Current and Future Research Interests (word count: 2492)

More than three decades after "sense/think/act" was accepted as a roadmap for robotics research and practice [1], we (the robotics community) have made large demonstrable advances[1] in isolated "thinking," with clearly defined inputs (e.g., *Go* board state) and outputs (e.g., allowed *Go* moves). However, at the dawn of commercially-viable robotics, we need further assurances that our machines will be prepared for interactions (both of sensors and actuators) with an unstructured and unmodeled world, irrespective of whether we believe in deliberative or reactive [2] versions of the sense/think/act paradigm.

When I entered the legged locomotion research arena, the success of Raibert's MIT Leg Lab in the 1980's was a huge influence—demonstrating simple machines that blindly hopped, ran, and performed acrobatic maneuvers [3]. Over three decades later, our state-of-the-art machines underwhelmed us[2] in the face of physical interaction. The DRC challenges were more complex than those solved by Raibert's robots, but it should give us pause that years of preparation were insufficient to generate successful control strategies for a predefined set of tasks. One of the fundamental reasons our robots are "stuck" is the lack of principled methodologies for re-using and re-combining motor control algorithms such as the ones Raibert's machines used—why must we re-invent how to walk and run again and again?

There is no doubting the difficulty of the challenges: these robots are all complex, highly non-linear and hybrid (smooth flows punctuated by discontinuous changes in the states—some selectable by the controller; some imposed by the physics) dynamical systems, they all have >20-dimensional state spaces (most much larger), and dynamic legged locomotion systems are all statically unstable[3]. An additional symptom of the complexity is the lack of breadth in robotics research (with many efforts focusing only on mathematics, software/algorithms, or design/hardware[4]); an unfortunate disconnection from the strongly interdependent nature of the challenge.

My Ph.D. research has emphasized a strong integration of three efforts: mathematical foundations of compositions in legged robotics, design of complex robotic systems, and fielding abstract principles on systems in the real world.

Compositions: a scheme for taming and using complexity in autonomous robotics

My research involves the programming of autonomous dynamic behaviors on physical robotic systems. The usage of "programming" naturally draws comparisons to computer science, the parallels to which are on one hand fraught—the physics of energy-exchange precludes success factors such as "re-use of library elements" [4] we are familiar with in computer engineering, and the physical world that our machines interact with is unpredictable and imprecise in a way that electrical signals on PCB's are not, necessitating structural stability against perturbations in states and parameters. On the other hand, the analogy is enticing: steps toward mirroring the ability in computer science of re-using primitives ("modules" or "functions") between robots, or having re-combinations in the same robot, would lead to rapid improvements in the capabilities of our machines.

My Ph.D. work has focused on programming dynamical steady-state locomotion on the horizontal plane, but using a method that generalizes across different morphologies (such as a hopping monoped, tailed biped, and a quadruped), at varying speeds of up to 5 body lengths/second, while maintaining formally-verifiable stability properties. This method manages the complexity of synthesis by pursuing the following paradigm of behavior programming:

1) **Task specification.** Tasks are a *parallel* composition of target dynamics; e.g., a bounding quadruped should control the horizontal components of its center-of-mass velocity to a desired setpoint, while ensuring its body roll angle is stabilized to zero, and its body pitch angle oscillates in synchrony with front and hind steps.
2) **Library of primitives/controllers.** We develop control strategies for *isolated template plants* (usually 2-dimensional) that need few parameters and up to two states as input: e.g., a vertically-constrained mass on a springy, actuated, massless leg can be controlled with actively tuned stiffness, damping, or shank force profiles.
3) **Composition of templates.** Controllers on the coupled high-dimensional body attempt to simultaneously instantiate the template behaviors while ignoring coupling interactions, thus still having only sparse dependence on states and parameters.

This implementation idea itself is not new; Raibert's machines were controlled with an intuitive composition of control strategies that stabilize each degree-of-freedom (DoF) in isolation: his planar hopper controlled its hopping

[1] Google's AlphaGo Defeats Chinese Go Master in Win for A.I. [NYTimes]
[2] The Hard Lessons of DARPA's Robotics Challenge [IEEE], The DARPA Robotics Challenge Was A Bust [PopSci]
[3] Even though quasi-static stability can be attained by keeping the center of gravity within the base of support (e.g. foot), this limits the machine to low speeds and renders it incapable of navigating rougher terrain where footholds cannot be chosen precisely.
[4] Surprisingly, including in the afore-mentioned DRC, where many teams participated on a "simulation track."

height and fore-aft speed with decoupled controllers [3]. However, Raibert's robots were plugged into the wall and had unlimited available power (untenable for aspiring mobile robots), very specific (advantageous) morphologies, and his work left open many unanswered questions: why were these compositions stable? Why did the apex hopping height have a steady-state error when the fore-aft speed increased? Most importantly, when do these compositions fail to work—and how can they be repaired, and generalized to general robotic morphologies?

