Dynamic Marketing Policies: Constructing Markov States for Reinforcement Learning

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Many firms want to target their customers with a sequence of marketing actions, rather than just a single action. We interpret sequential targeting problems as a Markov Decision Process (MDP), which can be solved using a range of Reinforcement Learning (RL) algorithms. MDPs require the construction of Markov state spaces. These state spaces summarize the current information about each customer in each time period, so that movements over time between Markov states describe customers' dynamic paths. The Markov property requires that the states are "memoryless," so that future outcomes depend only upon the current state, not upon earlier states.

We propose a method for constructing Markov states from historical transaction data by adapting a method that has been proposed in the computer science literature. Rather than designing states in transaction space, we construct predictions over how customers will respond to a firm's marketing actions. We then design states using these predictions, grouping customers together if their predicted behavior is similar. To make this approach computationally tractable, we adapt the method to exploit a common feature of transaction data (sparsity). As a result, a problem that faces computational challenges in many settings, becomes more feasible in a marketing setting. The method is straightforward to implement, and the resulting states can be used in standard RL algorithms. We evaluate the method using a novel validation approach. The findings confirm that the constructed states satisfy the Markov property, and are robust to the introduction of non-Markov distortions in the data.

1. Introduction

Standard targeting models are myopic. They treat marketing actions as single period decisions with single period outcomes. However, in practice, firms can often implement a sequence of marketing actions in order to affect current and future outcomes. For example, instead of just deciding which customers to call (and which not to call), it may be optimal to call, then email, then mail to one customer, while for another customer it is optimal to call twice and then email.

A sequential targeting problem can be interpreted as a Markov Decision Process (MDP), which can be solved using a range of Reinforcement Learning methods. We will formally define MDPs in Section 3, but we can illustrate the concept using a baseball example. The objective of the batting team is to score as many runs as possible in each inning. At each point in time, the situation can be summarized using a finite set of possible states. For example, suppose there is a runner on first base, the other bases are empty, and there is one out in the inning. We will label this as State 1. A key feature of an MDP is that there is a decision to be made. In our example, we will focus on the runner's decision to attempt to steal second base.[1] The runner has two action options (his "action space"): he can attempt to steal or not. If he steals and succeeds, the new state of the game will be: a runner on second base, other bases are empty, and there is one out in the inning (we will denote this as State 2). In contrast, if the runner steals and fails, the state of the game will change to: all of the bases are empty and there are two outs (denote this as State 3). Thus, the choice of action by the runner affects the transitions to future states.

The transitions to future states in turn affect the expected number of runs the team will score in that inning (the "expected rewards"). A simple empirical average from past Major League Baseball games reveals that on average a team scores 0.5088 runs in State 1, 0.6675 runs in State 2 and 0.0986 runs in State 3. Using these expected rewards, we can calculate the values associated with each action in State 1 (the "state-action pairs"): if the runner does not attempt to steal we remain in State 1 and expect to score 0.5088 runs. If the runner steals, the expected rewards from the *State1-Steal* (state-action) pair is: $0.6675 * \beta + 0.0986 * (1 - \beta)$, where β is the probability of success. By comparing the values of the two state-action pairs (*State 1-Steal* and *State 1-Not Steal*), the runner can choose the optimal action in State 1. In this case, the runner should steal as long as the probability of success exceeds 0.7201. This probability might also be estimated by observing past outcomes (ideally past outcomes for that runner and that pitcher).

Notice that the value associated with attempting a steal incorporates the long-term impact of the current action. This also allows the choice of the current action to incorporate the impact of future actions. For

[1] We will ignore the actions and outcomes for the batter.

example, if the runner is particularly good at stealing bases, and so is also likely to successfully steal from second base to third base, this will increase the expected number of runs the team will score in State 2. The expected rewards for *State1-Steal* should (and would) incorporate this adjustment. The states we have described are just three of many possible states in baseball. Players can learn the optimal action in the other states using a similar process.[2]

It is helpful to list the four steps in the process:

1. Design states that summarize the situation in each time period. These time periods typically match the timing in which a decision-maker (a firm) needs to choose actions (whether or not to contact a customer).
2. For each action in each state (each state-action pair), estimate the current period rewards and transition probabilities from the current state-action pair to any future states. In our baseball example, the transition probability for the *State 1- Steal* pair is β to State 2 and $(1- \beta)$ to State 3.
3. Estimate the value of each state-action pair for a given policy ("Policy Evaluation"). This is typically the expected value of current rewards (if any) together with the expected value of the rewards in future states. For example, in our baseball example, the value of the *State1-Steal* pair is calculated as $0.6675 * \beta + 0.0986 * (1 - \beta)$.
4. Improve the policy by choosing the action in each state that has the highest value ("Policy Improvement"). In State 1 the runner only steals second base if $0.5088 < 0.6675 * \beta + 0.0986 * (1 - \beta)$.

