap (Chi square test) X^2 (2, N = 796) (Cramer's V)			Concept (Fischer Exact Test) (N = 63)		
NoBe, Be, and Mix *p*-values	Control	Structure	Content	Control	Structure	Content	Control	Structure	Content
Structure	$X^2 = 3.06$, $p = .220$ $V = .060$			$X^2 = 9.28$, $p = .010$ $V = .076$			$p = .441$		
Content	$X^2 = 12.65$, $p = .002$ $V = .122$	$X^2 = 3.72$, $p = .162$ $V = .066$		$X^2 = 22.26$, $p > .001$ $V = .118$	$X^2 = 7.16$, $p = .029$ $V = .067$		$p = .001$	$p = .015$	
Structure-content	$X^2 = 18.29$, $p > .001$ $V = .147$	$X^2 = 7.16$, $p = .029$ $V = .092$	$X^2 = 0.56$, $p = .775$ $V = .026$	$X^2 = 36.89$, $p > .001$ $V = .152$	$X^2 = 15.76$, $p > .001$ $V = .100$	$X^2 = 2.09$, $p = .351$ $V = .036$	$p = .129$	$p = .637$	$p = .070$

Table 9. Self-assessment scores on Novelty, Meaningfulness, and Usefulness across the three reframing conditions and the control group including means and standard deviations.

	Novelty		Meaningfulness		Usefulness	
Self-assessment	Mean	Standard deviation	Mean	Standard deviation	Mean	Standard deviation
Control	3.8	1.7	4.0	1.3	4.7	1.2
Structure	4.1	1.4	4.3	1.4	5.0	0.9
Content	**4.2**	**1.5**	**4.8**	**1.1**	**5.5**	**1.1**
Structure-content	3.9	1.7	4.1	1.3	4.6	1.2

with pairs of abductive reasoning prompting questions connected to each step increased behavioural design quality. This provides important experimental support for prior claims of reframing's ability to expand and explore problem-solution spaces (Dorst and Cross 2001; Haase and Laursen 2019; Valkenburg and Dorst 1998; Van Boeijen, Daalhuizen, and Zijlstra 2020). These for example include that reframing provides a relevant approach to explore and mitigate implicit, inaccurate, or unfruitful assumptions across the problem-solution space (Dorst 2015; Maher, Poon, and Boulanger 1996), for example, ambiguities in preventing infections in nursing homes (Duarte and Daalhuizen 2024), or decreasing school bullying (O'Brien, Campbell, and Whiteford 2024). In our experiment, we demonstrate that reframing increases behavioural design quality in responding to a design brief with a significant behavioural component. Particularly, the participants using any of the three reframing stimuli identified problem and solution perspectives and developed concepts relevant to and reflective of the significant behavioural components in the brief due to increased emphasis on behavioural aspects and increased integration of behavioural and non-behavioural aspects. Thus, our findings confirm that reframing is a fruitful approach to expanding and exploring the complex problem-solution space associated with behavioural design.

In addition, we showed reasonable support for our sub-hypotheses, 'H2a: A *structure* reframing stimulus will increase behavioural design quality' and 'H2b: A *content* reframing stimulus will increase behavioural design quality' (Section 3). Importantly, while structure and content are essential elements of reframing (Daalhuizen and Cash 2021; Dorst 2011; Stompff, Smulders, and Henze 2016), little is known about their individual contribution to reframing. In unpacking the results, we find that the *structure*-only stimulus (only providing sequential steps of actions) decreases the proportion of outcomes emphasising *non-behavioural* aspects and only shifts the emphasis from *non-behavioural* to *behavioural* aspects in the concept data group. On the other hand, the *content*-only stimulus (only providing abductive reasoning prompting questions) shifts the emphasis from *non-behavioural* to *behavioural* aspects across the perspectives, mindmap, and concept data groups, with a dominant emphasis on *behavioural* concepts of 93% in the concept data group. At the same time, the combined *structure-content* stimulus shifts emphasis to *behavioural* aspects while also including mixed aspects. As such, our findings indicate that while the participants bring both a technical engineering and design mindset to the task, they generally gravitate towards emphasising non-behavioural aspects in the control condition (without use of reframing stimuli). While the participants using any of the three reframing stimuli generally gravitate towards emphasising behavioural aspects. Our results show that the reframing stimuli including *content*, i.e. the *content*-only stimulus and the combined *structure-content* stimulus, are most effective in shifting perspectives from emphasising non-behavioural to emphasising behavioural aspects across the three data groups. Here, the content-only stimulus even shifts to a dominant emphasis on *behavioural* aspects in the concept data group. Together, these results indicate that *content* is essential to explicit reframing, i.e. shifts in perspectives. As such, our results confirm that *structure* and *content* are both essential elements and effective in increasing reframing effectiveness, yet affect behavioural design quality in slightly different ways. More generally, while there is an ongoing discussion on the importance of both mindset and process (referred to as content and structure in this paper) in relation to design performance, and method quality specifically (Andreasen, Hansen, and Cash 2015; Daalhuizen and Cash 2021; Roozenburg and Eekels 1995), robust theory-building is still lacking. Here, our results offer initial insight into how these types of aspects operate differently and have complementary value in guiding action and enhancing performance or learning. Therefore, in addition to confirming that reframing is an effective and valuable way of exploring the problem-solution space, our findings also highlight the possibility of applying different adaptations of reframing *structure* and/or *content* to methods depending on the situated goal. For example, the content-only stimulus resulting in 93% *behavioural* concepts, could be used as a rapid approach throughout the behavioural design process to identify and develop an understanding of behavioural components across the problem-solution space in solving complex and open-ended problems (Nielsen et al. 2024). As such, our results–including our operationalisation of reframing stimuli–provide a foundation for building mature reframing methods (Daalhuizen and Cash 2021) and for additional reframing research.