My research has introduced and extended tools from applied dynamical systems theory—including hybrid dynamical averaging [5] and the averaging of almost-reversible systems [6]—in order to formalize these questions and begin to answer some of them. We have shown for the first time (notwithstanding Raibert's empirical demonstrations) when and why the presence of mechanical coupling in Raibert's planar hopper does not destabilize the controllers developed in isolation, why his quadruped coordinated its legs to bound without any coordination control, the relation of morphology to this coordination [7], and that similar hopping controllers could stabilize a tailed hopper [8].

This architecture—which aims to create modules (templates) that are externally robust, with carefully-defined interfaces built upon formal guarantees [6] to address fragility to interconnection errors—bears notable resemblance to the notion of complexity in [9], popularized by Doyle's "bow-tie" [10] metaphor.

For the control designer, the benefits of this paradigm include the following:

1) **Guarantees of stability** (for hybrid systems with > 5 dimensions, all but impossible without new analytical tools and not seen thus far in the literature) of control strategies with few parameters, simple functional forms, and a great deal of empirical robustness. Depending on the approach, the complexity of traditional control synthesis can grow unmanageably with system size (the "curse of dimensionality"). Some approaches to mitigate this have included the template-anchor framework [11], (hybrid) zero dynamics [12], but there has been no prescription for successfully using low-dimensional controllers (that don't require knowledge of all states and/or parameters).
2) Since target behaviors are generated through arbitrary (verifiable) compositions of a known library of templates, there is a **combinatorically growing expressible behavioral complexity** (while still maintaining low synthesis complexity). In consequent exploitation of this new expressive capability, the empirical work I did during my Ph.D. spanned the creation of three entirely new robot platforms (more on design below), and the synthesis of eight gaits on them.
3) Due largely to the diversity (and immaturity) of these platforms, I was motivated to prioritize **generalizability, transferability**, and **robustness to plant modeling errors** (since we had had little time to characterize and calibrate every aspect of these new robots—and in real world applications their dynamical and kinematic parameters will be changing dramatically from task to task so it is impractical to imagine fielding such precisely calibrated controllers). Optimization has recently surged in importance in the literature—with the advent of new tools [13] and the simultaneous boom in computing power—resulting in almost-universal preference among competitors in the recent DRC (e.g. [14]). However, the more "optimal" controllers are, the less robust they seem to be to modeling and perception errors [15], including those introduced to generalize and transfer control strategies.
4) As we come to rely more heavily on robotic machines, it is concerning that the failure modes of monolithic optimal or learned controllers offer few clues to the designer about how to repair them. Conversely, the "behaviorally granular" composition construction I suggest reveals **debug-able and potentially swappable "modules."**

Encouraged though we are with our progress on dynamic locomotion so far, we are constantly aware of how far the robots lag the animals that helped inspire their designs. At the interdisciplinary meetings Dynamic Walking (2014, 2017), AMAM (2015), and the Neuromechanical Control Workshop (2017), I've had the opportunity to convey my puzzlement about this topic: our actuators have higher power and force density (when appropriately geared) than muscle does (modulo scaling and packaging), we can make materials that are lighter and stronger, our robots don't need to perform nearly as many functions, but they still lag in sheer performance (jumping height, running speed, etc.). Even more shockingly, our robots can poll sensors and change actuator signals several thousand times per second, whereas humans cannot react to new stimuli faster than 7-8 times per second. A large library of known primitives (manifesting perhaps as synergies [16]), together with the ability to re-use and re-combine (the secrets of which we are starting to uncover with this research agenda), surely contributes largely to the success of the animals.

Moving beyond hopping, running, and walking

At the same time as working on these foundations of stable locomotion, my Ph.D. work begins to look beyond steady cyclic orbits, since our robots will often need to perform maneuvers to transition to entirely different tasks (generically, move between two attracting basins). We know much less about structural stability in this realm, but the notion of "sequential composition" offers an elegant expression of task switching [17] that is synergistic with the afore-mentioned parallel notion.