Standard Reinforcement Learning (RL) algorithms mimic this logic - a typical RL algorithm uses the same four steps. Steps 1 and 2 can be thought of as preliminary steps. Once the states are designed and the transition probabilities estimated, we do not re-visit Steps 1 and 2.

Notice that the value of each state-action pair depends upon the actions in future states. For this reason, Steps 3 and 4 (Policy Evaluation and Policy Improvement) are solved iteratively. We value the current state-action pairs using an initial policy (choice of actions in each state), we then use the values of each state-action pair to improve the policy. We can then re-value the state-action pairs under this improved policy, and then further improve the policy. This is the standard iterative approach for solving RL problems, and there are theoretical guarantees on its performance (we discuss this iterative approach in greater detail in Section 6).

[2] We caution that we use this baseball example solely to help illustrate the concept of an MDP to readers who are new to this concept. There are some obvious discrepancies between this baseball example and the targeting of marketing actions. For example, in baseball there is a competing team. The actions of competing firms are generally not an important focus in targeting models (although they could potentially be incorporated). The states in baseball are also somewhat pre-defined, which is not the case when targeting marketing actions. However, even in baseball, we could modify the design of states by looking beyond the positions of the runners to also consider the number of innings, the score in the game, the identity of the players, the weather, whether it is a home game etc.

This approach is directly applicable to targeting a sequence of marketing actions. However, because most RL methods are based on the MDP framework, designing states is important. The states must satisfy the Markov property, which requires that the rewards in the current state and the transitions to future states depend only upon which state a customer is in, and they do not depend upon past states. In our baseball example, the expected number of runs in State 2 must not depend upon how the status of the inning arrived at State 2. The probability of future outcomes should be the same irrespective of whether the inning was in State 1 and the runner stole second base versus a batter hit a double with no runners and 1 out in the inning. In this respect, the states must be memoryless. As we will demonstrate, even small breaches of the Markov assumption can lead to dramatic errors. To help understand why, imagine that two customers in the same state actually have different transition probabilities. Because they are in the same state, they will be treated as though they have the same transition probabilities, which is an error. The errors will introduce inaccuracies in predictions of which states the system will transition to in future periods, and these inaccuracies will generally survive and perpetuate throughout the dynamic system. In practice, the Markov property ensures that the distribution of future outcomes is identical within each state-action pair. Customers who end up at a given state are detectably identical under different policies. As a result, the learning algorithm can optimize solely using the states to identify differences between customers.

In this paper, we address the following research question: how do we construct Markov states from historical transaction data so that we can use RL to solve sequential targeting problems in marketing? The approach we propose relies on a key insight: states that satisfy the Markov property contain the same information about the probability of future outcomes as the raw training data. The predictive state representation (PSR) literature in computer science exploits this insight by proposing that we use the training data to calculate probabilities over how agents (customers) will respond to future (firm) actions.[3] These probabilities are not propensity scores; they measure the responsiveness of customers to the firm's marketing actions. Customers are grouped together if they are expected to respond in the same way to future marketing actions, and the resulting states are guaranteed to be Markov.[4]

Intuitively, we first construct predictions over how customers will respond to a firm's marketing actions. We then divide these continuous probabilities into discrete buckets to define discrete states. The main challenge is that there is an infinite combination of future customer behaviors (future events) to predict.

[3] See for example: Littman et al. (2001) and Singh et al. (2004).

[4] Grouping customers by their responsiveness to marketing actions has a parallel in the training of static targeting models, which consider a single firm action. Static models are typically trained (using experimental data) to segment customers based on responsiveness to firm actions (see for example: Ascarza 2018; Simester et al. 2019a; Dubé and Misra 2019). However, the segmentation in these static models does not need to satisfy the Markov assumption. Indeed, the Markov assumption is not well-defined in a static setting.

To address this, we identify a finite set of “core” future behaviors, from which we can estimate the probability of any future customer behavior. We then focus on the probabilities of these core future behaviors to create discrete states. The resulting discrete states satisfy Step 1 of the four steps in a standard RL algorithm (described above). Once we design the states, the remaining three steps can be performed using standard methods.

One of our contributions is to recognize that this problem is often easier to solve in a marketing setting, because transactions by individual customers are often relatively sparse. This can greatly reduce the range of future events that we need to consider, and considerably lower the computational burden of finding the “core” events. A problem that is computationally challenging in many settings becomes more feasible in many marketing settings.

