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Player Rating Systems for Balancing Human Computation Games: Testing the Effect of Bipartiteness

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ABSTRACT

Human Computation Games (HCGs) aim to engage volunteers to solve information tasks, yet suffer from low sustained engagement themselves. One potential reason for this is limited difficulty balance, as tasks difficulty is unknown and they cannot be freely changed. In this paper, we introduce the use of player rating systems for *selecting and sequencing tasks* as an approach to difficulty balancing in HCGs and game genres facing similar challenges. We identify the bipartite structure of user-task graphs as a potential issue of our approach: users never directly match users, tasks never match tasks. We therefore test how well common rating systems predict outcomes in bipartite versus non-bipartite chess data sets and log data of the HCG *Paradox*. Results indicate that bipartiteness does not negatively impact prediction accuracy: common rating systems outperform baseline predictions in HCG data, supporting our approach's viability. We outline limitations of our approach and future work.

Keywords

Human Computation Games, difficulty balancing, matchmaking, Elo, TrueSkill, Glicko, bipartite graphs

INTRODUCTION

Human computation games (HCGs) are an increasingly popular way of engaging potentially hundreds of thousands of volunteers in solving vast amounts of information tasks that cannot yet be computationally solved, such as identifying protein foldings (*Foldit*), classifying galaxies (*GalaxyZoo*), transcribing historical newspapers (*DigitalKoot*), or mapping neurons (*Eyewire*) (Pe-Than et al. 2015; Law and Ahn 2011).

The main rationale for HCGs is to use the engaging qualities of games to attract, engage, and retain volunteers to perform otherwise boring tasks (Pe-Than et al. 2015). However, evidence suggests that HCGs face severe engagement issues themselves (Reeves and Sherwood 2010; Sauermann and Franzoni 2015): on Zooniverse.org, currently the largest HCG platform, only 27 percent of volunteers on average return to a given project for a second

time, and most contributions are very short in duration. The overwhelming majority of work (around 85 percent) is performed by a small (4-7 percent) minority of “superusers” that return multiple days and for long sessions (Sauermann and Franzoni 2015). Hence, to fulfill the promise of HCGs, their designers have to find ways of better engaging users—to motivate the current majority of short-one-time visitors to either stay longer or, ideally, to turn into repeat-long-visitors.

A core feature of engaging game design is a well-balanced difficulty curve, in which players are faced with challenges whose difficulty matches their growing skill (Csikszentmihalyi 1990; Sweetser and Wyeth 2005; Koster 2004). Unlike entertainment games, however, HCGs cannot create or modify challenges *ad libitum* to optimize engagement; they have to work with the given information tasks of the scientific project. Additionally, the difficulty of these tasks is typically unknown in advance—an issue HCGs share with e.g. entertainment games with user-generated levels. We suggest this issue can be solved by estimating task difficulties and player skill to then *select and sequence* tasks from the task pool in an order that matches the user’s skill curve. In theory, multiplayer rating systems like Elo, Glicko, or TrueSkill could be adapted to estimate task difficulty and user skill in HCGs by treating both tasks and users as players. A major unresolved issue of this approach is the bipartite structure of the user-task graph in HCGs: users only ever “play against” tasks (and vice versa), which could easily result in insufficient flow of rating information through the graph, particularly when the graph is very sparse or unbalanced.

To assess the viability of our approach, we empirically tested the prediction accuracy of three common rating systems—Elo, Glicko, and TrueSkill—with two data sets. We tested bipartite versus non-bipartite and balanced versus unbalanced sub-samples of professional chess match data to see the effect of bipartiteness and balance with “standard” multiplayer data. Results indicate that bipartiteness has no effect on prediction performance, and that unbalancedness in some circumstances increases prediction quality, which appears to be connected to the existence of vertices with a high degree centrality. We also tested the systems with player data from the HCG *Paradox*, finding that all three outperform a baseline prediction. We discuss possible explanations for the observed data, and conclude with outlining limitations, future work, and applications of our approach beyond HCGs.

BACKGROUND

Challenge, Engagement, and Difficulty Balancing

One fundamental mechanism of game engagement is challenge: the pursuit and mastery of non-trivial challenges stokes arousal, binds attention, and provides intrinsically motivating experiences of competence (Klimmt and Hartmann 2006; Deterding 2015). Largely grounded in Csikszentmihaly’s flow theory (1990), game designers and researchers concur that game challenges are particularly engaging because or when they are well-balanced over time: the difficulty of presented challenges grows with the skill of players, such that players are faced with neither boringly easy nor frustratingly hard ones. Optimum engagement or “flow” is found at a “happy” medium where difficulty matches player skill, requiring their total attention and effort (Csikszentmihalyi 1990; Sweetser and Wyeth 2005; Koster 2004).

