Offline Reinforcement Learning on Real Robot with Realistic Data Sources

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Abstract

Offline reinforcement learning (ORL) holds great promise for robot learning due to its ability to learn from arbitrary pre-generated experience. However, current ORL benchmarks are almost entirely in simulation and utilize contrived datasets like replay buffers of online RL agents or sub-optimal trajectories, and thus hold limited relevence for real-world robotics. In this work, we posit that near-optimal and safe demonstrations collected from out-of-domain tasks are more practical data-sources for real world robot learning. We perform an extensive (800+ hours data, 270+ human hours evaluations, multiple seeds) empirical study of representative ORL methods on four real world tabletop manipulation tasks. Our study finds that ORL and imitation learning prefer different action spaces, and that ORL algorithms can generalize from leveraging offline heterogeneous data sources and outperform imitation learning. Project URL: `https://sites.google.com/view/real-orl-anon`

1 Introduction

Despite rapid advances, the applicability of Deep Reinforcement Learning (DRL) algorithms [1–8] to real-world robotics tasks is limited due to sample inefficiency and safety considerations. The field of offline reinforcement learning (ORL) [9, 10] has the potential to overcome these challenges, by learning only from pre-generated offline datasets, thereby circumventing safety and exploration challenges. This makes ORL well suited for applications with large datasets or those where online interactions are scarce and expensive (e.g. robotics). However, comprehensive benchmarking and empirical evaluation of ORL algorithms is significantly lagging behind the burst of algorithmic progress [11–21]. Widely used ORL benchmarks [22, 23] are entirely in simulation and use contrived data collection protocols that do not capture fundamental considerations of physical robots. Our work aims to bridge this gap by outlining practical offline dataset collection protocols that are representative of real-world robot settings. We perform a comprehensive empirical study spanning **6500+ robot trajectories** and **270+ human labor hours**, to benchmark and analyze three representative ORL algorithms thoroughly. We will release all the datasets, code, and hardware hooks from this paper.

While widely used simulated ORL benchmarks [22–24] utilize data sources like expert policies trained with *online* RL, exploratory policies, or even replay buffers of *online* RL agents, those data sources may fail to capture the challenges in real world: hardware noises coupled with varying reset conditions lead to covariate shift and violate the i.i.d. assumption about state distributions between train and test time. Further, such datasets are not feasible on physical robots and defeat the core motivation of avoiding the poor sample efficiency and safety of *online* RL! Recent works [24, 25] suggest that dataset distribution dramatically affect the relative performance of algorithms. Therefore,

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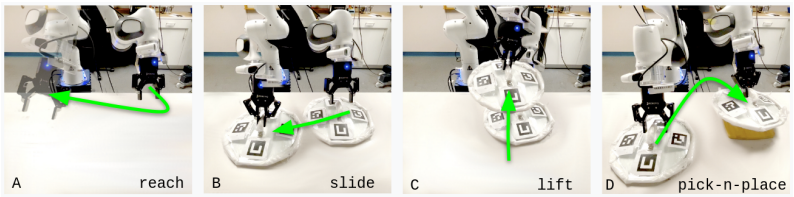

Figure 1: Canonical tasks for tabletop manipulation.

we consider the pertinent question: *What is a practical instantiation of the ORL setting for physical robots, and can existing ORL algorithms learn successful policies in such a setting?*

In this work, we envision *practical* scenarios to apply ORL for real-world robotics. Our first insight is that real-world datasets are likely to come from well-behaved policies that abide by safety and monetary constraints. Such trajectories can be collected by user demonstrations or through hand-scripted policies that are safe. Secondly, the goal of any learning (including ORL) is broad generalization and transfer. It is therefore critical to study whether a learning algorithm can leverage task-agnostic datasets to make progress on a new target task. In this work, we collect offline datasets consistent with these principles and evaluate representative ORL algorithms on a set of canonical table-top tasks as illustrated in Figure 1.

Evaluation studies on physical robots are sparse in the field due to time and resource constraints, but they are vital to furturing our understanding. Our real robot results corroborate and validate intuitions from simulated benchmarks [26] but also enable novel discoveries. We find that (1) even for scenarios with sufficiently high-quality data, some ORL algorithms could outperform behavior cloning (BC) [27] on specific tasks, (2) for scenarios that require generalization or transfer to new tasks with low data support, ORL agents generally outperform BC. (3) in cases with overlapping data support, ORL algorithms can leverage additional heterogeneous task-agnostic data to improve their own performance, and in some cases even surpass the best in-domain agent.

Our empirical evaluation is unique as it focuses on ORL algorithms' ability to leverage realistic and multi-task data sources, spans over several algorithm-agnostic tasks, trains various ORL algorithms and evaluates them directly in the real world. In summary, we believe our work establishes the effectiveness of offline RL algorithms in leveraging out of domain high-quality heterogeneous data for generalization and transfer in robot-learning, which is representative of real world applications.