We design a novel validation approach to show that the states designed by our proposed approach are robust to the introduction of non-Markov dynamics in the data generation process. To accomplish this, we need to know the ground truth of the data. For this reason, we use simulated data, which is constructed based on actual email experiments conducted by a large company. We introduce non-Markov distortions to both the rewards and the transition probabilities (Step 2 in the four steps listed above). We then show that our value function estimates are robust despite these distortions. This validation approach allows us to isolate the performance improvement attributable to designing states that satisfy the Markov assumption.

Our contributions are three-fold. First, we identify an important marketing problem that can be solved by RL techniques. More importantly, we point out that a key to the successful application of these RL methods to sequential targeting in marketing is to construct states that satisfy the Markov property. Second, we adapt the methods proposed in the PSR literature to our setting by exploiting the sparsity in transaction data. Sparsity arises if only a relatively small proportion of customers purchase at each time period, which is a feature that is common in customer transaction data. We highlight the importance of sparsity both when motivating our proposed method, and when distinguishing the method from standard PSR methods. Sparsity also allows us to provide a guarantee on the performance of our proposed method. Third, we propose a novel approach to validation, which highlights the importance of constructing states that satisfy the Markov property.

The paper continues in Section 2 with a review of the literature. We illustrate the criteria of “good” states in Section 3 and in Section 4 introduce the conceptual foundations of our proposed method. Section 5 describes the proposed approach in detail. A standard RL dynamic optimization method is briefly reviewed in Section 6, and Section 7 describes the validation of the method. The paper concludes in Section 8 with a discussion of limitations.

2. Literature Review

There is an extensive literature investigating how to target firms' marketing actions. Many of these papers focus on the myopic problem of choosing which marketing action to give to each customer in the next period. For example, Simester et al., (2019a) investigate seven widely used machine learning methods, and study their robustness to four data challenges. Dubé and Misra (2017) describe a method for customizing prices to different customers. Ostrovsky and Schwarz (2011) propose a model for setting reserve prices in internet advertising auctions. Rafieian and Yoganarasimhan (2018) investigate the problem of targeting mobile advertising in a large network.

Separate from the targeting literature, there have also been many studies demonstrating that marketing actions can have long-term implications (see for example Mela et al. 1997, Jedidi et al. 1999, and Anderson and Simester 2004). However, relatively few papers have attempted to optimize a sequence of marketing actions to solve long-run sequential targeting problems. One of the earliest attempts to solve this problem was Gönül and Shi (1998), who focused on the optimization of a sequence of catalog mailing decisions. They used the structural dynamic programming model proposed by Rust (1994), and jointly optimized both the firm's catalog mailing policy and the optimal customer response. Khan et al., (2009) used a similar approach to optimize digital promotions for an online grocery and drug retailer.

Simester et al., (2006) solve the sequential catalog mailing policies using a simple and straightforward model, which introduces Reinforcement Learning methods to marketing. They compare the performance of the model with the firm's current policy in a champion versus challenger field experiment. The model shows promise but under-performs for high-value customers. One possible explanation for this is that the state space they construct is not guaranteed to satisfy the Markov property. Zhang et al., (2014) consider a dynamic pricing problem in a B2B market using a hierarchical Bayesian hidden Markov model, and Hauser et al., (2009) apply a partially observable Markov decision process model to the website morphing problem. We will highlight the difference between this hidden Markov approach and our proposed approach in later discussion.

Many studies have focused on estimation and optimization. In contrast, our focus is on the design of state spaces. The design of state spaces can be thought of as a distinct problem, in the sense that the approach we propose can be used as an input to structural dynamic programming and other RL methods.

Other approaches have been proposed in the literature for overcoming the potential failure of the Markov assumption when constructing states. One method is to explicitly consider history when constructing states. By extending the focus to k-historical periods, it may be possible to ensure that the state space is at least k^{th}-order Markov. Another approach is to use Partially Observable Markov Decision Processes (POMDPs) (Lovejoy, 1991). This class of methods combine MDPs (to model system dynamics) with a hidden Markov model that connects unobserved states to observations. POMDPs have proven particularly well-suited to theory, and there are now several results characterizing the performance of POMDPs and providing guarantees on their performance. Unfortunately, POMDPs have not been as useful in practice. When comparing POMDPs with PSRs (which is the focus of this paper), Wingate (2012) identifies three advantages of PSRs. First, future predictions are easier to learn from data compared with POMDP. The reason is that predicted probabilities of future events are observable quantities, while states in POMDPs are unobservable. Second, all finite POMDP and k-history methods can be transformed to a future prediction state expression, while the converse is not true. Third, due to the second advantage, dynamic optimization methods developed for POMDP and k-history approaches have the potential to be applied to states designed using the PSR approach discussed in this paper.

