Why Do Experiments?

Paul Erdos (also attributed to Alfréd Rényi) encapsulated part of the academic role in research development with the quote, "A mathematician is a device for turning coffee into theorems." It is not exactly the same situation for engineers, but a similar sentiment holds true because the core research is captured in various theorems, algorithms, and system designs that are then codified in papers, theses, and patents. That said, I firmly believe that an academic's main products are the students: that is, the ones who you have helped train in the art of doing good research and whom are thus well prepared to contribute as researchers in industry, national laboratories, or academia. Although this academic role in research development is relatively clear, after 25 years of work on experimental control systems, I am often asked, "What is an appropriate academic role in experimentation and testing?"

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Of course, part of the answer is that hardware testbeds are often exciting and attract attention, and they are thus excellent outreach and recruitment tools. Furthermore, these experiments also tend to provide an overall perspective on the entire automated system, which helps to identify the weakest link in the system that can lead to new, often unexpected research areas (or, as Prof. Robert Cannon at Stanford University often said to me, "One thing leads to another"). However, there are still many good reasons to ask this question because the challenges of building and implementing a good experiment can be quite daunting. For example, although hardware costs are lower than before, experiments are still expensive and time-consuming to assemble (and fix). In addition, a diversity of skills (algorithms, hardware, and software) is required to conduct experiments, and that typically requires a large student group, which is costly and takes considerable effort to run. Furthermore, working with hardware

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A group photo taken during the Control@MIT event held on November 2, 2018, to celebrate control research on the MIT campus. (From left) (front row) Anuradha Annaswamy, Hamsa Balakrishnan, Ali Jadbabaie, Sanjoy Mitter, Sertac Karaman, and Jacob White; (back row) Mardavij Roozbehani, Luca Carlone, Jonathan How, Marija Ilic, John Tsitsiklis, Jean-Jacques Slotine, Russ Tedrake, Munther Dahleh, and Richard Braatz.

often also involves a lot of busy work, so it can be challenging to balance thesis research versus laboratory work, leading to too much focus on getting things to work and not enough focus on what the experiments will be about once the equipment is working.

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There are also many issues to resolve in the design of these experiments. First, many experiments on robotics and applied control systems are often more like demonstrations than scientific experiments (which include hypotheses and controlled variables). This typically results from a lack of formal instruction on what is an experiment? Therefore, careful consideration must be given to the purpose of the demonstration and/or experiment (that is, what is being tested and compared). The selection of the time and spatial scales of the experiments is also important, especially with operators in the loop (which is often the case in robot experiments). I think that the field should develop scaling laws (similar to the nondimensional analysis of wind-tunnel testing) to ensure that experiment results are relevant (balancing both time and spatial scales of the problem appropriately). The role of communication networks is also notoriously difficult to emulate correctly, so new paradigms are needed for correctly integrating that important technology.

The scope of the experiment (for example, from very simple demonstrations to complex flight campaigns) is also an important consideration. My advice to anyone planning to complete them is to think carefully about the cost–benefit tradeoffs and, as a researcher, decide where you want to be on that decision curve. This analysis should recognize that there are costs and limitations to what can be achieved. Therefore, make sure that goals are realistic and funding/effort levels are consistent with those goals. Specifically, indoor/outdoor operations for robotic testbeds are a major decision point. Although the legal issues associated with operating autonomous unmanned vehicles (for example, aerial vehicles and cars) outside have mostly been resolved, the logistical challenges of external testing are formidable.

The other option of testing inside has become infinitely easier with the growth and robustification of indoor localization (for example, motion capture technology and ultrawideband). However, it often still requires sacrifices of the sophistication of what will be tested. Similar decision points include whether to buy (larger upfront costs) versus build (longer development time) the equipment, whether to use onboard/offboard computation and sensors (onboard often leads to more impressive demonstrations at the cost of much higher risks), and the extent to which software interfaces and standards will be imposed (higher overhead costs but typically more robust and generalizable results).