A growing number of empirical studies demonstrate clear correlations between challenge and behavioral and experiential measures of player engagement (Klimmt et al. 2007; En-

geser and Rheinberg 2008; Lomas et al. 2013; Fullagar et al. 2013; Corem et al. 2013; Cechanowicz et al. 2014; Alexander et al. 2013; Denisova and Cairns 2015). However, studies also call into question the hypothesis that *optimal* engagement is found where player skill and task difficulty match – mathematically, where the odds of success or failure are 50/50. On average, players experience more enjoyment and stay longer the likelier they are to succeed (Rigby and Ryan 2007; Lomas et al. 2013). One large-scale experiment found that behavioral engagement (measured as number of trials times time on task) is highest at a success rate of about 90 percent (Lomas et al. 2013), suggesting that players seek out difficulty that is “just enough” to provide *some* sense of challenge (and hence competence) rather than difficulty that is “just barely” surmountable.

No matter what the exact point of optimum challenge, theory and evidence support that balancing difficulty is important for making games engaging. And indeed, difficulty balancing is a core practice of game design. Broadly speaking, one can differentiate three approaches: (1) *manual balancing* through playtesting and revision of game levels and tasks pre-release; (2) providing game levels and tasks in multiple predetermined *difficulty settings* that players choose from; (3) *dynamic difficulty adjustment*, where the game adjusts game parameters and content during gameplay to steer the game state toward a desired target (e.g. modulate enemy spawn rate to maintain a target player death probability) (Schell 2014; Michael and Chang 2013; Baldwin et al. 2013).

Constraints of difficulty balancing in HCGs and similar genres

If difficulty balancing is a proven approach to design well-balanced challenges, and well-balanced challenges are a key to game engagement, difficulty balancing should be a promising strategy for improving engagement in HCGs as well. However, HCGs present several design constraints typically not found in entertainment games (see also Logas et al. 2014):

1. The information tasks that constitute the “levels” of HCGs are predetermined by the scientific data set or problem. They cannot be designed or discarded at will to improve engagement.
2. The difficulty of these tasks is usually unknown.
3. Tasks cannot be straightforwardly manipulated to optimize their difficulty, as this could quickly compromise the scientific validity of generated solutions.
4. Investing large amounts of human labor into identifying and manipulating the difficulty of individual tasks defeats the purpose of HCGs to cost-efficiently solve computationally unsolvable tasks through crowdsourcing.

Hence, the above-mentioned common approaches to difficulty balancing do not easily apply to HCGs: manual balancing and difficulty settings are too human labor-intensive; and dynamic difficulty adjustment involves manipulating the actual tasks. Perhaps as a consequence of that, HCGs today by and large feature no difficulty adjustment. In fact, most of these constraints hold for user-generated-content-driven games such as the *LittleBigPlanet* series (Media Molecule 2008) or the recent *Super Mario Maker* (Nintendo 2015) as well: levels are predetermined, of unknown difficulty, and altering them would impinge on the creative authority of the users creating them.

Solution: task sequencing through multiplayer matchmaking

While tasks themselves cannot be easily manipulated, there is one aspect of HCGs that can be balanced: the selection and sequencing of which tasks get presented to players in which order. Fundamentally, this form of difficulty balancing requires a mechanism that can (a) efficiently estimate task difficulty and user skill and (b) choose a task from a task pool that presents an optimally engaging challenge to the current user. Where tasks and task difficulty parameters exist in a well-formalized form and optimal solutions are known, approaches exist to potentially produce a well-balanced task sequence (see Butler et al. 2015 for one example). However, in such cases, human computation is by definition no longer needed as tasks can be computationally solved. And in most cases of HCG tasks, tasks are ill-formalized, difficulty parameters are ill-understood, and optimal solutions are unknown. Hence, if at all, systems have been using very rough heuristics like task size to arrange tasks in difficulty curves, resulting in suboptimal player experiences (Logas et al. 2014).

We suggest that multiplayer game matchmaking systems (Delalleau et al. 2012) provide an attractive template for the selection-and-sequencing balancing of HCGs. Matchmaking systems draw on ratings of player skill to identify players with roughly equal skill from a pool of currently active players and pair them into a balanced match. Match outcome data is then used to adjust the ratings of player skills. Most matchmaking systems today employ variants of one of three player rating systems: Elo, Glicko, and TrueSkill (Delalleau et al. 2012, see below). Matchmaking systems have been used outside of purely player-vs-player rating applications in some cases. Closely related to our work in this regard is the game *Jumpcraft* (Snyder and Izquierdo 2014), which used TrueSkill to estimate the ratings of players and user-generated levels in a platforming game. The game *Matchin* (Hacker and Ahn 2009) used Elo and TrueSkill to rate images to find the most preferred images in a set.

