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Problem Solving: What are the Important Questions?

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Abstract

Problem solving research is in need for re-thinking main questions. The purpose of this paper is a stock-taking of some of the identified problems, to discuss potential remedies for them, and to look for future perspectives. I see three areas for discussion: (1) What are the phenomena to be explained? (2) What methods should be used? What methodology is appropriate to the subject? (3) What is the progress in theory since the legendary work from Newell and Simon (1972)? What can we expect from new data sources? How can we relate data to theoretical assumptions?

Keywords: problem solving; methodology; research questions.

“How does the solution arise from the problem situation? In what ways is the solution of a problem attained?” (Duncker, 1945, p.1)

Introduction

In 1982, Ulric Neisser published a famous paper entitled "Memory: What are the important questions?" that requested a change in research topics for memory research. Instead of research on laboratory-induced phenomena, he wrote, researchers should look for more real-life orientation of their research questions and thereby increased the relevance of psychological memory research by addressing questions like eyewitness testimony and other phenomena of everyday memories.

In my view that is based on 30 years active research in this subfield, problem solving research is in need for a similar shift. After the seminal paper from Dietrich Dörner (1980) that proposed to move from simple to complex problems, a lot of research has been initiated in that area (for an overview, see the two editions: Frensch & Funke, 1995; Sternberg & Frensch, 1991) and delivered new insights into phenomena like intellectual emergency reaction (Dörner, 1997) or into the connection between emotion and complex problems (Spering, Wagener, & Funke, 2005). At the same time, a decline of traditional problem solving research in the style of Newell and Simon (1972) has occurred, if one follows the description given by Ohlsson (2012).

The request for a change in research paradigms is not a new one – in the nearby area of decision making, claims about a shift of the research focus to more natural situations are clearly articulated. Gary Klein and his “Naturalistic Decision Making” group (see for a review: Klein, 2008) postulated such a shift many years ago; also, Gigerenzer and his ABC group (see for a review: Gigerenzer & Gaissmaier,

2011) promoted a shift in the field of decision making to incorporate “ecological rationality”. Similarly, Huber (2012) pointed to the fact that decision behavior in realistic risky scenarios is quite different from that in gambles; it requires not probability estimation but active risk-management.

The purpose of this paper is a stock-taking of some of the problems identified, to discuss potential remedies for them and to look for future perspectives. To discuss the current state in the field of problem solving, it needs more space than six pages in a conference paper – but at least it is a starting point for discussion.

I see three major areas for discussion: (1) What are the phenomena to be explained? (2) What methods, what methodology is appropriate to the subject? Is the neglect of introspection really adequate to our state of the art? (3) What is the progress in theory since the legendary work from Newell and Simon (1972)? What can we expect from new data sources like detailed computer log-files with interaction protocols? How can we relate data to theoretical assumptions?

About Phenomena: What are the Interesting Issues?

If one reads papers on problem solving, the world as it is reflected in the articles looks as if there were no changes for the last 50 years. Tasks like the *Nine-Dot Problem* (first mentioned in Sam Loyd's *Cyclopedia of Puzzles* from 1914) that require insight problem solving are still famous (e.g., MacGregor, Ormerod, & Chronicle, 2001). The same is true for the *Tower of Hanoi* (e.g., Anderson, Albert, & Fincham, 2005), a kind of “drosophila” for problem solving research.

But the phenomena outside the lab (i.e., the problems we have to face in reality) have changed dramatically. Intransparency, dynamics, and polytely, for example, are omnipresent in daily lifes’ problem spaces. I will explain these three features shortly.

Attributes of real-world problems Real problems are not presented on a silver platter and do not show all necessary information to the problem solving person (*intransparency*). On the contrary: information selection has become a new problem – on the one side to identify missing information, on the other side to find valuable information in a flood of potentially misleading or false data.

It is not only information collection and selection that has changed drastically. Typically, real life problems are not static situations like a position on a chessboard – on the contrary: they change over time while we are still in the

process of searching for solutions (*dynamics*). High-stake problems (e.g., catastrophic events) often change over time very quickly and require adaptive strategies and flexible responses.

Last but not least: the goal function is not well defined or, even worse, contains seemingly contradictory goals (Blech & Funke, 2010). Especially in political conflicts, the polytelic structure of the situation makes it sometimes impossible to find acceptable compromises (*polytely*).