2 Preliminaries and Related Work

We use the Markov Decision Process (MDP) framework for ORL detailed in Appendix. 6.1. While there exist abundant evaluations and benchmarks for ORL algorithms in simulation [22–24], they predominantly use datasets with idealistic coverage and i.i.d. samples which are not realistic assumptions in the real world. Prior works investigating offline RL for these settings on physical robots are limited: Kostrikov et al. [18] did not provide real robot evaluation for IQL, which we conduct in this work; Chebotar et al. [28], Kalashnikov et al. [29] evaluate performance on a specialized Arm-Farm; Rafailov et al. [30] evaluate on a single drawer closing task; Singh et al. [17], Kumar et al. [31] evaluate only one algorithm (COG, CQL, respectively). Mandlekar et al. [32] evaluate BCQ and CQL alongside BC on three real robotics tasks, but their evaluations consider only in-domain setting: that the agents were trained only on the specific task data, without giving them access to a pre-generated, offline dataset. Thus, it is unclear whether insights from simulated benchmarks or limited hardware evaluation can generalize broadly.

In this work, we evaluate three representative ORL algorithms: Advantage-Weighted Actor Critic (AWAC) [19], Implicit Q-Learning (IQL) [18], and Model-based Offline REinforcement Learning (MOREL) [12], along with Behavior Cloning (BC) [27]. BC is a model-free IL algorithm that remains a strong baseline for real robot tasks due to its practicality. AWAC and IQL both train an off-policy value function and then derive a policy to maximize the expected reward. AWAC uses KL divergence minimization to constrain the policy to be close to the given policy distribution.

IQL leverages expectile regression to avoid querying the value function for out-of-distribution queries. MOREL is a model-based approach: it recovers a dynamics model that allows it to apply policy gradient RL algorithms. More detailed related work can be found in Appendix 6.1.

3 Experiment Design

We aim to answer the following questions. **(1)** Are ORL algorithms sensitive to any specific state and action space parameterization? **(2)** How do they perform against imitation learning (IL) approaches? **(3)** How do they perform in out-of-domain tasks requiring (a) generalization, and (b) re-targeting? We outline our choice of candidate algorithms and attach our performance metrics in Appendix. 6.5.

Algorithms We compare four algorithms introduced in 2: Behavior Cloning (BC) [27], Model-based Offline REinforcement Learning (MOREL) [12], Advantage-Weighted Actor Critic (AWAC) [19] and Implicit Q-Learning (IQL) [18].

Tasks and Dataset. We consider four classic manipulation tasks common in literature: `reach`, `slide`, `lift`, and `pick-n-place (PnP)` (see Figure 1) and designed reward functions for each (Table. 4in Appendix. 6.2). In total, we collected a dataset of around 3000 trajectories with characteristics shown in Appendix.6.3. Our offline dataset is represented as a series of transition tuples $\{(s, a, s')_{task}\}$. States consist of joint positions, joint velocities, and positions of the object to interact with (if applicable). Actions contain target joint positions. More details are available in Appendix 6.3.

A. In-domain Ablations. We first train all agents using in-domain data (only data that were collected for the test task) to test ORL algorithms' sensitivity to varying data representation. (Ablation results are in Appendix. 6.7.) We use the outcome of the ablations and the best-performing setting for each algorithm to study generalization and transfer in the following three scenarios:

B. Generalization: Lacking data support. The data collection may not cover the task space uniformly. Can ORL algorithms generalize to a task space that lacks data support? We create a new dataset from our slide task by dividing the task space to three regions: left, center, right. We remove trajectories where the object was initially placed in the center region. We train and evaluate all agents by asking them to slide an object initially placed in each region.

C. Generalization: Re-targeting data for dynamic tasks. For the slide task, our collected demonstration has static goal positions. We test agents trained using such static data in a dynamic setting by updating the goal at a fixed frequency, and asking the agents to grasp and slide the lid following some predetermined curves. This task can be viewed as a simplified version of daily tasks like drawing and cleaning, which require possibly repeated actions and a much longer horizon than usual IL and ORL tasks. We collected the ideal trajectories via human demonstration.

D. Transfer: Reusing data from different tasks. We investigate whether we can reuse hetergenuous data collected from previous tasks to train a policy for a new task. ORL algorithms can use the reward function for the test task to relabel datasets for other tasks and possibly improve performance. We train our algorithm with different combinations of canonical task demonstrations ("train-data") and evaluate each agent on each individual task.

4 Results and Discussion

Generalization to regions that lack data support. Table 3 trains agent using a carved-out dataset and compares the agents' performance on regions with more data support versus the region with less data support (`Center`). We also train all agents using the full dataset and evaluate them on the `Center` region. We discovered that: (1) on the region with abundant support (`Left` and `Right`), BC/IQL performed better than AWAC/MOREL , aligning with our previous observation, (2) on regions that have less data support, AWAC and MOREL could match BC's performance despite their initial disadvantage; and (3) ORL agents trained with carved-out dataset and evaluated on carved-out region performed no worse than them trained with full dataset, in contrast to BC agent, which performed significantly worse after carving-out.

Generalization to dynamic tasks. Table 2 lists the ideal curves and the curves traced by each model. BC had the worst performance among all models since it failed to complete tracing of the circle, square, and number 8 which requires larger ranges of motion. Meanwhile, ORL methods largely succeeded tracing the entire curve, demonstrating stronger generalizing ability.