Formally, player matches in competitive multiplayer games and user task solutions in HCGs are highly analogous. Just as players in multiplayer tournaments and rankings engage in multiple matches, HCG users typically play multiple levels, and HCG levels are typically played multiple times, triangulating player solutions to control quality, generate aggregates, or collect multiple solutions for a problem with an unknown optimum (Pe-Than et al. 2015). These prior solution attempts are a readily available signal for both task difficulty and user skill, analogous to match outcomes in multiplayer games. If one equates both HCG users and tasks with players in multiplayer rating systems, one receives one unified skill *and* difficulty measure for both at once: the rating score. Conveniently, all rating systems allow the calculation of winning odds of each side of the match based on the delta between their rating scores. And as we saw, winning odds are one quantitative measure of the balancedness of a challenge that has been shown to correlate with sustained player engagement (Lomas et al. 2013). In short, by using the information of prior solution attempts to give users and tasks skill/difficulty ratings, we should be able to achieve a desired engagement-optimal degree of challenge by presenting users with tasks whose rating score delta to the player's rating score predicts the desired degree of success (e.g., a 90 percent chance of solving the task).

However, there is one significant difference between HCG user-task matching and multiplayer game player-player matching. In multiplayer game communities, any player can theoretically play against any other player. This is not the case in HCGs: here, members of the “user” team only ever play against members of the opposite “task” team and vice versa,

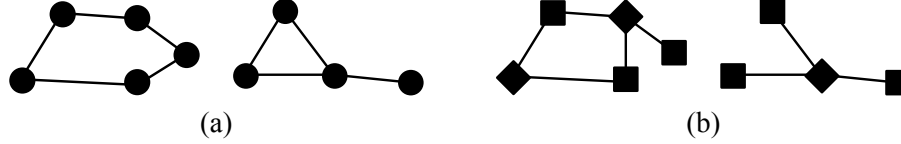


Figure 1: Examples of (a) non-bipartite and (b) bipartite graphs. Vertices in the bipartite graphs can be divided into two sets U and W (here indicated as diamonds and squares) such that no edge connects two vertices in the same set.

never against a member of their own team.

In graph theoretical terms, if one models users and tasks as vertices and solution attempts or matches as edges between the vertices, HCG sessions present a *bipartite graph* (Asratian et al. 1998): their vertices can be divided into two disjoint sets U and W such that every edge connects a vertex in U with one in W . Examples are shown in Figure 1.

This raises the question whether existing rating systems, developed for non-bipartite player-match graphs, are equally capable to predict winning probabilities for bipartite ones. In non-bipartite player graphs, information about player skill (in the shape of match outcomes) flows more or less freely between vertices: three players A, B, and C may all inform and be informed by each other’s ratings through direct matches. In a bipartite player graph with players A and B in one set U and player C in set W , A and B can never directly inform each other’s rating through matches, only indirectly through both consecutively matching C. In other terms, bipartite graphs limit the maximum *density* or proportion of actual to possible number of edges in the graph (Scott 2012). The lower the density of a graph, the higher the probability of *structural holes*: clusters of vertices and edges not linked to each other, resulting in incomplete information flow between vertices (Scott 2012). Another potential issue of bipartite graphs in particular is *balance* (Asratian et al. 1998). A bipartite graph is balanced if its two vertex sets are of equal size. Given equal density, the more unbalanced a graph is, the less likely it is that a vertex in the larger set has more than one edge, meaning it never receives or sends more than one piece of rating information. We can assume that many HCGs have very unbalanced graphs, with tasks outnumbering players.

In summary, applying multiplayer rating systems to HCG users and tasks could be a promising approach to difficulty balancing in HCGs. However, it is unclear whether the bipartite nature of the user-task graph negatively affects the information flow between vertices and thus, the quality and predictive accuracy of the ratings. To answer this question, we empirically tested the predictive accuracy of popular rating systems against two relevant data sets.

METHOD

Rating systems

We chose to test Elo, Glicko, and TrueSkill due to their wide use in multiplayer matchmaking systems. Elo was developed by Arpad Elo (1978) for rating chess players and is today widely used in chess and other competitive sports. Elo models player performance as a normally distributed random variable, with the mean being the player’s true skill, expressed

in a numerical Elo rating. If a player wins a match, their Elo rating increases, else it goes down—and more or less so depending on the skill of the other player. That is, the greater the delta between the Elo rating of both players, the more likely the stronger player will win, and the more a loss of the stronger player will increase the weaker player’s rating (and vice versa).

One problem with Elo is that it does not take into account how reliable player ratings are, that is, on how many and how recent matches they are based. In response, Glickman (1999) developed the likewise widely used Glicko and Glicko-2 systems, which include a “ratings deviation” expressing the uncertainty of the rating.

Finally, Herbrich et al. (2007) built on the ideas of Glicko to develop TrueSkill, a rating system that is notably used for player matchmaking on the Xbox Live online platform. Unlike Glicko, TrueSkill allows to compute player ratings in more than two-player and team matches.

