CHAPTER 2: COUNTERING FOUR RISKY ASSUMPTIONS

Project website: GameImpact.net

LONG TERM PROJECT BENEFITS

*Here are some possible gains from reducing fragmentation, some of which we hope to investigate in future publications. Have ideas? **Please contact us!** Shaping the field requires alignment on many levels -- from individual games to distribution, and research models.*

(a) For Game Designers and Makers

The process of research can seem daunting, but in fact all game designers engage in research when they test their game for playability. More work is needed to help bridge such formative approaches with summative evaluation. If the lack of evaluated games is any indication, a common scenario is to focus on creating the game and worry about evaluation once it is done (if at all). However, this approach typically leads to games that are only loosely optimized to meet their impact goals. The creative process can embrace impact theories as generative constraints, not an external annoyance. In the longer term, this project aims to help foster communication between game developers and researchers. We want to make it easier for game designers and researchers to apply their skills in aggregate.

(b) For Funders, Impact Investors, and Publishers

Funders have a tough job: to determine which projects merit investment, given the risks and impact alternatives. Evidence-based approaches are becoming common, but they are not sufficient and in fact can easily be misunderstood. Understanding fragmentation can help funders to justify their investments in more accessible language. More fundamentally, games are so inter-disciplinary that success may depend on collaboration across funding agencies to build valid models for what works. Reducing fragmentation can help multiple funders to coordinate and build a solid research base across the sector.

(c) For Researchers

A desired benefit of this project is to improve access to common, agreed-upon metrics that will be customizable for new game projects. Community standards will help researchers appraise the impact of social impact games more quickly. This will speed the process of acquiring funding for new or existing projects by not having to constantly convince funders of the basics. New researchers will have a starting point from which to learn about methods of impact and how to apply them to game projects. At the same time, researchers will better be able to suggest how they can support the design process throughout, improving the quality of the game and not simply measuring its effects.

(d) For NGOs and Cause-driven Managers

It has frequently been the role of cause-driven organizations (also called NGOs, social innovators, etc.) to act as "go-betweens." In game projects, they have the relationships with funders, game developers and researchers. Without ways to connect various frameworks of impact in games, producers struggle to coordinate and communicate the impact goals across funders, developers, and researchers. More straightforward language across disciplines and sectors will help these groups to do less translating and have greater confidence that their projects are living up to their impact potential.

COLLABORATING INSTITUTIONS

For a full list of discussion and distribution partners, see Appendix in the full report.

We are publishing these "four risky assumptions" on their own, separate from the main report. There are two reasons: they are useful independently, and they emerged after the initial report was circulated for feedback.

The original report on "A Fragmented Field" identified the high stakes and difficulty of obtaining the big picture for impact with games. After more than a decade, games that address social issues are at an inflection point: the funding base is broadening, and so is the language of impact. The result being that we often talk past each other when designing for and studying impact with games.

The assumptions presented below all reflect our deep fear that the **gulf between research and practice** may be growing as silos deepen. We seem to be missing a shared language of impact -- many terms are unwittingly divisive, and their power elevates one kind of game while undermining another. Both sides must come together: if developers refuse to model impact, or if researchers undermine the beauty and art of games, we will not succeed.

Each of the four assumptions below is sneaky, and seems to aggravate the field's fragmentation. The evidence for these deep assumptions, though often attested by leaders in the field, is indirect; therefore, this section offers careful provocations rather than definitive conclusions.

RISKY ASSUMPTIONS

WHEN FUNDING IS SCARCE, DELAY 'RESEARCH DESIGN' AND RESEARCH

In times of funding scarcity (i.e., always), difficult decisions about priorities have to be made. Scarcity raises questions about what can be separated, and what can be sequenced. While it may be appropriate to delay the execution of third-party research, we warn that it is danger-ous to defer the "research design."

Research design (aka the "blue print" of the study) can be just as important and difficult as game design. But **don't confuse the research with the research design**. The research design is a planning phase, and is part of the design process — without data. We can think of the research design as a kind of "creative problem solving" that is required to convince ourselves — and others — that there was impact, what kind of impact, and based on what evidence and logic.

Difficult decisions about the sequence of design and research still need to be made, even assuming the research design is determined early. One way to empower designers and produc-ers is to **make the strategy more visible**, so that all stakeholders can understand how research is sequenced strategically. For example, consider these diverging viewpoints (we are not endorsing any of these as right, but do think all should be on the table):

A. Delay all research. Only fund research when the product shows promise.
B. Always allocate 5% to research. Such rigid formulas are not unusual for "program evaluation."
C. Either 0% or 500%. The cost of some research designs go far beyond the development resources, leading some to take the attitude that anything less than full funding is a waste of resources.
D. Scaling is the only question worth investing in for research.
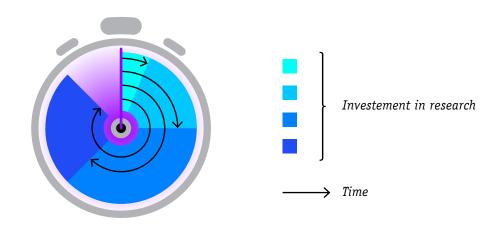E. Quality is the only question worth investing in for research, since the market should handle everything else.

