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IS OUR RESEARCH INTERESTING: A REFLECTION

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Abstract

In recent decades, research in Information Technology in Construction (ITC) has flourished, with growing output across topics such as BIM, automation, AI, and digital twins. However, this growth in quantity has not necessarily been matched by intellectual novelty. This paper critically examines the “interestingness” of ITC research, drawing on theoretical frameworks from Murray Davis, Karl Popper, Thomas Kuhn, and Gray & Wegner. It argues that while much current work is technically competent, it too often confirms existing assumptions and follows predictable patterns, rarely challenging paradigms or inviting surprises. Drawing from Davis’s definition of interesting research as that which denies its audience’s assumptions, and Popper’s principle of falsifiability, the paper critiques the field’s bias toward positive results and safe hypotheses. It then presents Gray and Wegner’s six guidelines for interesting research—such as making the hidden visible, resolving paradoxes, and reframing goals—and demonstrates how these can be applied to construction IT. The paper offers actionable recommendations: designing studies that risk failure, embracing negative results, integrating qualitative methods, and aligning research with fundamental industry challenges. Ultimately, it calls for a shift from merely accumulating solutions to cultivating intellectually vibrant, paradigm-aware scholarship. By valuing bold ideas alongside practical rigor, the ITC community can produce work that not only informs practice but also challenges norms, reshapes understanding and becomes more likely to uncover novel paths.

Keywords: construction information technology, research method, interesting

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1. Introduction

Over the past three decades, the author has worked in the research and academic field of information technology in construction (ITC), contributing to the development of the field both as a scholar and in editorial roles in prominent journals such as Automation in Construction and ITcon. During that time, the volume of published research has grown exponentially. From 2010 to 2020, the global output of scientific and engineering articles increased from 1.9 million to 2.9 million annually, a 52.6% rise. In 2022 alone, approximately 3.3 million scientific papers were published worldwide [1].

This growth is reflected in construction research as well. The number of papers published in Automation in Construction tripled between 2015 and 2024 [2]. The trend is similar with other journals in the field. Additionally, new journals have been added.

Much of this growth is driven by the "publish or perish" imperative in academia and the business models of academic publishers, now mostly relying on the authors to pay for the publication of their work. As publications remain a key metric for career progression and institutional evaluation, the quantity of output may begin to outstrip its conceptual value.

This raises the question this paper addresses: in an era of prolific publishing, how much of the research being conducted, and more importantly, how many of the publications published are truly interesting?

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2. Defining Interesting Research

The notion of "interesting" research has been explored in the social sciences and philosophy of science. Davis [3] argued that a theory is interesting when it denies an audience's assumptions. Rather than confirming existing beliefs, interesting work challenges orthodoxy and compels the reader to reconsider what has been taken for granted. In this sense, a paper's appeal lies not only in its conceptual soundness, scientific novelty and problem-solving utility, but primarily in its capacity to surprise.

Gray and Wegner [4] extended this idea with six guidelines for interesting research. They emphasized that interesting research is often provocative and theoretically generative, not necessarily the most rigorous or methodologically novel. Thus, the value of a research paper does not always correlate with how well it conforms to established beliefs; it lies in its intellectual impact.

3. Kinds of Engineering Research

Most papers published in construction and engineering journals fall into three broad categories: papers that review exisitng literature, papers that test a hypothesis on the nature of social, technical or natural reality, and papers that propose a novel technical solution to a problem. In construction fields related to computing, the research involves implementing or developing some kind of information technology. Generally, the authors are finding that the new technology is useful. A working prototype is proving that that technology can be used to address a problem. Hypotheses are rarely refuted and problems are usually solved. Unsurprisingly, problems can be solved with IT which previously have been solved without it. This creates a paradox: while nearly all studies appear "successful," few challenge foundational assumptions or risk failure. Most follow well-trodden routes, pivot around well-established solutions, apply a methods to yet another topic. Few open genuinely new directions or offer results and solutions that could not have been foreseen.