Agent	Train Data	Test Task `slide`	`lift`	`PnP`
BC	in-domain	.68 ± .15	.82 ± .18	.82 ± .19
	slide	.68 ± .15	.58 ± .06	.61 ± .08
	slide+lift	.60 ± .13	.58 ± .05	.61 ± .12
	slide+lift+PnP	.61 ± .14	.61 ± .08	.64 ± .14
MOREL	in-domain	.63 ± .16	.68 ± .19	.75 ± .20
	slide	.63 ± .16	.61 ± .06	.74 ± .17
	slide+lift	.62 ± .15	.73 ± .18	.64 ± .17
	slide+lift+PnP	.72 ± .13	**.90 ± .13**	.75 ± .18
AWAC	in-domain	.73 ± .11	.86 ± .15 *	.74 ± .18 *
	slide	.73 ± .11	.64 ± .06	.77 ± .11*
	slide+lift	.73 ± .11 *	.90 ± .15	.81 ± .12
	slide+lift+PnP	.64 ± .14 *	.73 ± .20 *	.76 ± .19 *
IQL	in-domain	**.77 ± .07**	.88 ± .12	.60 ± .23
	slide	.76 ± .10	.26 ± .03	.81 ± .11
	slide+lift	.70 ± .14	.86 ± .17	**.84 ± .11**
	slide+lift+PnP	.64 ± .14	.68 ± .16	.83 ± .18

Table 1: Training agents trained with different combinations of offline data.

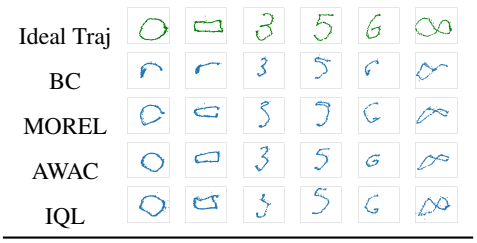

Table 2: Trajectory tracking results of each agent. Each dot represents the location of the lid at a time step.

Start	BC	MOREL	AWAC	IQL
`Left`	.79 ± .06	.57 ± .06	.70 ± .12	.70 ± .07
`Right`	.77 ± .02	.80 ± .03	.71 ± .14	.81 ± .02
`Center`	.76 ± .01	.79 ± .02	.83 ± .03	.81 ± .01
`Center (FULL)`	.79 ± .02	.78 ± .02	.81 ± .02	.81 ± .05

Table 3: Training agents using a carved-out dataset.

Transfer learning by leveraging heterogeneous dataset Table 1 evaluates the performance of ORL agents when trained with combinations of datasets. The best in-domain agent, transfer learning agents that improve over their in-domain counterparts are colored. The **best agent for each task** is bolded. Agents that could not converge during training are marked with (*). Some agents triggered violent crashes on robot (<0). Underline scores are swept over 3 seeds. We observe that:

1. The performance of ORL agents after leveraging data from different tasks can vary, due to the characteristics of the algorithm, the task, the design of the reward function, and the data distribution.
2. We observed all ORL agents *could* improve their own performance using some task/data combinations. Noticeably, MOREL achieved higher or comparable performance on all tasks after leveraging more offline data. Intuitively, MOREL's dynamic model training process could benefit from any realistic data, regardless of whether the data was in-domain or out-of-domain.
3. Certain task-agnostic data could provide overlapping data support and enable effective transfer learning, allowing some ORL agents to surpass imitation learning and even the best in-domain agents. On `slide` and `lift`, all ORL algorithms managed to surpass BC.
4. ORL algorithms are not guaranteed to increase performance by including more data. The performance changes of ORL are likely to vary by agents, the task and dataset distribution.

5 Conclusion

We conducted an empirical study of representative ORL algorithms on real-world robotic learning tasks.We find that (1) even for tasks with abundant amount of high-quality data, IQL can be a competitive baseline against BC, (2) ORL algorithms generalize better to regions with low data-support and to dynamic tasks, (3) the performance changes of ORL after leveraging heterogeneous data are likely to vary by agents, the design of the task, and the characteristics of the data, (4) heterogeneous task-agnostic data could provide data support and enable transfer learning, allowing ORL agents to improve their performance. Our evaluations indicate that even in out-of-domain multi-task data regime, offline RL is an effective paradigm to leverage out-of-domain data.

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6 Appendix

6.1 Related Work

Preliminaries. We model the environment as a Markov Decision Process (MDP): $M = \langle S, A, R, T, \rho_0, H \rangle$ where $S \subseteq \mathbb{R}^n$ is the state space, $A \subseteq \mathbb{R}^m$ is the action space, $R : S \times A \to \mathbb{R}$ is the reward function, $T : S \times A \times S \to \mathbb{R}_+$ is the (stochastic) transition dynamics, $\rho_0 : S \to \mathbb{R}_+$ is the initial state distribution, and H is the maximum trajectory horizon. In the ORL setting, we assume access to the reward function R and a pre-generated dataset of the form: $\mathbb{D} = \{\tau_1, \tau_2, \ldots \tau_N\}$, where each $\tau_i = (s_0, a_0, s_1, a_1, \ldots s_H)$ is a trajectory collected using a behavioral policy or a mix of policies $\pi_b : S \times A \to \mathbb{R}_+$.