Figure 1: Research can begin at different times, and take different amounts of time

Investement in research

Time

The greatest danger may come from **repeatedly picking the same option** without thought. To counterbalance, our field might push each game project to declare how they sequence and frame design and research, thus necessitating some (public) reflection about which combination is best for their situation. Similarly, **funders with a wide portfolio** of games should be pushed to reflect on how they approach research across a set of games; for example, some projects might be primarily about answering a research question, while others extend established research and so might need less resources to establish they are indeed aligning with a proven impact model.
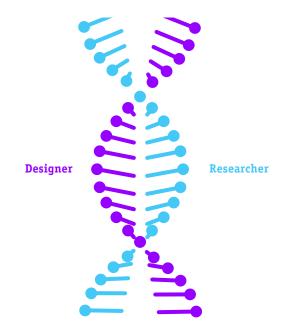
> *...positive solution:* "Always have a research design, but decide case-by-case on the investment to collect specific data."

RESEARCH IS SEPARATE FROM DESIGN (AND IS CONDUCTED EXTERNALLY)

A frame of "mutual iteration" will yield better impact for many projects, and simultaneously reduce fragmentation. In part, this requires a broader notion of "research" as overlapping with standard design practice.

Figure 2: Good research is often interwoven with design

Designer   Researcher

With that in mind, we urge more respect for user testing as a kind of essential research, and thus more respect for designers as applied researchers, since all good games require play testing. This is a surprisingly overlooked reality, both by designers and researchers. Ultimately, although there are some understandable reasons for emphasizing and scrutinizing robust research design, we argue that placing research on a pedestal, also comes with risks. Most importantly, impact could be lessened if research is delegated to external sources at the expense of deeper integration with design iteration.

Game designers may not realize their options -- let alone their own role in "research." In particular, when designers see game testing and usability as separate from "research," they may fail to capture valuable data on impact. For example, if they only ask whether their players are "engaged" in a narrow sense, they may miss deeper engagement with the issues that brought the player to the game in the first place (e.g., to connect with others, to engage with a social issue, to have an excuse to make a difference). Of course, some research is impractical for making short-term decisions. But we argue that there is great value in empowering designers to optimize the game with the "research" model -- i.e., the model for observing impact that might be used in a formal evaluation after the game has launched.

Additionally, we suspect that there is particular tactical value in mutual advice between designers and researchers. Specifically, designers can be asked to recommend how they might evaluate the game (summative); simultaneously, evaluators can be asked to recommend how they might improve the game (formative). Improving the linkage between formative and summative research (and formative and summative researchers) seems likely to reduce fragmentation and improve our field-level conversation. Along the way, we are helping to take the word "research" a notch down from its pedestal to be more accessible to all.

> ...*positive solution:* Iterative design should include "mutual iteration" -- including the research approach and "paper prototype" evidence" (they should co-evolve; good designers must think like researchers and vice-versa)

RISKY ASSUMPTION #3

THE LOGIC MODEL IS OBVIOUS

It is not uncommon for game projects to launch without publicly declaring how they expect impact to come about. That's understandable -- it is pretty easy to describe a vision for the outcome, but much harder to explain the causal logic that leads to success. We can describe the gap as a missing or underdeveloped logic model.

(For those new to the nonprofit sector, logic models are used by organizations to plan and account for their impact, and are often spelled out when organizations dive into strategic planning.)
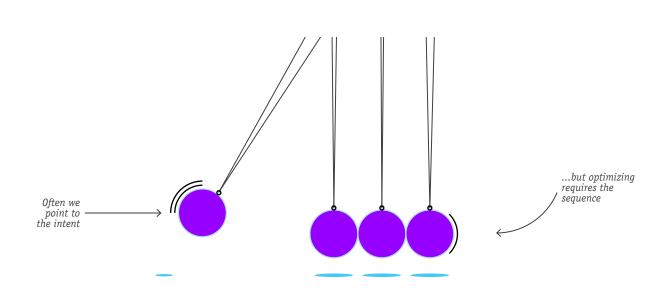
Figure 3: The logic model is what-caused-what

Often we point to the intent

...but optimizing requires the sequence

Particular danger comes if design teams consider their model "obvious." What that often means in practice is that the "logic" is only descriptive -- without causal claims. For example, "the players will learn math through Dominoes" is a start, because it implies a causal factor (Dominoes). However, it does not specify how playing dominoes actually leads to math skills. To do that, you might say that "math is deeply learned through practice, and Dominoes forces players to practice basic math (especially dividing by five)." More radically, you might also say that "playing Dominoes in teams can create a 'need to know' that catalyzes much faster acqui-sition of math skills like division -- including by showing players the social benefits of being skilled at dividing by five."