Data Sets

For our analysis we used two data sets. The first stems from the Deloitte/FIDE Chess Rating Challenge, provided by Kaggle (2011). We chose this data set of professional chess matches for two reasons: first, multiplayer (chess) rankings are the original use case from which all three systems were developed. Hence, they should allow us to observe any effects of bipartiteness isolated from any potential unknown confounds of “non-standard” uses cases. Second, the chosen data set is large enough to allow us to extract similar bipartite and non-bipartite subgraphs for comparison. The full “primary training” data set covers a period of 132 months, and consists of 1,840,124 matches among 54,205 players. To simplify later processing, we removed all ties. To improve the number of matches per player in the data set, we removed players who only participated in a few matches, by retaining only matches between players who participated in 100 or more matches. This resulted in a data set of 262,648 matches among 5,321 players. In this data set, a match is between two players; an individual player may be either white or black in any given match. We will refer to this as the FIDE data set.

Our second data set stems from the HCG *Paradox*, developed by the University of Washington’s Center for Game Science (2015). The game is built around the concept of *crowd-sourced formal verification* of software (Dean et al. 2015; Dietl et al. 2012): players are solving constraint problems that can contribute to a proof that a piece of software is free from certain classes of errors. Each level in the game is a system of constraints for the player to solve, cast as a puzzle involving changing the size of variables to satisfy those constraints. The player receives a score based on how many constraints they have satisfied. If the player can find a setting for the variables that satisfies all the constraints, they have solved the constraint system and proved a property about the underlying code. Since it may not be possible to satisfy all the constraints, puzzles have a target score that indicates the number of constraints that need to be satisfied for the player to complete the level.

We processed gameplay logs of the version of *Paradox* posted on the `verigames.com` website from May to September 2015. If a player managed to achieve the goal score in a level, we considered this a win for the player; otherwise, it is a loss. We considered the

same set of constraints with a different target score a different level. Starting with non-tutorial levels, we found 5,671 matches among 348 players and 2,442 levels. We retained only matches between players who participated in 10 or more matches, resulting in 1,507 matches among 121 players and 70 levels. In this data set, a match is always between a player and a level. We will refer to this as the *Paradox* data set.

In both cases, we took the first 95 percent of the matches to use as training data, and the last 5 percent as test data. Due to the ordering effects of playing (we expect that both chess and *Paradox* players improve over time), we preferred to test on data at the end of the data set rather than use a full cross-validation.

Graph Representation

We can model all the matches, players, and levels in the data sets as a graph; in general, a graph $G = (V, E)$ consists of a set of vertices V and set of edges E . In our case, the vertices represent players/users or levels/tasks, and each edge represents a match between two players or a player/user and a level/task. A bipartite graph $B = (U, W, E)$ consists of set of edges E and two disjoint sets of vertices U and W ; each edge connects a vertex from U with one in W . There may be multiple edges between two vertices, representing two players engaging in multiple matches or a user attempting a task multiple times. We represent bipartite graphs using B and non-bipartite graphs using N .

As noted, one graph property of interest is the *average degree* of the graph, $\delta = \frac{2|E|}{|V|}$. The *degree* of a vertex is its number of incident edges: in our case, the number of matches it is involved in. The average degree of the graph is the average number of matches players or levels take part in. More matches should give us a better estimate of the player’s skill or level’s difficulty. Thus, we would like to compare graphs that have the same average degree.

Another property of interest, which only applies to bipartite graphs, is how *balanced* it is. A bipartite graph is said to be balanced if its two vertex sets are of equal sizes, that is, $|U| = |W|$ (Asratian et al. 1998). We generalize this to a notion of *how* balanced a bipartite graph is, by looking at what fraction of the vertices are in the smaller set: $\beta = \frac{\min(|U|, |W|)}{|U| + |W|}$. A balanced graph would have $\beta = 0.5$; the closer β is to zero, the more “unbalanced” a graph is. This is of interest as in an HCG, it is not necessarily the case that the number of players and levels will be the same, and therefore, we may have a very unbalanced graph.

Subgraph Construction

To examine matchmaking for prediction, we constructed many randomly-selected *training subgraphs* from the training portion of the data sets, and *test subgraphs* from the test portion. Subgraphs derived from training set are notated as G_{tr} ; subgraphs derived from the test set as G_{te} .

Using the FIDE data set, we constructed subgraphs with similar properties for comparison. We selected subsets of the vertices and edges from the FIDE data set to create bipartite and non-bipartite subgraphs using the approach described in Figure 2. This approach constructed pairs of training subgraphs, one bipartite (B_{tr}) and one non-bipartite (N_{tr}), that had the