Finally, this paper can also be compared with recent work in marketing studying how to evaluate targeting models. The previous work has focused on validating myopic targeting problems. Simester et al., (2019b) propose the use of a randomized-by-action (RBA) design to improve the efficiency of model comparisons. Hitsch and Misra (2018) also propose an RBA design, and stress the comparison of direct and indirect methods. Three recent papers have proposed the use of counterfactual policy logging in different settings to improve the efficiency of model comparison methods (see Johnson et al. 2016, Johnson et al. 2017, and Simester et al. 2019b).

3. Criteria for Designing States

We first introduce the concept of a Markov Decision Process (MDP) and provide formal definitions of states and the Markov property. We then apply these definitions to the task of designing an optimal sequence of marketing actions using historical transaction data. Readers who are familiar with the use of MDPs for targeting can skip this section, and move ahead to Section 4.

States and MDPs

A "state" is a concept that originated from Markov Decision Processes (MDPs). An MDP is a model of framing an agent's learning problem, and the basis of many Reinforcement Learning algorithms. An MDP is composed of four elements (S, A, R, P): S represents the state space; A is the action space; $R: S \times A \rightarrow \mathbb{R}$ denotes the stochastic numerical reward associated with each state-action pair; $P: S \times A \rightarrow S$ denotes the stochastic transition probability associated with each state-action pair. The transition probabilities represent the dynamics of the MDP. In this paper, we focus on a finite MDP where (S, A, R) all have a finite number of elements.

The baseball example can be used to illustrate each of these concepts. Here, the runner is the agent, and everything that the runner interacts with is the environment. The interactions occur in discrete time periods $\{0, 1, 2, 3, \ldots\}$. These time periods typically match the occasions on which the agent makes decisions. In our baseball setting, the time periods might refer to each pitch, because the runner decides to steal or not on each pitch.

At each period t, a runner in state $s_t \in S$ selects an action $a_t \in A$. In this example, the runner has two action options: $A = \{Steal, Not\ Steal\}$. As a result of the runner's action a_t and the state of the environment s_t, the agent receives a numerical reward $r_t \in R$. Here, R is the number of runs scored in the time period, which is a number and can be 0. The reward R is generally a random variable that is a draw from a stochastic distribution. This distribution may be degenerate and so the reward in that time period may be known with certainty (for example, the expected reward in our *State 1-Not Steal* pair).

In period $t + 1$, the runner transits to state, $s_{t+1} \in S$. This could be a transition back to the same state (this happens with certainty if the runner does not steal) or a transition to a different state. The transition probabilities depend upon the state in period t and the action taken in that period $a_t \in A$. The transition probabilities are stochastic, although the stochastic probability distribution may also be degenerate (such as when the runner does not steal). Unless the transition probabilities are degenerate, the runner enters period $t + 1$ with some uncertainty about which state he will transition to in period $t + 1$. We model these dynamics using the transition probability P. Specifically, consider $s_t = s \in S$, $a_t = a \in A$ and $s_{t+1} = s' \in S$, then the transition probability is: $Pr\{s_{t+1} = s' | s_t = s, a_t = a\}$.

In our simple example, we defined the states in baseball using the location of runners on bases and the number of outs in the inning. However, there may be many other factors that affect the reward R and the transition probability P. For example, the success of the attempt to steal may depend upon the catcher, the pitcher, the second baseman, the weather, the stage of the season, past progress of the game, etc. In an MDP framework, we summarize all of these environmental conditions using the concept of *states*. A critical requirement is that in an MDP framework, the reward and transition probabilities in each time period only depend upon the current period's state and the agent's action. Formally, $r_t(s_t, a_t)$ and $p_t(s_{t+1}|s_t, a_t)$ only depend upon s_t and a_t. They do <u>not</u> depend upon any previous states or actions, such as s_{t-1} and a_{t-1}. This assumption is the Markov assumption in an MDP. This requirement is one place that Reinforcement Learning models can fail when applied to marketing problems (and is the focus of this paper).

