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Paper ID	824
Paper authors	John Oberlin, Stefanie Tellex
Paper title	Bandit-Based Adaptation for Robotic Grasping
Paper subtitle	
Track	()
Paper Type	Long Paper (7 pages)
Keywords	Robotics and Vision::Manipulation ** Robotics and Vision::Vision and Perception ** Machine Learning::Reinforcement Learning
Abstract	A key aim of current research is to create robots that can reliably manipulate objects. However, in many instances, information needed to infer a successful grasp is latent in the environment and arises from complicated physical dynamics; for example, a heavy object might need to be grasped in the middle or else it will twist out of the robot's gripper. The contribution of this paper is a formalization of object picking as an N-armed bandit problem, where each potential grasp point corresponds to an arm with unknown pick success rate. This formalization enables us to apply bandit-based exploration algorithms to enable a robot to identify the best arm by attempting to pick up the object and tracking its successes and failures. Because the number of grasp points is very large, we define a new algorithm for best arm identification in budgeted bandits that computes confidence bounds while incorporating prior information, enabling the robot to quickly find a near optimal arm without pulling all the arms as in UCB-based approaches. We demonstrate that our adaptation step significantly improves accuracy over a non-adaptive system, enabling a robot to improve grasping models through experience.

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This paper introduces a formalization of the object picking problem in robotics as an N-armed bandit problem, where each potential grasp point of an object corresponds to an arm with unknown pick success rate. In the problems tackled in this work, each grasp takes more than 90 seconds and there are more than 1000 potential arms or grasp points. Therefore, try every possibility several times is unfeasible. Instead, the authors propose to provide the system with prior knowledge that can be adjusted online.

The first step for object grasping is object detection and localization. The authors follow an approach where each object is downsampled to a 21 times 21 grid at 1cm resolution, and they define 4 different gripper orientations, so every object can be gripped in 1764 different ways. The prior knowledge provided to the system is given as a map from x, y and orientation to a normalized value of success.

From the N-armed bandit point of view, the agent's goal is to identify a good arm (grasping point with probability of success larger than k) with probability c. Such value is estimated as the probability that the true payout probability is larger than the threshold c, given the number of successes and failures. Its Cumulative Distribution Function can be computed, and they provide the algorithm that computes it online. The algorithm is not described by steps, so I suggest to incorporate a detailed description. For instance, the algorithm uses a parameter n, that I understand that is the number of arms. But that is not said explicitly anywhere.

I also think the authors should make a larger effort to explain if the method generalizes among the different instances of objects, and if so, how do they do. They clarify that point for the priors at the end of section 2. But nothing is said about the learned probabilities. For instance, the p_{below}, p_{above} and p{threshold} learned for an arm j in Algorithm 1, for what object instance is being learned? Do you have to learn one for each object? From the evaluation, I can deduce that there is not generalization. But that point should be clarified. Overall, because a real improvement should be the capability of the algorithm to generalize to new objects.

The evaluation, specifically table 1, is focused on measuring the increment of success from the prior knowledge to that obtained after learning. But the authors does not define in the robotic evaluation how they define the prior knowledge. They do for simulation, although they do not evaluate the effect of such prior in the result. For instance, if the prior is very optimistic, the learning process could be very different than if the prior is pessimistic? For instance, in Reinforcement Learning, an e-greedy exploration strategy produces very different learning processes depending on how the value function is initialized. Does it happen in this case? If so, how difficult is to find a good prior to initialize the algorithm? What is the worst case?

In addition, some other parameters are not evaluated. For instance, the algorithm tries to find better arms until the increment in performance is above a threshold... How can we define what is the right threshold? This is important, because a low threshold will produce a brief exploration, while a large one could prevent the algorithm to find a solution.

Summary of review
Interesting paper, but with some important issues that should be analyzed.

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The paper describes a method for learning the grasp points of unknown objects through the adaptation of a n-bandit algorithm. The robot initializes the value of the points from a prior, and then tries different grasp points until it's sufficiently confident on their reliability, or a maximum number of trials has been reached.