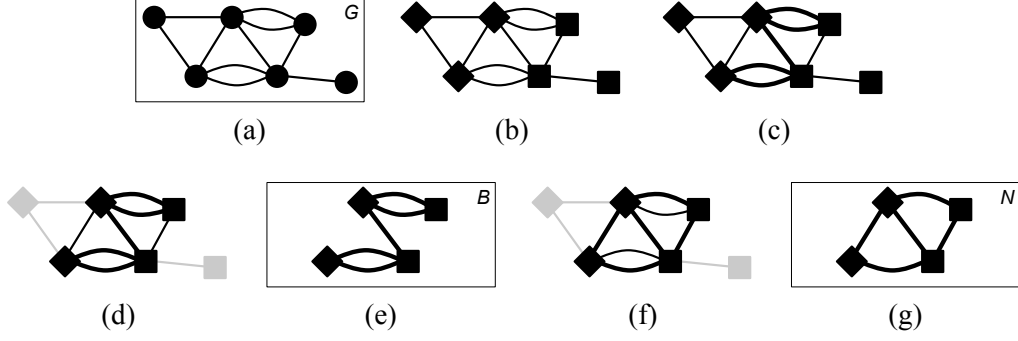


Figure 2: Subgraph Generation of desired edge count $|E|$. (a) Starting from an initial graph G , (b) vertices are randomly partitioned into sets U (diamonds) and W (squares). (c) Up to $|E|$ edges between U and W are randomly selected (bold) and (d) vertices that are not incident to any selected edges are discarded. (e) The selected edges and incident vertices are stored as a bipartite subgraph B . (f) A new set of edges is randomly selected (bold), starting with edges that are incident to vertices that do not have any incident edges; (g) The newly selected edges and incident vertices are stored as a non-bipartite subgraph N . Note that there is no guarantee that N is non-bipartite, or particularly different from B (for example, one edge change change make a graph non-bipartite). However, in practice, all the subgraphs N we constructed this way were bipartite. To construct the corresponding test subgraphs, bipartite and non-bipartite edges that connect vertices that were not removed from the training subgraph are randomly selected from the test subgraph, starting with edges that are incident to vertices that do not have any incident edges.

same number of vertices and edges, and thus the same average degree, along with their corresponding test subgraphs (B_{te} and N_{te}). By controlling the relative sizes of U and W , we controlled how balanced the constructed bipartite subgraph was. To examine graphs that are balanced differently, we constructed subgraphs at $\beta \approx 0.05$ and $\beta \approx 0.50$.

We used the *Paradox* data set to examine prediction on a data set from a real HCG. Since this graph was already bipartite (between players and levels), we simply selected a random subset of matches to use from the training set (P_{tr}) along with the matches between the corresponding vertices in the test data set (P_{te}).

ANALYSIS

Prediction Error

We trained the rating systems by playing back all the matches in the training subgraph using a specified system, and then predicted the outcome of the matches in test subgraphs. As an error metric for prediction, we used the fraction of matches in the test subgraph that were predicted incorrectly, similar to Herbrich et al. (2007). We use $err_M(X, Y)$ to represent rating system M , training on subgraph X , and predicting on Y . Training systems include *Elo*, *TS* (TrueSkill), and *Glicko-2* (Glicko-2). For baselines, we also use $err_{wh}(\bullet, Y)$ as always predicting white; this method does not require training subgraphs.

We used our own implementation of Elo that initialized ratings to 1200 and used $K = 24$. We used available Python Glicko-2 (Kirkman 2010) and TrueSkill (Lee 2015) implemen-