Definition of problems What is a “problem”? In simple situations, problems are “well-defined”. There is a set of givens, some operators to change the givens, and a well-defined state that has to be reached with minimal effort. In complex situations, problem definitions vary with the different perspectives from the different stakeholders. It varies because the different stakeholders follow different goals. Consequently, in “wicked problems” (Rittel & Webber, 1973) there are no clearly defined goal states; the problem is “ill-defined” in terms of an optimal goal state.

Problem solving research with wicked problems is also research about a persons’ preferences and her/his goal structures – without goals, there are no problems. Changing the aspiration level leads to a change in the difficulty of the problem under question – a change that is also possible if one alters the level of resolution in the description of the problem space (Selten et al., 2012).

Interactions with emotion and motivation Problem solving is seen primarily as a cognitive activity – but is that a true description of the phenomenon? If someone does not reach a goal immediately the situation can become frustrating. Problems are defined by producing negative emotions. So, why are we not researching the regulation processes in the case of complex problems?

The same is true for motivation: When do we loose interest in a problem and give up (or change our aspiration level; see the section above)? Why show some problem solvers more “grit” than others? The self-regulatory activities are not only related to cognition, but also to emotion and motivation. Our theories about problem solving are mostly theories about cognitive processes that ignore interactions with other psychic functions.

Consequences So, from my point of view problem solving research has to address at least some of these features in more detail and follow the recommendations given by Klein (2008) about the need for “naturalistic decision making”. How people deal with uncertainty is a still unexplained phenomenon (Dörner, 1980; Mackinnon & Wearing, 1980; Osman, 2010). Also, there remains as an open question the one of processing complexity: Does complex problem solving require *complex* cognition, or is it simply more of the same simple basic cognitive processes (see Funke, 2010)?

Rethinking Methods and Methodology: What is the Best Way for Data Collection?

The questions of method and methodology depend on epistemological assumptions that are prevalent in certain scientific communities. They change over time depending on the current paradigms. I will not discuss fundamental issues but concentrate on the value of introspection and on sampling issues (sampling of problems as well as of solvers).

Introspection, Single Cases, Simulations Whereas in the beginning of modern psychology of thinking, introspection was still an acceptable technique, behavioristic traditions have eliminated this source of evidence nearly completely. In a recent paper, Jäkel and Schreiber (2013) encouraged cognitive scientists to revisit this methodological approach. In their understanding, introspection (“...the ill-understood and problematic metacognitive processes that are central to introspective methods and that distinguish them cognitively from think-aloud methods”, Jäkel & Schreiber 2013, p. 22) offers a chance for a better understanding of cognitive and meta-cognitive processes.

What we need are in-depth descriptions and analyses of how people deal with complex dynamic situations in daily life, business, politics, science, and technology. Successes as well as failures may provide a rich source of data for researchers (Dörner, 1997). Kluwe (1995) put emphasis on the use of single cases of complex problem solving but up to now not much research has been done in that direction (but see, e.g., Dörner & Güss, 2011). As has been demonstrated impressingly in the case of catastrophes (e.g., Zapf & Reason, 1994), such singular events contain a lot of information that can be used as proof of concepts and theories. Cognitive modeling often starts with the reproduction of single case activities. A database with such cases would allow for testing and comparing different models with broadly accepted and deeply documented reference cases.

Last but not least: the use of computer simulations (Brehmer & Dörner, 1993; Gray, 2002) brings complexity into the lab and allows for manipulation of time (real-time, slow-motion, fast motion) and space (reality, fiction). But can we really trust results from simulated scenarios? Isn’t it necessary to go into the field, search for high-stake situations and see how stakeholders act there? We need more research on this issue before a solid answer can be given.

Stimulus sampling of problems For assessment purposes, a large item universe is needed. That is one of the advantages of formal systems (Funke, 2001) like structural equation systems or finite state automata. The disadvantage of using these formalisms is the restricted range of problems that all follow the same model. Subjects are confronted with changing semantics but the deep structure of the problems does *not* change: one has to deal with linear combinations or with state transitions. After a while, the problem situations

become routine and the assessment no longer addresses problem solving behavior but routine actions. How long are subjects in those assessment situations problem solvers and when do they learn from experience?