The idea is simple, which is good. Grasping, however, is a very active research topic, and the authors seem to be unaware of a fair amount of literature on the subject. This makes this paper look a bit out of context, and I find it difficult to compare it with others.
Evaluating a grasping algorithm is not an easy task, as there are no benchmarks at the moment. Therefore, each new method is tested on an arbitrary number and type of objects, and defines an accuracy measurement of its own. Nonetheless, it is untrue that, as the authors mention in the introduction, "Systems for general-purpose manipulation are computationally expensive and do not enjoy high accuracy on novel objects". These are a few works that actually achieve quite a good accuracy on novel objects:

Grasping novel objects with depth segmentation, IROS 2010, 87.5%
A hybrid approach for grasping 3D objects, IROS 2009, 85%
Robotic grasping of unmodeled objects using time-of-flight range data and finger torque information, IROS 2010, 81%
Learning to grasp objects with multiple contact points, ICRA 2010, 81.87%
Three finger precision grasp on incomplete 3D point clouds, ICRA 2014, 85%

While it is not possible to compare these value directly, it is also not correct to say that there are no fast methods that work well on unknown objects. Furthermore, in Combining active learning and reactive control for robot grasping, RAS 2010, The authors also use the success rate of evaluated grasps to determine future grasps, and in particular they model the problem as a continuum-armed bandit problem. Overall, the discussion of the position of this paper in the literature has to be more thorough.

In the related work section, the authors cite the survey paper from Bogh et al., which asserts: "Following Shimoga [2], analytic refers to methods that construct force-closure grasps with a multi-fingered robotic hand that are dexterous, in equilibrium, stable and exhibit a certain dynamic behaviour.". Then, the authors claim that "our system initially uses analytic approaches to generate a grasp hypothesis space which allows it to pick previously encountered objects". It doesn't seem to me that the authors use analytic approaches at all. They do not evaluate grasps in terms of force-closure or equilibrium properties. They only provide a prior based on 2D filter responses.

About the applicability of this method, the authors specify that if the object is knocked over by the robot, it has to be placed again in the reference pose. Is this also necessary in test? This would make it not suitable for general-purpose grasping. I understand the authors intend to addrees this limitation, but until that happens we can't know if a robot could actually employ this method in practice. Furthermore, between building the IR scans and trying different picks, sometimes tens of them, this method must be fairly slow. In my personal opinion, this could be a good way of selecting between a few candidate grasps, possibly generated by (actual) analytical methods, but this general formulation including only a prior on all possible grasp points is too data intensive to be practical. I may be wrong, but a comparison with other learning algorithms for grasping is necessary to assess the applicability of the proposed method.

In terms of clarity and readability, most of the paper reads well. I would improve Section 3, which relies on the reader to know quite a bit about n-armed bandits. Also, I think c has been used for k from the paragraph explaining Equation 1, which creates some confusion.

Minor comment: The "Object detection and localization" section is really vague. Instead of saying that "conventional computer vision algorithms" are used the authors should try at least to mention the names of some of the algorithms they use, and make their pipeline reproducible.

All in all, I think this paper establishes that the proposed algorithm performs better than Thomson sampling at solving the bandit problem the authors formulate, but does not establish the whole method as a viable grasp option compared to other grasping methods.

Summary of review
The paper describes a method for learning the grasp points of unknown objects through the adaptation of a n-bandit algorithm. The algorithm seems to perform well at the formulated bandit problem, but it is not clear whether this formulation is a practical way of tackling grasping, and how this compares with the rest of the literature.

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Comments to author(s)
The authors describe an approach for grasp selection based on a bandit approach. There are several points that require expansion or clarity in order to assess the novel properties of this work.

The prior plays a significant role in the success of your approach. Without it the bandit based formulation makes very little sense given the significant cost of sampling arms. The paper can be strengthened by discussing the intuitive properties of the prior so as to provide some insight into its function. Construction of high quality priors for grasping could be its own contribution, but currently discussion of the prior is relegated to a single paragraph.

It would also strengthen the paper by contrasting your algorithm clearly with the work by Kaufman et. al (AISTATS 2012: On Bayesian Upper Confidence Bounds for Bandit Problems). This paper similarly introduces prior information into the algorithmic process of arm sampling. The also provide proofs of convergence and comparison to typical frequentist bandit approaches.

Summary of review
While this is an interesting application of bandits to an usual problem space there needs to be further clarification that the algorithm represents a novel contribution.

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