The goal in ORL is to learn a near-optimal policy,

$$\pi^* := \arg\max_{\pi} \; \mathbb{E}_{M,\pi} \left[\sum_{t=0}^{H} r(s_t, a_t) \right].$$

In the general case, the optimal policy π^* may not be learnable using $\mathbb{D}$ due to a lack of sufficient exploration in the dataset. In this case, we would seek the best policy learnable from the dataset, or, at the very least, a policy improves upon behavioral policy.

Offline RL Algorithms. Recent years have seen tremendous interests in offline RL and the development of new ORL algorithms. Most of these algorithms incorporate some form of regularization or conservatism. This can take many forms, such as regularized policy gradients or actor critic algorithms [14, 15, 19, 33–35], approximate dynamic programming [11, 13, 17, 18], and model-based RL [12, 36–38]. We select a representative ORL algorithms from each category: AWAC [19], IQL [18] and MOREL [12]. In this work, we do not propose new algorithms for offline RL; rather we study a spectrum of representative ORL algorithms and evaluate their assumptions and effectiveness on a physical robot under realistic usage scenarios.

Offline RL Benchmarks and Evaluation. In conjunction with algorithmic advances, offline RL benchmarks have also been proposed. However, they are predominantly captured with simulation [22, 23, 39] using datasets with idealistic coverage, i.i.d. samples, and synchronous execution. Most of these assumptions are invalid in real world which is stochastic and has operational delays. Prior works investigating offline RL for these settings on physical robots are limited. For instance, Kostrikov et al. [18] did not provide real robot evaluation for IQL, which we conduct in this work; Chebotar et al. [28], Kalashnikov et al. [29] evaluate performance on a specialized Arm-Farm; Rafailov et al. [30] evaluate on a single drawer closing task; Singh et al. [17], Kumar et al. [31] evaluate only one algorithm (COG, CQL, respectively). Mandlekar et al. [32] evaluate BCQ and CQL alongside BC on three real robotics tasks, but their evaluations consider only in-domain setting: that the agents were trained only on the specific task data, without giving them access to a pre-generated, offline dataset. Thus, it is unclear whether insights from simulated benchmarks or limited hardware evaluation can generalize broadly. Our work aims to bridge this gap by empirically studying representative offline RL algorithms on a suite of real-world robot learning tasks with an emphasize on transfer learning and out-domain generalization. See Section 2 for detailed discussion.

Imitation Learning (IL). IL [40] is an alternate approach to training control policies for robotics. Unlike RL, which learns policies by optimizing rewards (or costs), IL (and inverse RL [41–43]) learns by mimicking expert demonstrations and typically requires no reward function. IL has been studied in both the offline setting [44, 45], where the agent learns from a fixed set of expert demonstrations, and the online setting [46, 47], where the agent can perform additional environment interactions. A combination of RL and IL has also been explored in prior work [48, 49]. Our offline dataset consists of trajectories from a heuristic hand-scripted policy collected under expert supervision, which represents a dataset of reasonably *high quality*. As a result, we consider offline IL and, behavior cloning in particular, as a baseline algorithm in our empirical evaluation.

6.2 Canonical Task Setup

To investigate the effectiveness of ORL algorithms on real-world robot learning tasks, we adhere to a few guiding principles: (1) we make design choices representing the wider community to the extent possible, (2) we strive to be fair to all baselines by providing them their best chance and work in consultation with their authors; and (3) we prioritize reproducibility and data sharing. We will open-source our data, camera images along with our training and evaluation codebase.

Hardware Setup. Hardware plays a seminal role in robotic capability. For reproducibility and extensibility, we selected a hardware platform that is well-established, non-custom, and commonly used in the field. After an exhaustive literature survey [50–55], we converged on a table-top manipulation setup, shown in Figure 2. It consists of a table-mounted Franka panda arm that uses a RobotiQ parallel gripper as its end effector, which is accompanied by two Intel 435 RGBD cameras. Our robot has 8 DOF, uses factory-supplied default controller gains, accepts position commands at 15 Hz, and runs a low-level joint position controller at 1000 Hz. To perceive the object to interact with, we exact the position of the AprilTags attached to the object from RGB images. Our robot states consist of joint positions, joint velocities, and positions of the object to interact with (if applicable). Our policies compute actions (desired joint pose) using robot proprioception, tracked object locations, and desired goal location.

Data Collection. We use a hand-designed, scripted policy developed under expert supervision to collect (dominantly) successful trajectories for all our canonical tasks. To highlight ORL algorithms ability to overcome suboptimal dataset, previous works [22, 39, 32] have crippled expert policies with noise, use half-trained RL policies or collect human demonstrations with varying qualities to highlight the performance gain over compromised datasets. We posit that such data sources are not representative of robotics domains, where noisy or random behaviors are unsafe and detrimental to hardware's stability. Instead of infusing noise or failure data points to serve as negative examples, we believe that mixing data collected from various tasks offers a more realistic setting in which to apply ORL on real robots for three reasons: (1) collecting such "random/roaming/explorative" data on a real robot autonomously would require comprehensive safety constraints, expert supervision and oversight, (2) engaging experts to record such random data in large quantities makes less sense than utilizing them to collecting meaningful trajectories on a real task, and (3) designing task-specific strategies and stress testing ORL's ability against such a strong dataset is more viable than using a compromised dataset. We collected offline dataset using heuristic strategies designed with reasonable efforts and, to avoid biases favoring task/algorithm, frozen the dataset ahead of time.

