Chapter 10
Aligning Problem Solving and Gameplay: A Model for Future Research and Design

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ABSTRACT

Problem solving is often discussed as one of the benefits of games and game-based learning (e.g., Gee, 2007a, Van Eck 2006a), yet little empirical research exists to support this assertion. It will be critical to establish and validate models of problem solving in games (Van Eck, 2007), but this will be difficult if not impossible without a better understanding of problem solving than currently exists in the field of serious games. While games can be used to teach a variety of content across multiple domains (Van Eck, 2006b, 2008), the ability of games to promote problem solving may be more important to the field of serious games because problem-solving skills cross all domains and are among the most difficult learning outcomes to achieve. This may be particularly important in science, technology, engineering, and math (STEM), which is why serious game researchers are building games to promote problem solving in science (e.g., Gaydos & Squire, this volume; Van Eck, Hung, Bowman, & Love, 2009). Current research and design theory in serious games are insufficient to explain the relationship between problem solving and games, nor do they support the design of educational games intended to promote problem solving. Problem solving and problem-based learning (PBL) have been studied intensely in both Europe and the United States for more than 75 years. Most recently, researchers (e.g., Jonassen, 1997, 2000, & 2002;Hung, 2006a; Jonassen & Hung, 2008) have made advances in both the delineation and definition of problem types and models for designing effective problems and PBL. Any models and research on the relation of games and problem solving must build on the existing research base in problem solving and PBL rather than unwittingly covering old ground in these areas. In this chapter, we present an overview of the dimensions upon which different problems vary, including domain knowledge and structuredness and their associated learning outcomes. We then propose a classification of gameplay (as opposed to game genre) that accounts for the cognitive skills encountered during gameplay, relying in part on pre-

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INTRODUCTION

It has been argued that games are a kind of disruptive technology (e.g., Strawn, 2007), but they can only be so to the extent that they solve a widely recognized problem that has value to sufficient numbers of people. For game-based learning to truly become a disruptive technology, it must address a critical need that is difficult to meet any other way. Many have argued that games address critical thinking and problem-solving skills (e.g., Gee, 2007a; Greenfield, in press; Van Eck, 2006a, 2007; Yanuzzi & Behrenhausen, this volume) that our current educational system is failing to provide (e.g., Broussard, La Lopa, & Ross-Davis, 2007; OECD, 2004).

Problem solving may well be the most powerful pedagogical benefit of commercial games in general and of game-based learning and serious games specifically. Whether our current educational system recognizes the need for problem solving as a learning outcome, and whether or not it can support it with existing resources and infrastructure, it seems clear that problem solving and the related research and design we do will remain an important area of study in the field of serious games.

Unfortunately, while researchers have begun to move the discussion of problem solving beyond descriptive to theoretical (e.g., Gee, 2007a & 2007b; Van Eck, 2007) and the practical (Van Eck, 2008), the majority of our discussion can be summed up as “Games are problems being solved by players; therefore, playing games will help people be better problem solvers.” Our research tends to be primarily descriptive, wherein we describe the admittedly complex behavior involved in working one’s way through a game like World of Warcraft (Blizzard, 2001) as evidence that problem solving must surely be going on during that process. This is sufficient for making the case that games most likely address problem solving and are therefore worthy of further study, but this is not sufficient to guide our development of serious games to directly address problem solving as a learning outcome. Problem solving is far more complex than many first realize, just as games are more complex than they appear at first to the general public. For example, we cannot discuss problem solving without understanding what type of problem we are referring to: creating a menu for guests who have different diet restrictions, troubleshooting a car that won’t start, diagnosing a patient’s back pain problem, or solving global warming. Each type of problem differs significantly in structuredness, requirements for prior knowledge, ability to embed other subproblems, cognitive structure, etc. Just as we recognize that game genres (e.g., first-person shooter, adventure, role-playing games [RPGs], massively multiplayer online games [MMOGs]) encourage different gameplay experiences, we need to recognize the

vious classifications systems (e.g., Apperley, 2006), Mark Wolf’s (2006) concept of grids of interactivity (which we call iGrids), and our own cognitive analysis of gameplay. We then use this classification system, the iGrids, and example games to describe eleven different types of problems, the ways in which they differ, and the gameplay types most likely to support them. We conclude with a description of the ability of problems and games themselves to address specific learning outcomes independent of problem solving, including domain-specific learning, higher-order thinking, psychomotor skills, and attitude change. Implications for future research are also described. We believe that this approach can guide the design of games intended to promote problem solving and points the way toward future research in problem solving and games.
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