We stress that the Markov assumption is not a restriction on the decision-making process, but on the *state*. The agent's decision process may not be memoryless, but we can carefully design the state space to make it memoryless. For example, when defining states we can include all the past information in the environment that is relevant for the current period rewards and state transitions. If we can accomplish this, we are guaranteed to satisfy the Markov assumption. However, we cannot use all past history data because it is computationally infeasible. The transition probabilities need to be updated recursively (see the discussion of the state updating process at the end of Section 4). Thus, we need to find sufficient statistics for all of the relevant information, which is the (non-trivial) goal of state construction.

Not all Reinforcement Learning methods depend upon the Markov assumption. For example, Approximate Dynamic Programming methods (which include Deep Reinforcement Learning), do not rely upon the Markov assumption. The method we use for learning (after defining the states) belongs to the tabular family of Reinforcement Learning methods, which have well-established theoretical convergence properties. In contrast, approximation methods have known limitations in the robustness of their convergence. In addition, approximation methods cannot augment the state representation with memories of past observations. They must still embed the k-history idea in state representations.[5] However, approximation methods have the ability to deal with large scale problems, which is a limitation of the learning method we present here. We see this as an important future research opportunity and will discuss it further in Section 8.

We next apply the MDP framework to the design of dynamic marketing policies in which a firm wants to optimize a sequence of marketing actions.

MDPs and Targeting

Many firms want to "target" their marketing actions by matching different marketing actions to different customers. The marketing actions may include promotions, pricing, advertising messages, product recommendations, outbound telephone calls etc. For ease of exposition, in this paper, we will focus on the

[5] We can illustrate using linear approximation methods as an example. Linear methods assume that the approximate function, $\hat{v}(\cdot, \omega)$, is a linear function of the weight vector ω. Corresponding to every state s, there is a real-valued vector $x(s) \equiv (x_1(s), x_2(s), \dots, x_d(s))^T$, with the same number of components as ω. The vector $x(s)$ is called a feature vector representing state s. For example, in our baseball example, features can include bases, outs, components, etc. Linear methods approximate the state-value function by the inner product between the weight ω and the features $x(s)$:

$$\hat{v}(\cdot, \omega) \equiv \omega^T x(s).$$

The mapping from states to features has much in common with the familiar tasks of interpolation and regression. We can use various functional forms, for example, linear, polynomials, Gaussian, etc. No matter which functional form we use, we still look at the original data space. To control ω as a finite vector over time, we need to reduce feature representations to a limited number of previous periods (which is the k-history idea). Deep Reinforcement Learning, assumes a nonlinear function form between the weight ω and the features $x(s)$, but still cannot incorporate all past information in the data.

targeting of a sequence of email advertisements. The procedure for applying the method to other marketing problems should be clear from this email application.

We interpret the firm's sequence of mailing decisions as an infinite horizon task, and seek to maximize the discounted stream of expected future profits. Recall that time is measured in discrete periods, which in this application is the mailing date for each email campaign. We again use S to denote the set of discrete states. In sequential targeting problems, each state groups together "similar" customers at each time period. When we say "similar", what we hope for is that customers in the same state will respond in a similar way to future targeting policies. Clearly, this is an important challenge. How to design such a state space that satisfies the Markov assumption is the problem that we tackle.

We will assume that the firm can choose from two possible actions: mail or not mail. To stress the relationship between action and state, we use notation $a_{ts} \in A = \{0,1\}$, where $a_{ts} = 1$ denotes a decision to mail at period t to all customers in state s. The numerical reward at each period $r_t(s_t, a_{ts})$ is the net profit earned from a customer's order less (any) mailing costs, and the transition probability $p_t(s_{t+1}|s_t, a_{ts})$ gives the probability of becoming a customer in state s_{t+1} when the customer is in state s_t at period t and receives mailing action a_{ts}.

A policy ($\pi: S \to A$) describes the mailing decision for each state. The goal of the firm is to find a policy that maximizes the discounted expected future profits:

$$V^{\pi}(s_0) = E_{\pi}[\sum_{t=1}^{\infty} \delta^t r_t(s_t, a_{ts})|s_0].$$

Here, δ is a discount factor between time periods, and s_0 is the initial state at period zero. In an MDP framework, we normally describe $V^{\pi}(s_0)$ as the value function of state s_0 under a policy π.

A critical assumption in an MDP is that the states satisfy the Markov property. In the next section we propose a method for designing Markov states.

4. Introduction of the Proposed Method

The goal while constructing states is to create a mapping from the historical transaction data to some manageable dimension space that satisfies the Markov property. What we propose is to map from the space of historical transaction data to probability space, representing predictions of how customers will respond to future marketing actions. The challenge is to find a set of events that are sufficient for the prediction of all other future purchasing events. We elaborate on this concept below.