8. Limitations and future work

There are three main limitations to consider when evaluating the contribution of this work. First, while our findings answer our main hypothesis, due to the small sample size and corresponding effect sizes of our exploratory study, follow on confirmatory experiments are needed to evaluate the individual impact of reframing structure and content elements and develop the scope of generalisation. Further work could, for example, include replicating our study with a larger sample size, which can now be determined through power analysis based on our results.

Second, due to the limitations of current reframing literature and our emphasis on internal validity, we chose to run the experiment with individual participants and formulate the design task as a linear sequence. While this setup is highly suitable for securing stable conditions fundamental to hypothesis testing (Hacking 1992; Neuman 2007), it arguably reduces ecological validity, where problem-solving is typically approached abductively in a mix of individual and team work (Cash et al. 2022c). As such, to increase ecological validity, future studies in theory testing mode could, for example, include: (i) a replication of this experiment comprising the same experiment materials but with teams instead of individuals, (ii) an adaption to allow for more representative iterative work, and (iii) a comparison between students and professional practitioners. Here, team dynamics (Baligar et al. 2022), different levels of design mindset (Lavrsen, Carbon, and Daalhuizen 2025) and level of expertise (Bunt et al. 2025) should be considered.

Third, while this study focused on coupling behavioural design and reframing, the lack of existing research on this specific relationship led to a more general coupling by providing the participants with an open-ended design brief with a significant behavioural component in the problem-solution space. In emphasising the robustness of findings and stimuli simplicity, we derived our reframing stimuli as directly as possible from existing theory. As such, the stimuli in this experiment represent general, domain-independent reframing structure and content elements adapted from frame creation and abductive reasoning (Dorst 2011; Dorst and Cross 2001; Koskela, Paavola, and Kroll 2018; Stompff, Smulders, and Henze 2016). Thus, while our findings both serve as a foundation for more general reframing studies across domains, further work is needed to examine and evaluate a direct coupling of reframing and behavioural design aimed at maturing a behavioural design specific approach.

9. Conclusion

This paper set out to evaluate reframing in behavioural design. To this end, we conducted an exploratory controlled experiment. In the experiment, we provided engineering design students with a complex, open-ended problem brief on the consequences of high meat intake with a significant behavioural component. The participants were then asked to use a model as aid when conducting a sequence of design tasks generating three types of design output: perspectives (problem and solution frames), mindmaps (arranged perspectives), and a concept. The provided model either conveyed one of three separate reframing stimuli or a control. Derived from frame creation and abductive reasoning, the reframing stimuli comprised either only *structure* (sequential steps of action), only *content* (unordered abductive reasoning prompting questions), or combined *structure-content*.

Our findings show that all three reframing stimuli significantly increased behavioural design quality compared to our control condition. Specifically, the participants using any of the three reframing stimuli increased emphasis on behavioural aspects and enhanced integration of behavioural and technical aspects of problem and solution understanding. As such, they identified problem and solution perspectives and developed concepts relevant to and reflective of the significant behavioural components in the brief. Here, our findings extend current research by providing important experimental support for prior claims of reframing as an appropriate approach to tackling complex, open-ended problems. In addition, as an initial step towards integrating explicit reframing in behavioural design, our study demonstrates reframing as a valuable explicit and systematic approach to develop problem-solution understanding in behavioural design by expanding the problem-solution space with increased emphasis on behavioural impact. Lastly, we point to the need for further research on reframing, including building and evaluating specific behavioural design reframing methods and understanding the role played by different elements of method content in shaping design outputs.

Disclosure statement

No potential conflict of interest was reported by the author(s).

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