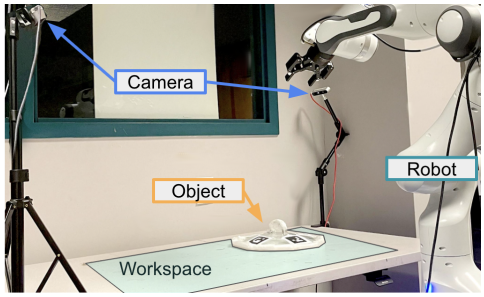


Figure 2: Our setup consists of a commonly used Franka arm, a RobotiQ parallel gripper, and two Intel Realsense 435 cameras.

To create scripted policies for all tasks, we first separate each task into stages with a goal location. We then compute a target robot state in joint angles, where the goal position is reached by the gripper via Mujoco's IK solver. The scripted policy takes tiny steps toward this goal location until some task-specific criteria are reached. Our heuristic policies didn't reach the theoretical maximum possible scores due to controller noises and tracking noises. However, they complete the task at a high success rate and have comparable performance to human demonstration.

Canonical Tasks We consider four classic manipulation tasks common in literature: `reach`, `slide`, `lift`, and `pick-n-place (PnP)` (see Figure 1). `reach` requires the robot to move from a randomly sampled configuration in the workspace to another configuration. The other three tasks involve a heavy glass lid with a handle, which is initialized randomly on the table. `slide` requires the robot to hold and move the lid along the table to a specified goal location. `lift` requires the robot to grasp

and lift the lid 10 cm off the table. PnP requires the robot to grasp, lift, move and place the lid at a designated goal position i.e. the chopping board. The four tasks constitute a representative range of common tabletop manipulation challenges: reach focuses on free movements while the other three tasks involve intermittent interaction dynamics between the table, lid, and the parallel grippers. We model each canonical task as a MDP with an unique reward function. Details on our tasks are in Appendix. 6.2.

We considered four canonical tasks: reach, slide, lift and PnP. To apply ORL, each task can be formulated as an MDP. The state contains the joint position of the robot, the gripper open position ($\mathcal{R} \sim [0, 0.08]$), (optionally) the velocity of the joints, (optionally) the tracked tag position and a goal position. To facilitate RL training, we came up with a continuous reward function for each task $r : state \rightarrow \mathcal{R}$, as shown in Table 4, considering the position of the gripper x, the position of tracked AprilTag t (if exists), the position of goal g, the Euclidean distance function dis between two 3D coordinates, a convenient function $height$ to denote the height of a given coordinates. While the reward for reach and slide are naturally smaller than 1, we explicitly cap the maximum reward for lift to be 1 since we don't encourage agents to lift up the lid arbitrarily high. We don't cap the PnP reward since we encourage the pick-n-place policy to be distinguished from the policy with a height bonus $height(t)$.

We used heuristic policies to collect the demonstration data, as described in Sec. 2. Our policies have a reasonable success rate *accomplishing* the task but is not designed to be optimal in solving the MDP. To evaluate and compare between agents, we instead report the maximum reward over the trajectory as a proxy of the task completion ("score"). We report our heuristic policies' accumulated reward average over trajectories and the score.

Task	$r(s)$	$\sum r(s)$	Score
Reach	$1 - dis(g - x)$	173	0.99
Slide	$1 - (2 * dis(g - t) + dis(t - x))$	223	0.93
Lift	$min(1, 0.57 - dis(t - x) + height(t))$	167	1
Pick-n-place	$1 - (dis(g - t) + 2 * dis(t - x)) * 0.9 + height(t)$	281	1.09

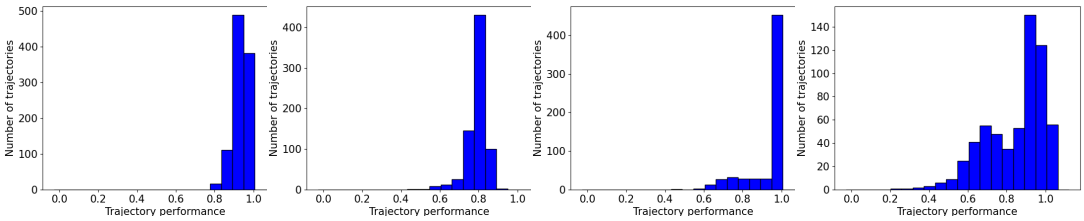
Table 4: Characteristics of task and collected data.