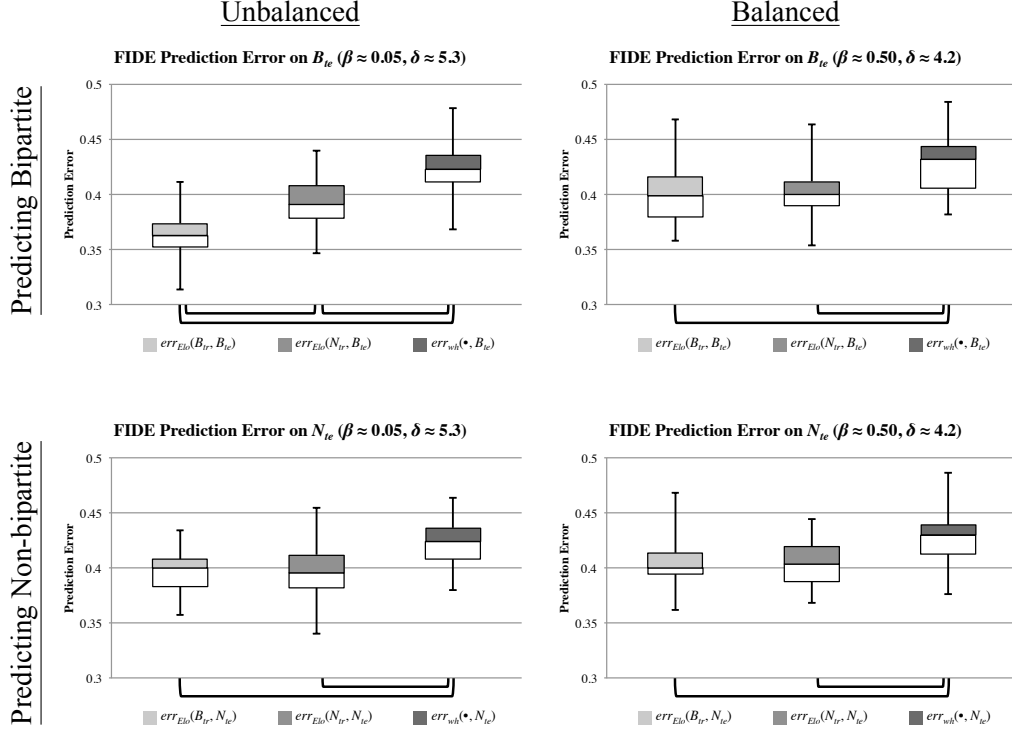


Figure 3: Prediction error on FIDE data set. Box plots show the minimum, first quartile, median, third quartile, and maximum prediction errors. Lines under the plot show statistically significant comparisons.

tations with their default initialization and update rules. We predicted the player with the higher rating would win a match.

To get a better idea of the general performance of each approach, for each graph, we constructed 50 subgraphs and compared the prediction error of different approaches. Since prediction error was not normally distributed, for each set of comparisons we applied the Kruskal-Wallis test. For subsequent 2-way comparisons we applied the Wilcoxon Rank Sum test; here we applied the Bonferroni correction by multiplying observed p -values by the number of pairwise comparisons within each set of comparisons. Lines underneath plots, if shown, give statistically significant comparisons ($p < .05$).

FIDE Prediction Error

Using the FIDE data set, we constructed 50 bipartite and 50 non-bipartite training subgraphs at both $\beta = 0.05$ and $\beta = 0.50$, all with 10,000 edges, and corresponding test subgraphs of 500 edges. We then made predictions on both the resulting bipartite and non-bipartite test subgraphs. As a baseline, we chose always predicting white, as white won games more frequently in the data set. Prediction results on the FIDE data set are shown in Figure 3 and Table 1.

In all cases, predictions made using Elo training outperformed simply predicting white, improving median prediction error up to 14 percent. In most cases, there was no difference

Predicting Bipartite

Unbalanced

$H(2) = 95.495$ $p < .001$	$err_{Elo}(B_{tr}, B_{te})$ M=0.36	$err_{Elo}(N_{tr}, B_{te})$ M=0.39	$err_{wh}(\bullet, B_{te})$ M=0.42
$err_{Elo}(B_{tr}, B_{te})$		$U = 363.5, p < .001$	$U = 31.0, p < .001$
$err_{Elo}(N_{tr}, B_{te})$			$U = 330.5, p < .001$

Balanced

$H(2) = 33.097$ $p < .001$	$err_{Elo}(B_{tr}, B_{te})$ M=0.40	$err_{Elo}(N_{tr}, B_{te})$ M=0.40	$err_{wh}(\bullet, B_{te})$ M=0.43
$err_{Elo}(B_{tr}, B_{te})$		$U = 1184.0, n.s.$	$U = 531.0, p < .001$
$err_{Elo}(N_{tr}, B_{te})$			$U = 528.5, p < .001$

Predicting Non-bipartite

Unbalanced

$H(2) = 38.356$ $p < .001$	$err_{Elo}(B_{tr}, B_{te})$ M=0.40	$err_{Elo}(N_{tr}, B_{te})$ M=0.40	$err_{wh}(\bullet, B_{te})$ M=0.42
$err_{Elo}(B_{tr}, B_{te})$		$U = 1270.0, n.s.$	$U = 455.5, p < .001$
$err_{Elo}(N_{tr}, B_{te})$			$U = 492.0, p < .001$

Balanced

$H(2) = 32.764$ $p < .001$	$err_{Elo}(B_{tr}, B_{te})$ M=0.40	$err_{Elo}(N_{tr}, B_{te})$ M=0.40	$err_{wh}(\bullet, B_{te})$ M=0.43
$err_{Elo}(B_{tr}, B_{te})$		$U = 1185.0, n.s.$	$U = 502.5, p < .001$
$err_{Elo}(N_{tr}, B_{te})$			$U = 567.0, p < .001$

Table 1: Prediction error using FIDE data set. Kruskal-Wallis test results top-left in corner; Wilcoxon Rank Sum test results in individual entries.

between training on the bipartite and or non-bipartite subgraphs; this would indicate that a subgraph being bipartite did not have a detrimental effect on predictions (even when predicting non-bipartite matches that could never have taken place). The exception is the case of predicting the bipartite test set with unbalanced subgraphs. In this case, the unbalanced bipartite subgraphs outperform the non-bipartite subgraphs.

