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Are current definitions of 'research' stifling 'ideas' that might be relevant to our discipline? This article examines this question in the light of design, history/theory, teaching, and advocacy.

Ideas matter

Peggy Deamer

It was in the 2000s that I first learned about the tyranny of 'research' in our discipline. At the School of Architecture and Planning at Auckland University, where I was Head of School in 2007, the department's budget was linked to faculty 'research' output. The negative effects of this lined up like dominoes; research defined as publication couldn't recognise published work that was visual and not 'peer reviewed'; the smart faculty members learned quickly that teaching didn't bring in money and hence didn't contribute to promotions ('only the stupid still teach'); the production of 'research' was linked to having a PhD (architects teaching studio had to get doctorates); and schools of architecture responded to this new demand for doctorates by establishing new postgraduate degrees (think RMIT and its PhD in thinking hard about your design methodology – I don't consider myself an intellectual snob but I recognised that this was not what I did for my doctoral thesis). The irony was that this model was said to be based on that of the US, which encouraged 'innovation'. In reality, as we know, it was based on a business model of monetising research. In order to force this, 'innovation' was marshalled into a system of risk management. The humanities, aesthetics, performance, critical theory, political speculation – anything that didn't treat research as funding opportunity, as archival work, as social or empirical science, or as data-collection – was deemed a waste of university money.

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Design knowledge

I first thought about architectural research in the early 1990s when colleagues and I were teaching studio in a Foucaldian manner. Encouraging students to explore the spatial effect of surveillance in, say, an airport terminal, or hidden sexism in the organisation of domestic typologies, students had to research those conditions. In the 2000s, 'research- based' studios began to appear in the US schools of architecture (think Cecil Balmond's studio on non-linear systems of organisation or Kieran Timberlake's on prefabrication at the University of Pennsylvania), implying not only that there were topics too large for an individual student (or faculty member) to tackle on their own, but that research had moved from program to production; what could digital platforms do for form, material, and procurement? In the 2010s, the 'lab' studios that came into existence (think Columbia's GSAPP)[1] entailed a move away from production to a new definition of design: mapping, spatial quantification, and demographics not only required research but design became the visualisation of this research.

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But the trajectory of these various intersections of research and design – all of which support my belief that architects gain power by proving they are smart, in the world, and socially connected – have also made me fearful that ideas about 'design' get thrown out with the bathwater; that an emphasis on positivism not only indicates that designers have heard the siren song of funding (data!), but that the power structures embedded in the methods by which actual design gets implemented (architectural practice structures, architecture consultants, funding, contractors, subcontractors) get overlooked precisely because they are too familiar to warrant research.

History/theory knowledge

When I started on my PhD in 1982 at Princeton University, my only idea of research was theoretical. I had done my undergraduate work in

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philosophy; I had been an intern at the Institute for Architecture and Urban Studies working on linguistics and form; I wrote my application paper to Princeton on the relationship between Wittgenstein's philosophy and his sister's house in Vienna. I thought that when I started my coursework, I could continue to do theory work on form. (The program, after all, was identified as 'history/theory/criticism.') I quickly learned that research meant doing history. I struggled. But I learned the value of historical research; I saw that the chronological movement of an idea was essential to understanding its significance; I learned that access to the actual notebooks of my subject – Adrian Stokes – drew the invaluable link between biography and ideas. Still, throughout and as I moved from Stokesian ideas to psychoanalytic concepts to Marxist thought, I couldn't stop privileging the quality of the ideas over their lineage. As my history/theory work increasingly merged with advocacy, the emphasis on ideas vs historical research was not a big problem. I wrote what I believed in, and made sure that that belief was at least well-informed and researched.

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But more recently, when participating in history conferences, I've become conscious that my 'research' connects horizontally across disciplinary protocols while everyone else's digs deep into the bowels of specific archives. I've noticed that the more photographs of original documents one can display, the more 'legitimate'/valuable the research is considered to be. It not only makes me self-conscious, it makes me aware of how far the post-critical movement has infiltrated history/theory.