6.3 Dataset

Task	*# Traj*	*# Samples*	*Avg Score*	*Max Score*	*Human Score*	*Theoretical Best Score*
reach	1000	99752	0.960	0.99	0.963	1
slide	731	244422	0.819	0.93	0.834	1
lift	609	178515	0.948	1	1	1
PnP	616	327478	0.875	1.09	0.924	1.15

Table 5: Characteristics of collected data. *# Traj* denotes the total number of trajectories, *# Samples* denotes the total number of state-action-reward pairs. Each trajectory's score is the maximum reward in the trajectory. *Avg Score* shows the average scores per trajectories, *Max Score* shows the maximum reward achieved by trajectories in our dataset, *Human Score* shows the max reward achieved by a human teleoperator and *Theoretical Best Score* denotes the theoretical maximum possible reward determined by our reward function.

We also attach our collected data's score distribution on each task to demonstrate our dataset's overall quality. From Figure 3, we can see that the score distribution for each task skew heavily to the left, which means that most trajectories in our dataset are near-optimal and are suitable for imitation learning. In Appendix. 6.8 we further verify this.



(a) Reaching (b) Sliding (c) Lifting (d) Pick-n-place

Figure 3: Score distribution for each task of our dataset.

6.4 Open Source Code and Dataset

To remain anonymity we have only uploaded our collected dataset to here. Once accepted, we would share code and instructions on how to process and use our dataset.

6.5 Experiment Design

Training. Since neural network agents are empirically sensitive to parameters and seeds, we (1) used the same fixed random seed (123) for all our experiments with additional seed sweeping to strengthen the reproducibility of our results and (2) conducted equal amount of efforts for hyperparameter tuning efforts for all algorithms. Unlike traditional supervised learning, we cannot simply select the agents with the best validation loss for tuning the hyperparameters, because we cannot know the performance of an agent unless testing it on a real robot [32]. We thus keep our tuning simple and fair: starting with the default parameters and training 5 agents in 3 rounds, trying to make the agent converge. We observe that certain agents cannot converge after exhausting the allocated trials and report these results with a (*) marker, signaling the challenge in tuning parameters for such algorithms. More details are available in Appendix. 6.6.

Evaluation. Real robot evaluations can have high variance due to reset conditions and hardware noise. For each agent, we collect 12 trajectories and report their mean and standard deviation of scores. To confirm the reproducibility of our results and robustness to seed sweeping, for agents that contributed to our conclusions (usually the best and the second-best agents) we report performance swept over three consecutive random seeds (122, 124 in addition to the fixed seed of 123) in Appendix 6.9. To verify the statistical significance of our results when comparing performance between agents, we report the p-value of paired difference tests in Appendix 6.10.

6.6 Training Details

We use implementations of BC and MOREL from the MOREL author implementation. For the later, we add a weighted behavior cloning loss to its policy training step to serve as a regularizer, inspired by [35]. We use AWAC and IQL implemented in the open sourced d3rlpy library [56].

Our code base was built upon the author's implementation of MOREL [12] and the D3RLPY [56] library. We used the same fixed random seed for all our experiments (123), unless otherwise specified. For hyperparameter tuning, we always started training using the default hyperparameters. If the training loss reported by the agent did not converge, we adjusted the learning rate and retrain, up to 5 agents, till we find a model that converge or have been trained for $5,000,000$ steps using batch size 2048. For model whose training loss exploded (e.g., AWAC), we choose an checkpoint from earlier of the training when the loss were relatively stable for $100,000$ steps (frequently, this was an agent that finished about half a million to a million training steps). Surprisingly, when evaluated on real robot, models that reported convergence did not necessarily perform better than model that did not converge.

Practicality of Training and Tuning BC was the cheapest to train ($\sim$ 3min) and easiest to converge (no additional tuning required). MOREL was the second shortest to train ($\sim$ 4 hours); most MOREL agents were able to converge, judged by the reward of trajectories generated by the learned dynamics model. AWAC agents took longer to train ($\sim$ 12 hours) and had the most trouble converging (8 of the 16 agents in the ablation table could not converge in allocated trials). IQL agents took the longest to train (10 $\sim$ 24 hours) but had more success converging. Though loss convergence during training or a good reward estimated by the learned dynamics model or learned value function cannot indicate the agent's true performance, it is helpful for selecting an agent to test. Since some AWAC agents had trouble converging, we selected an earlier checkpoint before loss explosion and documented their performance, which, surprisingly, yielded higher reward than some agents that reported convergence. We leave it to future work to investigate this phenomenon.

6.7 Ablations

Table 6 summarizes all agents' performance for *in-domain* tasks. Interestingly, two of the ORL agents, IQL and AWAC, achieved higher scores than BC trained on 2 out of 4 tasks. After verifying statistical significance, we confirm that even with abundant, in-domain demonstrations, IQL outperformed BC on two tasks. For the other tasks: on the simplest task `reach`, the best version of all agents reached comparable performance. On the hardest task `PnP`, BC outperformed the best ORL agents. We thus recommend considering both BC and IQL as baseline for imitation learning.

Sensitivity to representation Empirically, BC demonstrated robustness to different state and action spaces, whereas ORL agents had high-variance. In 3 of 4 tasks considered, BCperformed the best when using absolution joint position as the action space and including velocity in the state space (**AbsVel**). On all tasks, ORL agents performed better using delta action space (**Delta**) rather than joint position. Intuitively, using the delta action space would be equivalent to restricting the policy to move in a unit ball centered around the current state. Such constraints could benefit RL policies which need exploration and sampling in action space more than it helped BC, which simply learns the mapping from states to actions. We also observed that our best agents all included velocity in their state space, despite that velocities on real robot fluctuate with hardware noise.