What are the benefits?

- Unexpectedly, articulating your logic can be wildly **generative**. Even simple models lead to new ideas -- including new ideas about how to optimize design, wrap around services, and track impact.
- For the field, there will be **fewer misunderstandings** between stakeholders. That's because all games have multiple pathways to impact; in other words, they're complex! (In terms of the report's main claims, we can reduce fragmentation in claims #1 and #3 with better logic models.)
- Finally, by specifying the logic of a game, the whole field will understand the game better. Looking across games, the logic model is what allows us to **"generalize" success** and try to improve a whole set of games... categorically!

Fortunately, anyone can articulate the logic model with a bit of effort. Simply state "what caused what" (or take your best guess!). Be brave. Making your logic public can feel a bit exposed and out on a limb -- but it also shows a kind of deeper confidence. When the game is just being released it is tempting to keep you cards close, but there are deep benefits to the field (and the game!) of proactive transparency.

> ...*positive solution:* "Articulate HOW your impact is happening (be transparent, be brave, reveal your logic model)."

TO SCALE IMPACT, OUR GAMES MUST BE MASS MEDIA

Who doesn't want scale? Surprisingly strong emotions often swirl around the topic of scaling. The problem is that assumptions on scaling can obscure alternatives to how change happens in the world.

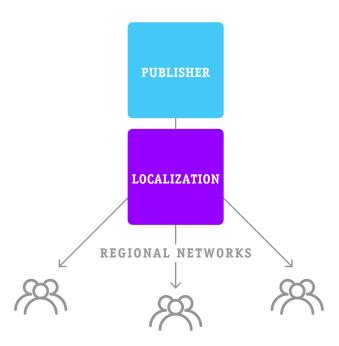The most common assumptions are true... *sometimes*. Consider:

- "We want impact... **as mass media**" (e.g., we need a massive audience -- so without a million downloads, why bother?)
- "We want scale... just **like commercial videogames**" (e.g., unless we can compete with commercial titles, how can a game have impact?)
- "We want scale... **by changing policy**" (e.g., unless the game changes a law, who cares if it affected public opinion — because we need structural change, right?)

...none of these is "wrong" per se, and the policy emphasis is strange enough to many artists, but they can obscure other possibilities.

Consider these *alternative scaling approaches:*

- Games can be used in a campaign that seeks to "**shift the culture**" of a community by triangulating several local interventions (e.g., to establish a "college going culture" in a particular high school, see this FutureBound study). Such triangulation is hard to achieve nationally, and so is more often pursued in cities, states, or even within a particular school.
- Some game projects embrace **local customization** as an approach to achieving scale, despite the costs. Theses projects resist the idea that a single international implementation would be effective for local communities. Much like local parks and economic planning, these games approach scale as the "mass localization" of an approach, in opposition to replication.

Figure 4: An alternative scaling model

Both emphasize a level of granularity beyond players and mass media. Instead of starting at the individual level (player) and scaling directly to "mass audience" level, they insist on the importance of establishing a coherent context like local culture.

Even traditional games can benefit from multiple models for scaling. Most simply, one game may actually have impact on multiple levels. For example, a game might set out to shift individual behavior, but discover it has shifted cultural norms as well. Simply to be good observers of our own games, we may need to actively stay open-minded to secondary and unintended impact models.

More proactively, a team with enough capacity and care might begin to combine several kinds of scale deliberately. For example, after launching a mass media game in the Android store, the team might also launch a series of community-based discussion groups. In fact, this may be the best strategy for ambitious goals like policy and social reform, which are never unidirectional but transform when society reaches a tipping point. Ultimately, our best games may be appropriated to target additional goals and secondary campaigns, gathering coherence for reform like a snowball.

Overall, we try to stay agnostic and resist picking one "best" model for scale. Our recommendation is to beware the assumptions that come with singular notions of scale — especially seeking scale via a mass media approach. Better games will come from making decisions about scale, rather than defaulting into an assumption. As a field, we can help each other identify secondary scaling opportunities and listen more deeply when we make room for multiple pathways to societal change.

> *...positive solution:* "There are multiple ways to reach scale (not just as mass media) for many games, and definitely for the field as a whole."