The brand-new tailed bipedal robot Jerboa [18], with its unique doubly-actuated tail, offered a rich crop of interesting new control problems, such as how to stabilize the tail angle as well the roll angle during freefall [19], and also how to leap off the ground, or simply move the body center atop a ledge or across a gap [20]. In the latter project, we leveraged previous work [21] on a decomposition of the *configuration space* allowing for a reduction of the combinatorial complexity inherent to making and contacts with the environment.

Lastly, embracing an embodied reactive version of the sense/think/act paradigm, I have published findings on the utility of both actuating our sensors [22], [23], as well as sensing using our actuators (discussed next), as we progress toward a future of rich physical interaction between our machines and their environments.

Steps towards making a real impact on the world

My Ph.D. advisor played an excellent role in ensuring that empirical validation accompanied each conceptual advance made by students. In the first few years after starting my Ph.D., I played the role of a "consumer" of the X-RHex platform [24] for empirical validation of sensorimotor control ideas. Soon it became clear that the mechanical intelligence of that platform "hid" some of the challenges in locomotion from the user: it is quite capable of running over moderately rough terrain, but neither affords the designer high-fidelity control of the forces it exerts on the world, nor is it able to report back the forces the world exerts on it. It was designed to be driven "open-loop" [25].

However, I was interested in exploring the challenges of active balance as well as the options afforded by more precise control of interaction forces. This resulted in a multi-year deep-dive (with collaborator Gavin Kenneally) into the architecture and construction of legged systems, to try and identify what was "missing:" commercial off-the-shelf motor controllers with sufficient power, force-control bandwidth, and ease-of-adoption were not available; the motors from "reputable" motor manufacturers such as Maxon were not, in fact, the best ones available based on physically fundamental limits; conventional geared series-elastic transmissions needlessly sacrificed control bandwidth. Based on improvements in these and several other areas (including custom design of every component except the motors), we implemented a revised design paradigm following which a family of direct-drive robots was created at UPenn [26], the first of which was the tailed bipedal Jerboa [18], and the second of which was the quadruped Minitaur.

These robots proved to be particularly easy to design dynamical gaits for ("program"), beginning from the abstract synthetic principles outlined in the previous section: the first Minitaur prototype went from construction to bounding at 3 body-lengths / second in about one month. It attracted attention from other researchers in the summer of 2015; encouraged by inquiries, Gavin and I co-founded the Ghost Robotics company later that year, and officially began distributing these robots to other Universities/corporations in the summer of 2016. While the initial motivation was to enable others' research to move at a pace commensurate with our findings, I have recently been greatly motivated by the possibility of making impact in the world outside academia. We traveled to the AUVSI 2017 exposition with a demonstration and discovered that many industries would find a legged robot such as Minitaur extremely beneficial to their workflow. "Legged robots are the future of ground robots," the CEO of Endeavor (one of the largest UGV manufacturers in the world) told us in a private correspondence following an introduction at AUVSI. We don't take this challenge lightly, and are excited by the challenge of producing a robotic system that fulfils and surpasses expectations of customers not used to the immaturity of embodied robots. I intend to help the company with initial technical development, hiring, and growth as I conclude my Ph.D., and eventually move myself into an advisory role. Real-world problems uncovered by industry are essential to motivate and guide academic research efforts, and they certainly will mine.

Lastly, an academic's role includes not just creation but also dissemination of knowledge, and I have made certain efforts toward the latter by contributing curricular material to two online robotics courses: a mobility course, where I created material on how we take inspiration from animals in the creation of robots and controlling them using compositions, as well as a robotics capstone, where I present a set of exercises to guide students through the process of learning about feedback control and creating a mobile inverted pendulum robot. These courses have been taken by over 50,000 students.

Toward a new era of work-programmable complex robots

Though multi-functional robot hardware has been created, the complexity in its functionality has been restrained by a lack of algorithms that appropriately embrace and manage interconnections. With ideas of verifiable behavioral modularity and a firm understanding of the hardware tools required to implement them, we are closer to identifying the components required to flexibly program the exchange of work between machines and their environment. Knowing how to combine and sequence stable basins to solve arbitrarily complex tasks will result in improved foundations for robotics as it goes from ad-hoc practice to science (with predictive theories) in the next few decades.

Description of Planned Postdoc (word count: 920)

During my Ph.D., as I made accelerating progress toward my own goal of creating dynamically stable gaits for a variety of robot platforms, I began to appreciate a bigger picture: a formalization of parallel composition is a pivotally important step for the future of robotic legged locomotion. I would like to use my postdoctoral study as an opportunity to identify similarly important and fundamental questions in other areas (hopefully making similarly meaningful contributions), as well as learn about tools favored by scientists in other domains. Additionally, I am hugely cognizant of the challenge of scientific communication, and the tendency of cluster-formation in citation (and readership) networks, further motivating a move into a distinct academic area.