To further explore why this might be the case, we looked at the distribution of matches within each type of subgraph. Looking at the histogram of the degree of individual vertices (Figure 4), it is clear that the unbalanced bipartite subgraphs have a few vertices of unusually high degree. These would correspond to players who have participated in a large number of matches: sometimes over 200, whereas no players in the the other subgraphs participated in more than 50 matches. In the unbalanced bipartite subgraphs we constructed, there are a few important vertices with high *degree centrality* that participate in a large number of matches: therefore, we may have a very good estimate of their skill and improved prediction on their future matches. At the limit of bipartite graph unbalance, there would be an one player participating in all matches. Similarly, in an unbalanced bipartite subgraph, there are simply fewer matches to choose from overall, and therefore in with a given number of matches, there will be more repeated matches, and fewer possible matches to test on.

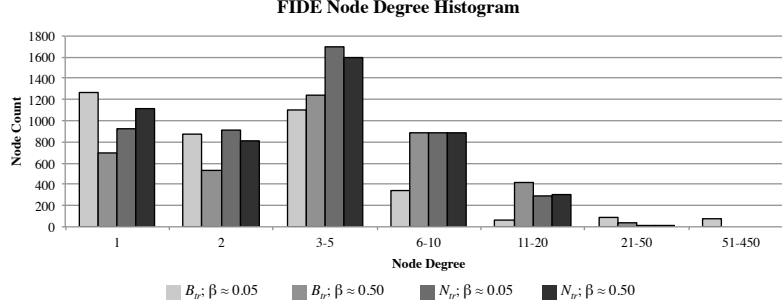


Figure 4: Histogram of vertex degrees (matches for a player) from FIDE data set. The median count is shown.

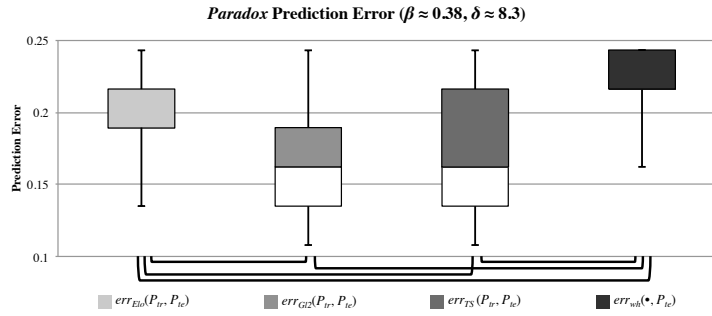


Figure 5: Prediction error for *Paradox*. Box plots show the minimum, first quartile, median, third quartile, and maximum prediction errors. Lines under the plot show statistically significant comparisons.

Paradox Prediction Error

Using the *Paradox* data set, we constructed 50 bipartite training subgraphs with 750 matches and $\beta \approx 0.38$; corresponding test subgraphs had 37 edges. In this case, white is the player, and black is level; there were relatively more levels than players. We compared the prediction performance of the Elo, Glicko-2, and TrueSkill systems. As a baseline, we chose always predicting white, as players were more likely to win than lose levels overall in the data set. Prediction results on test subgraphs are shown in Figure 5 and Table 2.

All three trained prediction systems outperformed predicting white, improving median prediction error up to 25%, and both Glicko-2 and TrueSkill outperformed Elo (though neither outperformed the other).

A histogram of the Elo ratings of levels and players is shown in Figure 6. Unsurprisingly, the players generally have higher ratings than the levels; this makes sense as in the data set players usually win. The area of overlap where some players have lower ratings than the levels may indicate where the system will predict players lose, and allow trained system predictions to outperform always guessing the player will win.

Paradox Prediction Error ($\beta \approx 0.38, \delta \approx 8.3$)				
$H(3) = 59.958$ $p < .001$	$err_{Elo}(P_{tr}, P_{te})$ M=0.19	$err_{G12}(P_{tr}, P_{te})$ M=0.16	$err_{TS}(P_{tr}, P_{te})$ M=0.16	$err_{wh}(\bullet, P_{te})$ M=0.22
$err_{Elo}(P_{tr}, P_{te})$		$U = 1829.0$ $p < .001$	$U = 1770.0$ $p = 0.001$	$U = 728.0$ $p < .001$
$err_{G12}(P_{tr}, P_{te})$			$U = 1241.0$ $n.s.$	$U = 339.0$ $p < .001$
$err_{TS}(P_{tr}, P_{te})$				$U = 409.0$ $p < .001$

Table 2: Prediction error for *Paradox*. Kruskal-Wallis test results in top-left corner; Wilcoxon Rank Sum test results in individual entries.

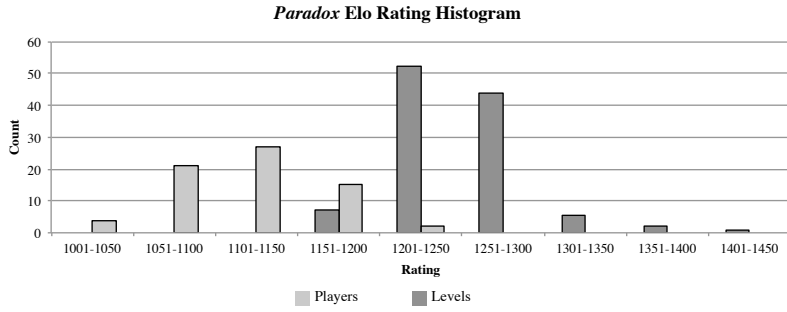


Figure 6: Histogram of player and level Elo ratings. The median count is shown.