State Representation

To understand our state representation idea, we need to introduce the concept of an *observation*, denoted by O. The firm's interaction with its environment can now be described as: at each time period t, the firm executes a mailing action $a_t \in A = \{Not\ Mail, Mail\} = \{0,1\}$ and receives an observation $o_t \in O$. Different customers can receive a different a_t, and the observation o_t can be multi-dimensional. For example, observation o_t can be a two-dimensional variable, with o_t^1 corresponding to the number of units a customer purchased at period t, and o_t^2 corresponding to the revenue from the customer's purchase at period t. Observation o_t can also include customer characteristics, such as age, gender, and environmental variables measuring seasonality. Because the observation can measure purchasing, the current period reward (in an MDP) can be represented in the observation. For ease of exposition, we will treat observation o_t as a one-dimensional variable. Specifically, we assume $o_t \in O = \{Not\ Buy, Buy\} = \{0,1\}$.

Suppose the firm is at time period t. An action-observation sequence is a sequence of alternating actions and observations. A *history*, denoted as $h = a_1 o_1 a_2 o_2 \dots a_{t-1} o_{t-1}$, describes the action-observation sequence that the firm has experienced from the beginning of time through period $t-1$. A *test*, denoted as $q = a^1 o^1 a^2 o^2 \dots a^n o^n$, describes the action-observation sequence that might happen in the future. We define the predicted probability of a test ($q = a^1 o^1 a^2 o^2 \dots a^n o^n$) conditional on a history ($h = a_1 o_1 a_2 o_2 \dots a_t o_t$) as:

$$p(q|h) \equiv Pr\{o_t = o^1, o_{t+1} = o^2, \dots, o_{t+n-1} = o^n | h, a_t = a^1, a_{t+1} = a^2, \dots, a_{t+n-1} = a^n\}.$$

We will use an example to help illustrate the meaning of this probability. In an email (or direct mail) targeting problem, the firm observes whether each customer received each past email, and whether the customer bought in each past period. This is the *history* we refer to. For example, consider the test: ($q = a^1 o^1 a^2 o^2$), where $a^1 = a^2 = 1$ and $o^1 = o^2 = 1$. The conditional probability $p(q|h)$ represents the predicted probability that a customer bought in both period t and period $t+1$, conditional on the customer's past history and receiving an email at period t and period $t+1$.

The central idea of Predictive State Representations (PSRs) is to choose a set of tests, and use the predictions of those tests to construct states. Suppose the set of tests has *m* elements, we can write this set of tests as $Q = \{q_1, q_2, \dots, q_m\}$. Informally, tests in Q can be understood as the prediction of future action-observation sequences. To be specific, we use the column vector $p(Q|h) = [p(q_1|h), p(q_2|h), \dots, p(q_m|h)]^T$ as state representations for each customer. Customers with the same $p(Q|h)$ will be grouped into the same state.

The next question is how to choose the set of tests Q so that the states satisfy the Markov assumption. We require that Q is chosen so that $p(Q|h)$ forms a sufficient statistic for all future test predictions. More formally, for any test q, there exists an $(m \times 1)$ vector m_q, such that[6]:

$$p(q|h) = p(Q|h)^T m_q.$$

Throughout the paper, we assume the existence of such a set of tests Q, and call the tests in set Q as *core tests*. This is not a strong assumption as for any problem that can be represented by a finite POMDP or a k-history model, there exists a set Q with finite elements that can also represent the problem (Wingate 2012). Moreover, compared with a POMDP, the number of core tests is no larger than the number of hidden states in the POMDP model (Littman et al., 2001).

Properties of the Constructed State

We next explain why the PSR states satisfy the Markov property. Second, we show that the proposed state representation can be recursively updated.

Suppose we already have some historical data up to period t for each customer, denoted as h_t^i for customer i. In our email setting, h_t^i includes whether each customer received an email and bought at each time period before time t. The state representation finds a compact summary of the historical data for each customer. A key insight is that: <u>states that satisfy the Markov property contain the same amount of information about the probability of future purchases as the raw history data</u>. Formally, if two customers (denoted as A and B) who have different histories, $h_t^A \neq h_t^B$, are assigned to the same state group, they should share the same probabilities for the next observation:

$$Pr\{o_t = o \big| h_t = h_t^A, a_t = a\} = Pr\{o_t = o | h_t = h_t^B, a_t = a\}.$$

If this holds for any customers who are assigned to the same state, we can say that the constructed states satisfy the Markov property. More generally, a Markov state should not only be good at predicting the next observation, but also any higher length observations.[7] The PSR approach is motivated by this insight. If we can find the correct core tests (future events) and accurately estimate the probabilities associated with these core tests, then the resulting states will satisfy the Markov assumption.