6.8 Training Behavior Cloning with Top-K% Trajectories

To ensure that our dataset contains high quality trajectories that is sufficient to train behavior cloning, we launched new experiments training behavior cloning using only the Top-k % of the best trajectories. In Figure. 3, we plot the distribution of performance of our data for each task. For `reach`, `slide`, `lift`, 90% of trajectories complete the task with good scores (> 0.75). For (our most difficult task), 50% of our collected trajectories completed the task (scores > 0.8).

Thus we train BC for `reach`, `slide`, `lift` on Top-90% of data and train BC for `PnP` on Top-50%,70%,90% of data and observe that, BC in our experiments benefit from using the full dataset.

6.9 Sweeping of Random Seeds

We evaluated an addition of 28 agents for 340 trajectories for a total of 70 hours including training and testing to inspect how the scores for critical agents (i.e., the best agents for a category) would vary by random seeds. We now have 3 seeds for each of the following agents:

1. The Best Agents for each task in Table 6
2. The Second Best Agents for each task in Table 6
3. ORL agents with out-domain datasets in in Table 1

The original agents are trained with seed 123, we trained the additional agents with seed 122 and seed 124. Each seed is evaluated on 12 trajectories. The results are listed and we observe that $\sim$60%

Task	Agent	Representations			
		AbsNoVel	**AbsVel**	**DeltaNoVel**	**DeltaVel**
Reach	BC	.86 ± .07	.77 ± .12	.91 ± .03	**.92 ± .05**
	Morel	.80 ± .09	.58 ± .11	.86 ± .07	.92 ± .04
	AWAC	.77 ± .11	.71 ± .16	.92 ± .03	**.93 ± .05**
	IQL	.84 ± .15	.87 ± .10	.90 ± .03	.89 ± .07
Slide	BC	.62 ± .17	**.68 ± .15**	.55 ± .20	.55 ± .10
	Morel	.36 ± .19	.12 ± .24	.53 ± .15	.63 ± .16
	AWAC	.55 ± .17 *	.59 ± .15 *	.57 ± 0.14	.73 ± .11
	IQL	.67 ± .14	.59 ± .17	.71 ± .14	**.77 ± .07**
Lift	BC	.76 ± .18	**.82 ± .18**	.72 ± .23	.61 ± .14
	Morel	.46 ± .19	.15 ± .09	.68 ± .19	.65 ± .16
	AWAC	.52 ± .08 *	<0 *	.86 ± .15 *	.82 ± .12
	IQL	.68 ± .16	<0	.84 ± .14	**.88 ± .15**
PnP	BC	.63 ± .12	**.82 ± .19**	.56 ± .05	.68 ± .20
	Morel	<0	<0	**.75 ± .20**	.75 ± .22
	AWAC	.45 ± .16 *	<0	.63 ± .23 *	.74 ± .18 *
	IQL	.47 ± .14	<0	.55 ± .16	.60 ± .23

Table 6: Performance of all algorithms on varying representations. We trained all agents on four settings: to include velocity in state or not (**Vel** versus **NoVel**); to use absolute or delta action space (**Abs** versus **Delta**). For each task, the best BC agent and the best ORL agent are highlighted and bolded. Agents that could not converge during training time are marked with (*). Some agents triggered violent crashes on robot (<0). Underline scores are swept over 3 seeds.

Task	Top-k%	#Trajs	Threshold for Demo	Score	Original Score (BC with full data)
`reach`	90	900	0.909	0.899 ± 0.037	0.924 ± 0.048
`slide`	90	657	0.774	0.659 ± 0.152	0.681 ± 0.147
`lift`	90	554	0.787	0.784 ± 0.157	0.823 ± 0.177
`PnP`	50	304	0.935	0.723 ± 0.217	0.818 ± 0.185
	70	426	0.792	0.789 ± 0.290	0.818 ± 0.185
	90	548	0.656	0.789 ± 0.204	0.818 ± 0.185

of newly trained agents change score by less than 1%, ∼90% of agents change by less than 2%, and the maximum change was 6% from one agent (whose score change does not affect our conclusion).

Best Agents in Table 6	**Seed 122**	**Seed 124**	**Seed 123** (original seed)	**Means w/ 3 seeds**	**Mean diff**
AWAC, DeltaVel,`reach`	0.920 ± 0.031	0.919 ± 0.066	0.935 ± 0.032	0.925 ± 0.047	0.01 (1.07%)
IQL, DeltaVel, `slide`	0.781 ± 0.038	0.763 ± 0.044	0.757 ± 0.095	0.767 ± 0.065	-0.01 (-1.32%)
IQL, DeltaVel, `lift`	0.877 ± 0.166	0.878 ± 0.158	0.884 ± 0.120	0.880 ± 0.149	0.004 (0.45%)
BC, AbsVel, `PnP`	0.819 ± 0.199	0.800 ± 0.195	0.836 ± 0.157	0.818 ± 0.185	0.018 (2.15%)