It is funny that problems that nowadays come under the term “complex problem solving”, are less complex (in terms of the previously described attributes of complex situations) than at the beginning of this new research tradition. The emphasis on psychometric qualities has led to a loss of variety. Systems thinking requires more than analyzing models with two or three linear equations – nonlinearity, cyclicity, rebound effects, etc. are inherent features of complex problems and should show up at least in *some* of the problems used for research and assessment purposes. Minimal complex systems run the danger of becoming minimal valid systems.

To increase the validity of assessments, we do not need more of the same, but different types of task requirements. The universe of (complex) problems seems to be large enough to allow for proper and diverse task selection. If stimulus sampling is not done broadly, Fiedler (2011, p. 166) warns against the consequences of bad practices, namely, that “findings may reveal more about the stimuli chosen than the persons being tested”.

Person sampling of problem solvers Sternberg (1995) points in his comparison of European and North American research on complex problems to the fact that different types of problem solvers are on focus: Americans prefer experts, Europeans prefer novices. Also, NDM has a clear preference to experts in their natural setting (Klein, 2008).

From my point of view, we need samples of novices as well as those of experts – the novices showing domain-general strategies influenced by little previous knowledge, the experts showing domain-specific procedures with a lot of world and domain knowledge in the background. As has been demonstrated in the case of creativity research, if one orients research to psychometric standards one needs larger samples and has therefore to reduce selection criteria (see Gruber & Bödeker, 2005, p. 4). Consequently, real experts in solving complex problems from which one could learn about successful strategies fall out of scope. The identified and measured thought processes might not be representative for the domain.

Consequences So, from my point of view problem solving research has to give up methodological rigorism and to open the field once again for controlled introspection and single-case studies that describe the phenomena in a less restricted way. At the same time, more variety in terms of problem situations as well as in terms of problem solvers is needed to get a broader view of the phenomenon. Psychometric requirements should not dictate the selection of tasks; instead, these requirements should be fulfilled after the search for valid sampling methods has been successful made.

Relating Theories to Data: How is Progress to be Expected?

Since the seminal work from Newell and Simon (1972) that introduced the concepts of “problem space”, “heuristic search”, and “logic theorist” not much progress in theory has occurred. The “theory of spaces” has been differentiated by, e.g., Burns and Vollmeyer (2002), Klahr and Dunbar (1988), or Simon and Lea (1974). The use of simple heuristics has been demonstrated in many experimental situations (Gigerenzer & Gaissmaier, 2011). Concerning logical decision making, the assumptions of a “homo economicus” are highly questionable. Decision makers outside of laboratory gambles do not search for probabilities, but for “risk-defusing operators” (Huber, 2012). Is the Instance-Based Learning Theory (Gonzalez, Lerch, & Lebiere, 2003) an optimal starting point for theory formulation? We have to apply theories like this one to our data and see if they can explain or even predict some of the phenomena that we find in our data sets.

Might we expect progress in theory if we go into “Big Data”? The use of data mining techniques is nowadays an interesting approach (e.g., Zoanetti, 2010). From my point of view, theory comes first by delivering the constructs we are searching for. This leads to the question: how is problem solving related to other constructs in the field? I will concentrate my review on the relation of problem solving to intelligence, wisdom, creativity, and executive functions.

Intelligence For many years, a controversy existed between those who believed in complex problem solving processes as being a separate competency (e.g., Beckmann & Guthke, 1995) compared to those who did not believe in this specific competency but who reduced CPS performance to basic abilities like working memory capacity and intelligence combined with knowledge (e.g., Süß, Kersting, & Oberauer, 1993).

The relationship between CPS and intelligence was indeed for long time an unclear one (for a review, see Wenke, Frensch, & Funke, 2005) but recent research, in the context of large-scale assessment (Greiff et al., 2013; Wüstenberg, Greiff, & Funke, 2012) as well as in other contexts (Danner, Hagemann, Schankin, Hager, & Funke, 2011), supported the view of an identifiable and measurable CPS competency beyond IQ. The exact attributes of this surplus competency and the actual processes behind this competency remain open.