This observation resonates with Popper's [5] criterion of falsifiability, which argues that a scientific hypothesis must be refutable. Research must have a possibility of failure. However, prototypes always succeed, because only rarely thresholds of failure are defined. When methods only validate what is already known, then research ceases to be scientific in Popper's sense.

Furthermore, Kuhn's [6] concept of scientific revolutions suggests that science progresses through paradigm shifts—moments when prevailing theories are overturned. However, in construction informatics, there appears to be a constant micro-paradigm churn, where new models are created for each new application without stabilizing conceptual foundations. As Turk [7] observed, the field operates in a near-continuous paradigm flux, which challenges cumulative knowledge building.

4. Applying Six Guidelines to Creative Construction Research

Gray and Wegner (ibid.) propose six guidelines for making research interesting: (1) Make the hidden visible by uncovering overlooked mechanisms or structures; (2) Make the invisible salient by drawing attention to underappreciated factors; (3) Resolve a paradox by offering a surprising explanation for conflicting observations; (4) Find a paradox by revealing contradictions in what appears consistent; (5) Reframe the question to shift the perspective or assumptions behind a familiar topic; and (6) Change the goal by challenging the purpose of the research itself. Together, these encourage creativity, surprise, and deeper theoretical insight.

There are opportunities for the above in ITC research and will be discussed below.

4.1. Make the Hidden Visible

Construction IT research frequently concentrates on improving tangible system components—interfaces, tool functionality, or performance metrics—while neglecting the less observable dimensions that shape technological outcomes. Aligning with Gray and Wegner's principle of "making the hidden visible," some of the work should focus on latent assumptions, structures, and sociotechnical dynamics that influence the success or failure of digital construction technologies.

One example is the treatment of tacit knowledge in BIM-enabled workflows: although BIM is widely perceived as a universal tool, users interpret models differently depending on professional background, organizational culture, and local conventions, which often leads to hidden misalignments in project coordination [8,9]. Similarly, while interoperability is often framed as a technical problem, hidden frictions—such as informal workarounds, proprietary lock-ins, or conflicting model semantics—persist beneath the surface of standardized exchange protocols [10]. Automated scheduling platforms, often celebrated for their optimization capabilities, may conceal risks related to algorithmic rigidity, underdefined fallback mechanisms, and loss of situational judgment, particularly under dynamic site conditions[11]. Research employing ethnographic inquiry, system audits, or post-mortem failure analysis can illuminate these unseen influences, thereby contributing to both the academic robustness and practical relevance of ITC.

4.2. Make the Invisible Salient

A complementary guideline from Gray and Wegner, “making the invisible salient,” urges researchers to emphasize overlooked dimensions that subtly but significantly shape system behavior. Within construction IT, much scholarship prioritizes information and product modelling, its richness and structure, while ignoring resulting process changes, measurable functional benefits, social dynamics, and psychological dimensions of user engagement. For example, the cognitive burden associated with complex BIM interfaces is an underexplored factor in user adoption, despite increasing system sophistication and task demands [12,13]. Likewise, while augmented reality (AR) is often framed as a productivity-enhancing overlay on the built environment, its implications for privacy, surveillance, and digital dependency have been insufficiently addressed in the literature [14]. Another invisible dimension involves the introduced process rigidities, not lean but obese information management and displaced goals – from ITC supporting building physical structures to ITC striving for increasingly complete information models [15]. Not enogh attention is given to psychosocial effects of technologies, includign BIM and robotics—such as perceived threats to job security, altered team dynamics, or diminished worker autonomy—despite early signals of these impacts in adjacent industries [16]. Future research that foregrounds these human and social concerns, using empirical and interpretive methods, would enrich the discourse with insights that move beyond technological progress for its own right to improve technology transfer, use and have a positive impact on practical productivity.