6.10 Statistical Significance of Conclusions

In this section we report the statistical significance of the conclusions we drew from our empirical study. To evaluate every trained agent for every task, we collected at least 12 trajectories and

Second Best in Table 6	**Seed 122**	**Seed 124**	**Seed 123** (original seed)	**Means w/ 3 seeds**	**Mean diff**
MOREL, DeltaVel, `reach`	0.919 ± 0.034	0.908 ± 0.042	0.925 ± 0.028	0.917 ± 0.036	0.008 (0.86%)
BC, DeltaVel, `reach`	0.921 ± 0.051	0.917 ± 0.055	0.934 ± 0.032	0.924 ± 0.048	0.01 (1.07%)
BC, AbsVel, `slide`	0.699 ± 0.125	0.698 ± 0.120	0.645 ± 0.18	0.681 ± 0.147	-0.036 (-5.58%)
MOREL, DeltaVel, `slide`	0.655 ± 0.157	0.602 ± 0.180	0.629 ± 0.136	0.629 ± 0.160	0 (0%)
AWAC, DeltaVel,`slide`	0.757 ± 0.068	0.703 ± 0.108	0.739 ± 0.144	0.732 ± 0.113	0.007 (0.95%)
BC, AbsVel, `lift`	0.821 ± 0.192	0.832 ± 0.177	0.818 ± 0.161	0.823 ± 0.177	-0.005 (0.61%)

ORL in Table 1	**Seed 122**	**Seed 124**	**Seed 123** (original seed)	**Means w/ 3 seeds**	**Mean diff**
AWAC on `PnP` w/ slide+lift	(diverged)	0.811 ± 0.103	0.815 ± 0.134	0.813 ± 0.121	0.002 (0.25%)
AWAC on `PnP` w/ slide+lift+pnp	0.759 ± 0.180	0.773 ± 0.204	0.742 ± 0.175	0.758 ± 0.188	-0.016 (-2.16%)
IQL on `PnP` w/ slide+lift	0.838 ± 0.103	0.847 ± 0.117	0.843 ± 0.120	0.842 ± 0.114	0.001 (0.12%)
IQL on `PnP` w/ slide+lift+pnp	0.842 ± 0.170	0.826 ± 0.211	0.829 ± 0.163	0.833 ± 0.183	-0.004 (-0.48%)
MOREL on `lift` w/ slide+lift+pnp	0.879 ± 0.124	0.904 ± 0.119	0.906 ± 0.151	0.896 ± 0.133	-0.01 (-1.1%)

calculated their scores. For best agents or agents used in comparison, we trained them with additional seed to collect a total of 36 trajectories.

We note that the distribution of scores is unknown. We cannot exclude the possibility of the distribution being skewed, as the agent could perform better in a certain task region because of the nature of the task. Therefore, we conducted the Wilcoxon signed T-test for paired samples to calculate the p-value.

With $p0.1$, we reject the null hypothesis that the two models' have identical scores. Tasks and application-domains determine the confidence level requirements for any application. This often requires domain knowledge and might not transfer between different applications even for the same task. For openness and interpretability, we clearly outline our statistical tests and list our p-values, leaving it up to the readers to justify their statistical significance required for their applications. We found that:

1. On in-domain tasks, we observe that: On the simplest task `reach`, all agents achieved comparable performance ($p > 0.1$). On `slide`, IQL outperformed BC ($0.77 > 0.68, p = 0.001$). On `lift`, IQL outperformed BC ($0.88 > 0.82, p = 0.041$). On `PnP`, BC outperformed the best ORL agent ($p = 0.016$).
2. Testing agent's ability to generalize to task space lacking data support, we verify that MOREL and AWAC achieved comparable or better performance than BC for regions lacking data support (MOREL: $0.79 \sim 0.76, p = 0.50$, AWAC: $0.83 > 0.76, p = 0.006, p = 0.25$), despite that these ORL agents were having poorer performance on regions that have more data support ($0.67 < 0.78, p = 0.062$).
3. In terms of leveraging multi-task data, MOREL has clearly benefited from inclusion of more data. On `slide`, the model achieved significantly higher performance when using combined data from three tasks $0.63 \sim 0.72, p = 0.027$). On `lift`, the model achieved significantly higher performance when using combined data from three tasks ($0.68 \rightarrow 0.90, p = 0.003$).

For AWAC, it gained performance on `lift`($0.86 \rightarrow 0.90, p = 0.059$). IQL had most success for `PnP` task after leveraging `slide+lift` data ($p = 0.001$).

6.11 Limitations

Our evaluation primarily focused on three representative ORL algorithms which have shown strong performance in simulated benchmarks. However, ORL is a rapidly evolving research field, with many different classes of algorithms [57, 14, 35]. Therefore, expanding the scope of tasks, evaluation, and algorithms would offer interesting and valuable future work. Moreover, all ORL algorithms have several hyperparameters that influence their learning. We followed instructions and conventional wisdom in the community to tune parameters, but acknowledge that our experiments do not preclude the possibility that one could obtain better performing agents using a different set of parameters and seeds. Hyperparameter and model selection for offline RL is an emerging research sub-field [31] and progress here would also help advance the applicability of ORL to robot learning. We hope this study and our open-source codebase will facilitate this undertaking.