CONCLUSION

Contributions and Discussion

In this paper, we identified lacking difficulty balancing as one potential contributor to the low retention rates of HCGs, and outlined four design constraints that impede the use of standard difficulty balancing strategies for HCGs: HCGs (1) work with predetermined tasks (2) whose difficulty is unknown and (3) cannot be manipulated without risking the scientific validity of outcomes or creating artificial inefficiency; furthermore (4) manually identifying and balancing difficulty defeats the purpose of cost-efficiently crowdsourcing information tasks. We argued that a promising route for HCG difficulty balancing is computationally identifying task difficulty and user skill and *selecting and sequencing tasks* to fit user skills. We suggested that player rating systems used in multiplayer matchmaking can be plausibly used to identify user skill and task difficulty, drawing on existing user solution data.

We identified the bipartite structure of user-task graphs in HCGs as a potential issue for this approach, particularly in unbalanced graphs, which can be expected to occur in HCGs. An empirical test of three common rating systems (Elo, Glicko-2, and TrueSkill) used on chess match graphs found no negative impact of bipartiteness on outcome prediction accuracy, suggesting that player rating systems work with bipartite graphs. We found prediction accuracy to actually be *higher* with unbalanced graphs, which is potentially due to the presence of “super vertices” with a very high degree centrality. This further supports the applicability of our approach to HCGs, as these commonly feature a number of “superusers” with likewise high degree centrality (Sauermann and Franzoni 2015).

Finally, we tested the prediction accuracy of the three systems with a player data set from the HCG *Paradox*. All three systems performed better than predicting that the user always “wins”, i.e. reaches the target score in each task, with TrueSkill and Glicko-2 outperforming Elo. This provides converging evidence for the viability of our approach, and suggests that the general advantages of TrueSkill and Glicko-2 hold in an HCG context.

In summary, our results suggest that applying standard player rating systems to HCG log data is a viable strategy for rating user skill and task difficulty, predicting task outcomes and thus, balancing difficulty in HCGs and user-generated-content-driven games.

Limitations and Future Work

One obvious limitation of our approach is that it relies on data from previous solution attempts. As we have seen, this is problematic for HCGs, where most users only engage once with few tasks (Sauermann and Franzoni 2015). To some extent, this might be an “empty bar problem” of reaching a critical mass: make the game engaging enough, and users will stay longer, generating more data which will help make the game more engaging. One partial solution is to have established “superusers” to play unplayed levels, relying on their investment in the game, and serve these once-played levels to novice players for verification. Another partial solution is to quickly assess user skill by exposing all users to a set of “calibration” levels where the true difficulty is known, e.g. during a tutorial phase. A final option is to computationally extract task features that correlate with existing user solution data and thus, task difficulty. In future work, we intend to triangulate solution data, qualitative analyses of features that constitute the difficulty of tasks (following Cook 2007; Deterding 2015) and machine learning to extract and validate task features that indicate difficulty. This way, our system would use two signals—solution data-based ratings and task features—to predict success probabilities for both played and unplayed tasks.

Second, the tested rating systems only compute win/loss, a very coarse outcome measure. In many HCGs, there are gradations of outcome quality, sometimes even with an unknown optimum. *Paradox* is a case in point: for the purposes of this paper, we artificially reduced a scored degree of constraint solving into reaching or missing a target score. More granular outcomes should provide richer information about skill and difficulty. Hence, future work will explore how to modify rating systems to allow for gradations of outcomes.

Third, we trained the tested systems on relatively large data sets. Given that many HCG volunteers participate only once and for a short time, a crucial question for practical feasibility is how fast rating systems learn, that is: how many solution attempts per task and user do they need to produce acceptably accurate predictions?

A fourth limitation concerns the use of just one particular HCG, whose features are not representative of the variety of HCG types (Pe-Than et al. 2015). Future work will use existing typologies of HCGs to rerun tests with data sets across a representative sample of types.

Fifth, while the link between challenge (measured as winning odds) and engagement is empirically established, and the present data reanalysis shows that we can predict winning odds with some accuracy using the proposed approach, more empirical work is obviously

needed, validating that HCGs employing the proposed balancing system result in higher player engagement.

Sixth and finally, a possible issue of balancing through task selection and sequencing is that a task pool at a given time doesn't hold tasks of acceptable difficulty for a given player—for instance if task pools are unevenly distributed in many low and few high difficulty tasks. We suggest that in these instances, procedural content generation could be used to insert “bridging” tasks that both smooth the difficulty curve and prepare users for upcoming high difficulty tasks.

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