I intend to pursue a future career in academia, with the hope of a greatly strengthened application after the broadening experience during my postdoc. My expectation of the outcome of a postdoc such as the one planned is that existing and new collaborations in several areas (see next section for examples) will benefit from

1) foundational principles of a computer-science-inspired programmatic analogy to synthesis using composition,
2) formal guarantees of structural stability against perturbation in states and parameters,
3) simultaneous emphasis on empirical validation and deployment of formal ideas, and
4) a method of specification that packages these into a formal language for programming energy-exchange.

Specific areas of interest, and relation to core research

My Ph.D. research enabled me to get greatly familiar with (hybrid) dynamical systems, perturbation methods for nonlinear systems, formalization of symmetry/reversibility, dynamical dimension reduction, hardware, platform, and system design; these tools prepare me well to be able to contribute quickly to each of the following areas of interest:

1) **Manipulation.** The long-standing tradition in robotic manipulation is to consider "primitives" as open-loop trajectories which do not include feedback control (my approach to locomotion). I believe that two reasons for this are that (a) the hybrid system in grasping is much more complex than in locomotion (number of modes explodes), and (b) available technology limits the utility of force-sensory information about the manipulant. We are now approaching a position where we may bring the robustness of feedback control to grasping, as indicated by recent work by Rodriguez (MIT) and Mason (CMU). My work on dynamical reductions (using averaging and anchor-template relationships) as well as on high-transparency robot hardware could be brought to bear directly on this task.
2) **Control and design of prostheses and exoskeleta.** My work on feedback control signals (which react well to perturbations), phase synchronization, and high-transparency, high bandwidth robot design offers a fresh perspective on both the construction and control of systems for rehabilitation that include a human in the loop.
3) **Learning.** I have been following the recent progress in machine learning, especially in the lead-up to and aftermath of a recent visit (as a Ghost Robotics representative) to Google's Brain team in Mountain View:
   a) Recent results in hierarchical learning from DeepMind [27] demonstrate the dimension-reduction utility of compositions.
   b) As tasks or morphologies increase in complexity beyond steady locomotion in the horizontal plane, it is likely that identifying a low-dimensional "task component" will be difficult; I am interested in the prospect of using increasingly accomplished function approximation tools to identify the relevant dimensions as well as strategies to control them from data. Biomechanists posit that muscle synergies (patterns of co-activation) are used to simplify coordination of DoF's in animals [16], and some roboticists have attempted to identify them from analysis of animal data [28], [29].
4) **Optimization.** My doctoral work takes steps toward "automating" the control synthesis process for legged robotics. The usage of rapidly-improving optimization tools in some of the work of Tedrake et. al. [30], [31] which provide guarantees at *design-time* (versus *a posteriori* verification as I have pursued in my own work), synergizes with my recent progress on symmetry ideas suggesting "constraints" on the feedback control signal. As in highly-optimized tolerance (HOT) [9], this would still be a modularity-committed scheme, carefully optimizing the interfaces for external robustness and intuitively strengthening the neck of the bow-tie [10].
5) **Biomechanics.**
   a) Max Donelan is at the early stages of a project to answer the question of why robots are worse than animals (cf. research statement), and collaboration with him or others would be an enticing prospect to better articulate the need for primitives and compositions.
   b) My interest in novel morphologies led to the creation of the Jerboa [18], but the design of that robot precludes performance commensurate to a kangaroo rat (Jerboa's inspiration) for a few reasons: running animals can change the impedance characteristics offered by muscle, generally stiffening as running speed increases [32], which the Jerboa can't do due to its fixed springs. Further, the cost-of-transport (CoT) of a kangaroo

(uniquely) goes *down* with speed [33], leading to incredibly efficient high-speed hopping. This feat would be impossible on the robotic designs we have created so far (including Jerboa and Minitaur), motivating collaboration with biomechanists on inspiration for the design of more efficient hopping/running robots.

c) It is conceivable that any contributions made while identifying primitives for robots (cf. “learning” above), such as assays for identifying compositional control strategies, will have overlap with the interests of biomechanists.

6) **Perception and reactive planning.** My work so far has focused on “blind” locomotion; I am interested in identifying new tasks where real-time perception, and a consequent sensorimotor loop, is required for task completion (e.g., a “stepping stones” task), perhaps tying back to my own active sensing work [22].

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