4.3. Resolve a Paradox

Construction IT is sated with contradictions that offer fertile ground for theoretically interesting research. One such paradox involves Building Information Modeling (BIM) 17: while often promoted as a collaboration-enhancing technology, empirical studies frequently report its role in reinforcing disciplinary boundaries, data silos, and hierarchical control structures [18,19]. Similarly, digital workflows intended to simplify project coordination can introduce new layers of complexity through redundant documentation, rigid standards, or excessive information exchange formalities [20]. These contradictions highlight the tension between intent of technology and emergent practice. Research that explicitly identifies such inconsistencies and proposes either theoretical explanations or design alternatives—such as adaptive governance frameworks, participatory modeling tools, or hybrid organizational models modelled after social media networks—could help resolve these tensions while advancing conceptual clarity. This aligns with Gray and Wegner’s notion that paradox-resolving research does not merely smooth over inconsistencies but reveals the structural or epistemic misalignments that give rise to them in the first place.

4.4. Find a Paradox

Beyond resolving known contradictions, compelling research can emerge from uncovering paradoxes where they are not yet recognized. In construction informatics, one widely held belief is that increased data availability leads to improved decision-making. Yet as digital dashboards, real-time KPIs, and sensor-rich environments become ubiquitous, project managers increasingly report symptoms of information overload, reactive behavior, and reduced autonomy [21,22]. ITC research has been extensive, yet industrial uptake has been trailing expectations [23]. Digital tools are being introduced to construction, yet productivity remains stagnant [24]. A similar paradox exists in the domain of construction robotics: while designed to reduce physical risk and improve productivity, these technologies may introduce new hazards, such as overreliance on automation, loss of tacit skills, or ethical concerns regarding workforce displacement [25]. In digital twin research, escalating fidelity and data integration can render systems so complex that they become inaccessible to frontline users, thereby contradicting their intended function as intuitive operational aids [26]. Research that reveals these paradoxes through ethnographic, reflective, or systemic approaches can shift prevailing narratives and open up new avenues of inquiry grounded in the lived realities of digital construction.

4.5. Reframe the Question

Reframing research questions enables a shift from incremental problem-solving to deeper epistemological critique. Traditional construction IT research often asks how digital tools can be better implemented or optimized. However, reframing these questions can open new research territory. Rather than asking how to improve BIM interoperability, one might ask what kinds of knowledge or labor practices are devalued or excluded by formal modeling processes [27]. Instead of asking how artificial intelligence (AI) can enhance scheduling precision, researchers might ask how algorithmic systems embed specific managerial ideologies or labor relations into the sequencing of work [28]. These reframed questions invite interdisciplinary inquiry and expose the normative assumptions built into construction technologies. They also invite researchers to examine not only how tools are used, but how they structure thought, behavior, and organizational culture. This orientation resonates with critical technology studies and aligns with Gray and Wegner's encouragement to reshape the underlying premises of inquiry itself.

4.6. Change the Goal

The final guideline proposed by Gray and Wegner, "change the goal," calls for questioning the very objectives driving a research agenda. In ITC, much work is oriented toward improving interoperability and increasing the amount of information available. While valuable, these aims often limit the horizon of what digital technologies could achieve. A shift in goal setting might involve asking how ICT can actually contribute to more specialization, higher productivity, and by adding elements of disintegration. Instead of optimizing project delivery timelines, a study might explore how designing systems can prioritize climate resilience or how planning systems could address worker well-being [29]. Instead of maximizing robotic automation, research might investigate how human-robot collaboration can enhance job satisfaction, skill development, and team cohesion on construction sites [30]. Such reframing moves beyond technical performance and opens ITC research to a richer set of values and goals. These alternative trajectories do not reject technology's utility but rather reposition it within broader context.

5. Conclusion

The sheer growth of publication in construction research demands a critical re-evaluation of what makes research worth reading. Interesting research does more than solve problems or prove hypotheses; it unsettles assumptions, reframes questions, and provokes new ways of thinking. Engineering disciplines, including construction, need to re-engage with methodologies that allow for this kind of exploration—be it through philosophical reflection, action research, or theory-challenging critique.

ITC research, to be truly creative, must become comfortable with ambiguity and failure, not just pivoting around small optimizations and confirmation of the obvious. By seeking out non-obvious problems and embracing broader methodological toolkits, the field can produce research that not only informs, but also inspires.

6. References

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