Good software development is time-consuming, and efficiently passing this code from one generation of students to another is an important issue that affects the ease of performing experiments. In my experience, code is rarely written with the goal of being understandable by others. Because the core technologies used (for example, computers, operating systems, and sensors) change frequently, there is often limited backward compatibility, which quickly renders old students' code obsolete. Thus, there is typically much repeating of the lessons learned over the course of an advisor's career. However, these inefficiencies must be expected and accounted for.

(From left) Lixing Huang, Kunal Garg, Jonathan How, Dimitra Panagou, Vishnu Chipade, William Bentz, Parag Bobade, and James Usevitch during a recent visit to Dimitra Panagou's lab at the University of Michigan, Ann Arbor.

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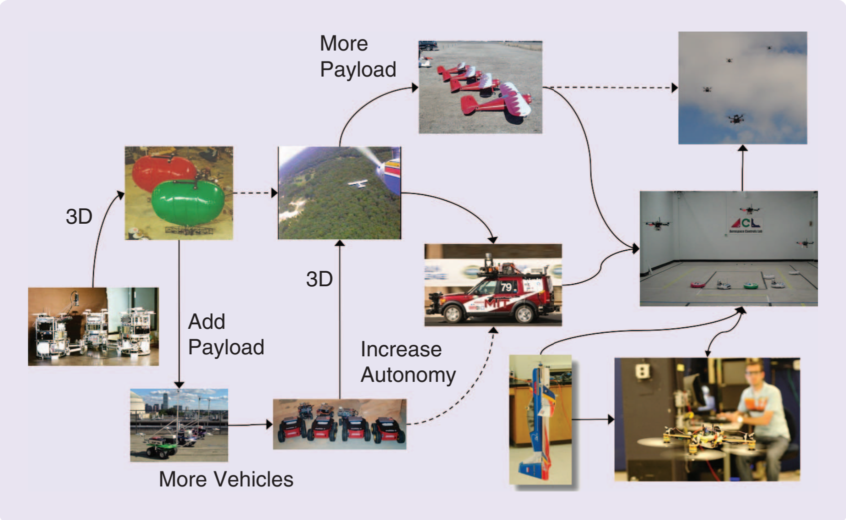

FIGURE 1 The evolution of the experimental platforms developed by my group over the past 20 years.

Even with these challenges, I believe that there are strong reasons to perform experiments. If all "simulations are doomed to succeed" (a quote often attributed to Rodney Brooks), then well-designed hardware demonstrations should add extra degrees of realism that show the technology working as expected. The results would then help anchor the simulations, verify the assumptions made, and lead to new research directions.

That is certainly the case in my career as I have moved from robust control experiments on board the shuttle, to GPS experiments for formation-flying satellites, planning and control of multiple unmanned aerial vehicles (UAVs), and autonomous cars (see Figure 1 for some examples). In each case, the demonstrations and experiments helped me identify new research thrusts. For example, the DARPA Urban Challenge and Fast Lightweight Autonomy program provided me with a much better appreciation of the impact of perception uncertainty in a planning system [1], [2]. The experience with external disturbances during early UAV flight experiments led to a careful consideration of robustness in planning algorithms [3]. The multirobot experiments using mixed integer linear planning noted the complexity of accounting for computation delay in receding horizon control [4]. The experiments with multirobot tasking highlighted limitations of assuming consensus and synchronized operations over dynamic radio networks [5]. Experiments that involved driving robots in pedestrian-intensive locations identified the complexity of modeling complex external environments in planning algorithms [6]. Our external multi-UAV experiments also highlighted the need to account for the imperfections in communication networks in planning algorithms [7]. The internal multi-UAV heath-aware planning experiments highlighted the need for research that automates the battery charge process [8] and improves the stimulus of the perception systems for the robotic teams [9].

On the basis of these experiences, I am a firm believer in the value of performing experiments and/or even small-scale demonstrations for many researchers in the field. Although the results of these demonstrations themselves may not be that important in the true sense of an experiment, simply completing them will often dramatically improve the students' (and researchers') understanding of the main issues. The results can also help validate the assumptions made in the theory and/or identify gaps in the algorithms. As I have found over my career, the insights provided by completing one thing often lead to new perspectives on exciting new research directions. As always, I welcome your thoughts and feedback.

Jonathan P. How

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