The equation above also suggests a test of whether a state satisfies the Markov property. The following conditional independence test provides a necessary (though not sufficient) test of whether a state is

[6] We assume a linear relationship for the sufficient statistic.
[7] Notice also that different periods of a specific customer should be treated in the same way as different customers.

Markov: $p(o_t|\ a_t)$ is independent of h_t for all observations in the same state.[8] We will also use this test to help evaluate our state construction method in Section 7.

It is also helpful if states are easy to update. Consider a specific customer who has a history h_t before period t. At period t, this customer receives mailing action a_t and makes purchasing decision o_t. We now have a new history up to period $t+1$, $h_{t+1} = h_t a_t o_t$. We can calculate the predicted probability $p(q_j|h_{t+1})$ where $q_j \in Q$ by:

$$p(q_j|h_{t+1}) = p(q_j|h_t a_t o_t) = \frac{p(a_t o_t q_j|h_t)}{p(a_t o_t|h_t)} = \frac{p(Q|h_t)^T m_{a_t o_t q_j}}{p(Q|h_t)^T m_{a_t o_t}}.$$

This state-update function is easy to estimate, and applies to all histories and all core tests. We next discuss the empirical construction of states from data.

5. Constructing States from Data

The discussion in Section 4 reveals that we need to do two things to learn states from data. First, we need to find the core test set Q. Second, we need to estimate the conditional probability $p(Q|h)$ for any possible h. We start with the problem of finding the core tests.

Finding Core Tests

We first discuss how to find the core-test set Q. This is a challenging task, and early attempts to solve the problem were not able to provide any theoretical performance guarantees (see for example James and Singh 2004 and Wingate and Singh 2008). We propose an adaptive approach based on James and Singh (2004) that exploits ideas from linear algebra (Golub and Van Loan, 2013). Our method, which is easy to understand and implement, exploits sparsity. As we discussed in the Introduction, a common feature of transaction data is that many customers do not purchase every period, and so the incidence of purchases is "sparse" (see for example the distribution of outcomes in Figure 2 in Section 7). This feature will greatly simplify the process of finding core tests.

To understand our proposed approach, we introduce the concept of a *system dynamic matrix*. The system dynamic matrix is a way to represent a sequential decision-making problem (without introducing any new assumptions). Consider an ordering over all possible tests: q_1, q_2 ... . Without loss of generality, we assume that the tests are arranged in order of increasing length. Within the same test length, they are

[8] The test is necessary though not sufficient because it only considers predictions of the next observation, while Markov states should be good at predicting any length future observations.

ordered in increasing values of our binary outcome indicator. For example, the four tests with length one are:

$$q_1 = \text{"}00\text{"} = \{a^1 = Not\ mail, o^1 = Not\ buy\},$$

$$q_2 = \text{"}01\text{"} = \{a^1 = Not\ mail, o^1 = Buy\},$$

$$q_3 = \text{"}10\text{"} = \{a^1 = Mail, o^1 = Not\ buy\},$$

$$q_4 = \text{"}11\text{"} = \{a^1 = Mail, o^1 = Buy\}.$$

The sixteen tests of length two are listed in the Appendix. We define an ordering over histories h_1, h_2 ... similar to the ordering over tests. The difference is that we include the zero-length/initial history $\emptyset$ as the first history in the ordering. The system dynamic matrix is such that the column corresponds to all tests by ordering, the row corresponds to all histories by ordering, and the entries are the corresponding conditional probability $p(q|h)$. An example is given in Figure 1.

Figure 1. System Dynamic Matrix

$$\begin{array}{c|cccccc}
 & q_1 & q_2 & q_3 & q_4 & q_5 & \cdots \\
\hline
\emptyset & p(q_1|\emptyset) & p(q_2|\emptyset) & p(q_3|\emptyset) & p(q_4|\emptyset) & p(q_5|\emptyset) & \\
h_1 & p(q_1|h_1) & p(q_2|h_1) & p(q_3|h_1) & p(q_4|h_1) & p(q_5|h_1) & \\
\vdots & & & & & & \\
h_4 & p(q_1|h_4) & p(q_2|h_4) & p(q_3|h_4) & p(q_4|h_4) & p(q_5|h_4) & \\
h_5 & p(q_1|h_5) & p(q_2|h_5) & p(q_3|h_5) & p(q_4|h_5) & p(q_5|h_5) & \\
\vdots & & & & & & \cdots
\end{array}$$

Notes. Tests and histories are arranged in order of increasing length and, within the same length, in increasing categorical indicator order. The length one tests and histories are listed in the main text: q_1 = "00", q_2 = "01", q_3 = "10", q_4 = "11". The length two tests and histories start with q_5 = "0000" $= \{a^1 = Not\ mail, o^1 = Not\ buy, a^2 = Not\ mail, o^2 = Not\ buy\}$. The other length two tests and histories are listed in the Appendix. $\emptyset$ represents zero-length history, $h_1 = q_1$, $h_4 = q_4$ and $h_5 = q_5$.