Teaching knowledge

When you teach a seminar in the history/theory area at Yale, where I now work, the students in the course will only get 'required history/theory credit' if they write a fifteen-page research paper. This was instituted because certain seminars taught by studio/design faculty in subjects that didn't fit into any other administratively designated area – for example; technology, design production, urbanism/landscape – were placed in the history/theory area as a catch-all. Not all would qualify legitimately as either history or theory subjects, nor would their deliverables constitute 'research' (that is, primary sources where possible; appraise secondary sources to understand where your argument fits into the discourse). The fifteen-page paper rule was, and is, important quality control that I helped put in place.

But recently, in the seminar I now teach, 'Architecture and Contemporary Labor', the security I have felt over what constitutes a research paper has been shaken. On the one hand, students have wanted to write (and I am somewhat interested in) position papers that propel a new future, don't look to historical models, and are more reliant on white papers and op-eds than library books. On the other hand, the class has had numerous guests whose research is scientifically based and data-driven; these guests, the majority of whom work on sustainability, describe their work as 'real research' (vs digital experimentation – think of the difference between deploying parametrics for environmental analysis vs formal visualisation). I admire this dedication to pulling architecture and design away from theories of intuition, genius, formal experimentation, and individualism. I am thankful that this research makes clear that architecture's engagement with 'innovation' goes beyond things produced on the screen. But when a student asked one of these guests where they thought 'theoretical research' stood vis-à-vis this 'real research', I was sympathetic to the confusion the students felt about what they were doing in my class (theoretical research) and that offered by these empirically oriented guests. Were the students in my class not doing 'real research'?

Advocacy knowledge

I have an ongoing dispute with a colleague who, like me, thinks that the profession of architecture needs a radical overhaul. Understanding the potential of BIM, advocating for new delivery models such as Integrated Project Delivery, and pressing for a closer alignment between the academy and the profession are just some of the mechanisms that we agree could enable reformation in both the profession and the discipline. Unlike me, however, he believes that the only way to persuade architects and owners to change their approach to project delivery and architecture's value proposition is through data – data that proves that the above methodologies save time and (hence) money. I, too, believe that money speaks. I, too, realise that data is persuasive.

'He believes that the only way to persuade architects and owners to change their approach to project delivery and architecture's value proposition is through data.'

But I also believe that architects continue to serve a system that does not have their own best interest at heart and that this is the result of (capitalist) ideology. It is not only that we need to prove to owners and ourselves that money will be saved by a more holistic approach to design and execution, but also that we need to convince ourselves that our current self-identity is problematic. As long

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as we believe that 'architecture is a calling, not a career' and/or subconsciously that Ayn Rand's model of egotistical 'vision' is the path to success, we are in ideological trouble. To counteract this, we must generate fresh ideas about work, life, desire, relationships, management, the imagined future; we must study how, historically, we have willfully subjected ourselves to diminished returns. We can argue about what comes first – our financial reality or our identity awareness – but the singular emphasis on data is, for me, shortsighted.

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Research and knowledge

All of the above notions of research that (in my description) pit it against ideas can be seen as examples of the fluidity of the term in a promiscuous discipline. But I think it is more than this. I think that the four current factors shaping research – funding opportunity, archival work, social or empirical science, data-collection – are historically determined. There is a link between the emphasis on facts and business that is particular to an age that honours the security of information over knowledge; monetisation of the status quo over innovative futures; specialisation over the undefined; science over the humanities; expertise over intelligence. The irony of the conflation of neoliberalism and the 'knowledge economy' is not only the commodification of knowledge but of research as well.

This should not be taken as a diatribe against history in favour of theory; I admire my colleagues who are historians in ways that I will never be; who see that history produces and is produced by ideology. Nor is my concern for the non-empirical an excuse for sloppy thinking. I cringe at the idea that opinions equal knowledge. Rather, my position (or is it my angst?) suggests that the immediate world around us offers evidence of things gone wrong that require new ideas. Ideas matter.

Notes

1. See 'labs' under: <https://www.arch.columbia.edu/research> [accessed 17 December 2017]. The labs listed here – fabrication lab, conservation lab, extraction lab, death lab, global Africa lab, urban community and health equity lab, urban design lab – are a fraction of the number prior to the recent change in deanship.

Author's biography

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