Robotics: An Academic Discipline

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Abstract— **Position: robotics is ready to become an academic discipline in its own right, rather than an interdisciplinary field of study, where robotics is the discipline of the creation, composition, structure, evaluation, and properties of artificial (designed) embodied (physically instantiated) capabilities rather than the study of robotic systems and applications.**

I. INTRODUCTION

Thirty-three years after [1] posited that robotics would become an academic discipline in the next ten years, we are on the cusp of finally reaching his goal.

The first necessary step towards this change is the establishment of a definition for the field similar to those found in physics, chemistry, and biology.

II. BACKGROUND

In the natural sciences, fields are defined in terms of the fundamental concept that is the core concern of the field, and the processes or interactions related to that core concept. Chemistry focuses on *substances*, their fundamental properties, and the transformations associated with materials, biology focuses on *organisms*, their fundamental properties, and the transformations associated with life, and physics focuses on *matter and energy*, their fundamental properties, and how they are transformed.

Robotics, however, is currently defined both within and outside the research community as an interdisciplinary field of study, drawing from mechanical engineering, electrical engineering, computer science, and the task domain for which the robot is developed.

Within the community, we have assumed that our fundamental focus is the robot or the system of robots. Our peer reviewed publications, our conferences, our workshops, all rest on this underlying assumption. The experimental portion of our papers indicates seriousness and real world applicability, while the mathematical portion indicates scientific and engineering value. This is hiding the fact that our discipline is not currently a science. We are not applying the scientific method to learn about the properties of the element that is the core focus of our field. Instead, we perform repeatable-in-one-lab experiments and completely-unrepeatable experiments and hope that making a mathematical model of the system and its software will somehow bless our research with the moniker “scientific”.

We devalue the specification and evaluation of autonomous systems, theoretical work that does not fit neatly into an experimental or mathematical paradigm. When the robot is centered in the discussion, the design of behaviors, hardware, and decision making mechanisms is prioritized over developing and understanding the underlying principles behind the experimental results we observe.

Part of our problem is that we experience pressure to develop physical systems both from within and from without. From without, we are inundated with problems that need a physical solution by funding agencies and potential customers because we work on systems designed to perform work. From within, we are pressured to develop physical systems because decades ago, we ran into trouble with a theory-only approach to robotics problems. Solutions relying on the artificial intelligence theories of the day ran into perceptual issues, where the eventual attempts to implement the theoretically-derived algorithms failed due to misunderstanding of the attainable perception of the robot. This drove a backlash against theory-only papers and towards papers that included a physical implementation in the real world as proof of concept. Between this push to prove results and funding agency needs for real-world solutions, we have naturally gravitated towards physical solutions rather than towards theoretical research supporting the underlying design principles behind those solutions.

There is a tremendous amount of work in the field that is concentrated on building newer and better control algorithms, newer and better hardware, newer and better perception algorithms, and newer and better decision-making approaches. There is work going on to apply these achievements to bigger and more complicated problems. But the work necessary to take this ad hoc aggregation of available algorithms and turn it into a science of robotics has been neglected.

III. DEFINITION OF THE DISCIPLINE OF ROBOTICS

The definition of the discipline of robotics should be as follows:

> *Robotics is the scientific and engineering discipline concerned with the creation, composition, structure, evaluation, and properties of embodied artificial capabilities.*

This shift in focus from the robot or system to the capability it provides continues to include all existing robotics work, the vast majority of which is concerned with the creation of capabilities in the context of physical robots and immobots. It encompasses the most advanced autonomous and artificial intelligence-based systems, the least complex [2] vehicle, and the least autonomous robot arm. If the purpose of the robot or its software is to provide a capability, then it is part of the study of robotics.

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If there is no intent for it to ever have a body, and it functions entirely in a virtual world, it would not be embodied and would not be considered part of the field. If it is an emergent property of an organism — fully natural in its function and evolution — it would not be artificial and would not be considered part of the field. Robot-centric capabilities require both embodiment and engineered design.

As with any definition, it leaves gray areas where systems may or may not be included in the discipline. Artificial intelligence algorithms used in a virtual environment, for example, may be included if they may also be used in a robot.

IV. CAPABILITY-CENTERED ROBOTICS

Focusing on the capability rather than the robot enables us to properly address the wide variety of work left to be done as we progress towards a mature discipline. Mature disciplines encompass both theoretical and applied practitioners, are capable of expressing the underlying principles of the field as laws and theorems, and have room in their respectable publications for short position papers, complex mathematical representations, comprehensive experimental data (ideally reproducible experimental data), and a robust approach to evaluation of their systems.

In our current publishing model, graduate students are expected to publish both theoretical and experimental results in the same short conference paper. All our practitioners must be both theoreticians and experimentalists. By shifting to a capability-centered approach to robotics, we enable both the academic study of the components and properties of a capability in the abstract and the structure, implementation, and evaluation of a capability in the concrete.

There are currently no underlying principles of the field in general other than tongue in cheek references to the flawed nature of demonstrations as proof of concept, our lack of ability to reproduce an experiment's results, and our frequent inability to agree on evaluation metrics, even within a common problem space. With the shift to capability-centered robotics, we can define underlying principles based on the ability of *different* systems to accomplish the *same* goal. We no longer have to fight the fact that implementing an algorithm on one robot is not predictive of its performance on another robot or in another environment. Instead, we can evaluate our systems at the capability level. This is what robotics competitions often attempt to do, but we have been looking for underlying principles related to specific hardware and software designs, rather than looking for underlying principles that apply across all robots and all designs.

The majority of our publications have no room for theoreticians or experimental system integration specialists. We have only one journal (IEEE Robotics and Automation Magazine) that explicitly supports publication of replication studies and has a defined process for handling them. Shifting to an explicitly capability-centered approach identifies gaps in the topics that our publications will accept and will drive the development of new or expanded publication opportunities.

Short six page conference papers are expected to provide a level of thoroughness that was reserved for journals as little as ten years ago. A theoretical paper explaining a new method for documenting or cataloging capabilities should have a home in a journal that accepts theoretical papers. Experimental results of a replication attempt should have a home in a reputable publication that accepts articles without extensive mathematical support — the contribution is the failure or success of the replication attempt, the contribution to our better understanding of what capability the original algorithm can actually provide. A new path planning algorithm that succeeds at one path planning challenge environment and fails at another should have a home in a reputable journal — the contribution is our improved understanding of the relationship between the algorithm details and the environments within which it can *and cannot* provide the desired capability.

Evaluation is another problem area. When we focus on evaluating the system rather than on evaluating the capability, we fail to document important information about the system's constraints and flaws in terms of the capability it provides. Applying a capability-centered approach to evaluation of a robotic system forces us to reckon with and document the limitations on its applicability. That reckoning will push us as a field to develop tools and techniques to evaluate capabilities beyond the specific application considered in the development of the algorithm, further contributing to the increased maturity of our field.

V. CONCLUSIONS

Defining robotics as a discipline focused on the capability enables us to mature as a scientific as well as an engineering endeavor.

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