Wisdom The connection to wisdom is also unclear: it *should* be related to problem solving because finding acceptable solutions for conflicts is *one* important attribute of wisdom (Baltes & Smith, 2008). Wisdom is also needed for choosing appropriate strategies. For example, the famous Chinese *Thirty-Six Stratagems* can be seen as a collection of strategic wisdom that can be applied to complex problems. Managerial problem solving might profit from these ideas. Without research on these issues, we do not know.

As long as knowledge-poor problems dominate our research labs, we will not detect strong effects of accumulated and crystallized knowledge.

Creativity The more complex a problem becomes, the more creativity in finding appropriate solutions is necessary. Because problem solving is defined as non-routine behavior, it is essentially connected to creativity (Simonton, 2012). The introductory quote to this paper from Duncker (1945) points exactly to that connection. To be clear: creativity is *one* important aspect of problem solving, but does not cover other aspects of the concept that consist also of strategy development and use, knowledge acquisition and application, etc.

The assumed strong connection between creativity and problem solving has implications for assessment: the more complex a presented problem, the more freedom in generating solutions by a subject is required. If a subject has to choose options for actions from pull-down menus, assessment of creativity in the context of generating and finding solutions to problems is massively restricted.

Decision making, planning, executive functions In a recent review, Diamond (2012) differentiates three higher executive functions: reasoning, problem solving, and planning. The first two are said to be synonymous with fluid intelligence. The strong interest in self-regulation processes connects these research areas under a common label (see Zelazo, Carter, Reznick, & Frye, 1997).

Decision making should be seen as a specific cognitive activity within a larger activity called problem solving. In the course of problem solving, many decisions have to be made – but they have to be orchestrated and integrated into the course of action regulation. Metacognition is needed to monitor the whole process and to redirect attention depending on solution progress.

Consequences The connection between problem solving and related constructs can be tested by comparing models of their relationship with appropriate data. From my point of view, process models need other types of data than structural models: Because computer-based assessment technologies offer the chance to get rich log-files, developing and testing such models should be possible. Here I see also a chance for cognitive modeling to inform us about basic processes and how they contribute to the constructs on the next levels of aggregation.

Final Remarks

Problem solving is one of the key competencies in the 21st century. That explains the interest of PISA and other worldwide operating large-scale assessments of the competencies of next generations' workforce. The reason for this is simple: more and more business is done non-routinely and therefore requires problem solving activities (see, e.g., Autor, Murnane & Levy, 2003).

To be clear: problem solving requires more than moving towers or connecting dots under some restrictions. Problem solving is not only a cognitive process but includes also motivation and emotion regulation due to frustrations (Barth & Funke, 2010). Problem solving also requires more than solving linear equation systems: The concept of minimal complexity (Greiff & Funke, 2009) and the resulting assessment of dynamic decision making (see Greiff, Wüstenberg, & Funke, 2012; Wüstenberg, Greiff, & Funke, 2012) were useful steps for classroom assessment but it needs a more ambitious, informationally comprehensive environment to address the richness of problem solving in dynamic environments.

When summarizing the recent history of problem solving research under the auspices from what he correctly labeled the “Newell-Simon paradigm”, Ohlsson (2012, p. 117) wrote:

“In summary, Newell and Simon’s first concept of generality, codified in the General Problem Solver, failed as a psychological theory because it is not true: there is no single problem solving mechanism, no universal strategy that people apply across all domains and of which every task-specific strategy is a specific instance. Their second concept of generality initiated research on the cognitive architecture. The latter is a successful scientific concern with many accomplishments and a bright future. But it buys generality by focusing on a time band at which problem solving becomes invisible, like an elephant viewed from one inch away.”

I have no worry about the future of problem solving research when I look around me. In politics, on the business market, in our environment but also in our personal life complexities are ever increasing. So, the need for research is there – what seems to be missing is an attractive problem solving theory that stimulates further research, a proper set of diverse tasks for experimenting with them, and a methodology that opens again the window to inner processes.

Coming back to Ulric Neisser’s (1978) famous paper on memory research – did it change anything? Research is a slow tanker – but once an idea is in the world it starts to influence people. Following Neisser’s comments, memory research today is much more real-world oriented than 35 years ago. So, if my considerations could nudge problem solving researchers into a more real-life orientation, I would be happy.

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