The <u>core tests</u> correspond to the linearly independent columns in the system dynamic matrix. This means that to find the set of core tests Q, we just need to find the linearly independent columns of the system dynamic matrix (which has infinite-dimension). If we know all of the matrix entries of the first row, we can get all of the other matrix entries using the state update relationship (which we presented at the end of Section 4). Specifically, we can obtain a specific entry of the system dynamic matrix through:

$$p(q|h) = \frac{p(hq)}{p(h)}.$$

Thus, if we know all of the matrix entries of the first row, we can get all of the other matrix entries using this state update relationship.

The difficulties are three-fold. First, operations on large matrix are computationally challenging. Second, we do not know the true conditional probability $p(q|h)$. Instead, we need to estimate these probabilities from data. This introduces the potential for estimation errors, and these errors could make linearly dependent columns appear independent (it is also possible, though less likely, that independent columns may become dependent). Third, in practice, the length of the available data is finite. For example, we will use a series of just six experiments in the Validation Section. Yet, we are trying to learn the independent columns of an infinite dimension matrix. Because of these data challenges, we can only hope to find approximately correct core tests.

For the first challenge, we recognize that customer transactions are often rare, and so transaction data is often sparse. Our proposed approach exploits this feature of the data to lower the computational burden. For the remaining two challenges, we analyze the problem in the following order. First, we will assume that the probabilities in the system dynamic matrix are known exactly, and consider how to find the linearly independent columns. This step focuses on an infinite system dynamic matrix. Second, we consider how to adjust the algorithm to accommodate estimation errors in the probabilities (the system dynamic matrix remains infinite). Because the adjustments are mathematically involved, we relegate the discussions of this step to the Appendix. Third, we consider how finite data and a finite system dynamic matrix affect the proposed algorithm.[9]

We begin by describing our proposed algorithm, and describing how sparsity helps overcome the computational challenge. The algorithm includes the following five-steps:

1. Delete history rows whose corresponding tests have zero probability at zero-length history. If a test has zero probability at zero-length history, it also has zero probability at longer history. The histories that are not deleted provide the rows of the submatrices used in the next steps.
2. Start from the submatrix containing all tests up to length one. Calculate the rank of this matrix, and the corresponding linearly independent columns.
3. Expand the submatrix to the one whose columns are the union of all length-one tests, the linearly independent tests found so far, and all one-step extensions of these independent tests.[10]
4. Calculate the rank and linearly independent columns of this new submatrix.
5. Repeat Steps 3 and 4 until the rank does not change.

We illustrate the five-steps using a detailed example in the Appendix.

[9] We also explicitly investigate the finite sample issue in Section 7, when we validate the proposed method.
[10] Given a test q, a one-step extension refers to a new test aoq, where $a \in A = \{0,1\}$ and $o \in O = \{0,1\}$.

In Step 1 of the algorithm we exploit the sparsity that is common in transaction histories. For example, in our simulated data, there are five observation levels and two action levels. This results in 10 length-one tests, 100 length-two tests, 1000 length-three tests, etc. We can see that the system dynamic matrix grows exponentially. However, because transactions are sparse, our method will delete many history rows in Step 1. This greatly reduces the computational burden when we expand the submatrix in Steps 3, 4 and 5. In our application, the sparsity in the transaction data allows us to delete approximately 80% of the history paths, which leaves a computationally feasible submatrix.

We can also show that as the size of the system dynamic matrix increases, the level of sparsity grows at least as quickly. This result is important. It increases confidence that our proposed algorithm for finding core tests will continue to be computationally feasible as the size of the system dynamic matrix grows. We relegate the detailed proof to the Appendix, but summarize the main ideas using an illustrative example.

> **Result 1**. As the test length increases, the number of zero probability tests (at zero history) scales faster than the total number of tests.
>
> **Outline of the Proof.** Let $H(L)$ denote the total number of tests with length L, and $G(L)$ denote the number of L-length tests with zero probability at zero history. We aim to show that as L increases, $G(L+1)/G(L) > H(L+1)/H(L)$, so that $G(L)$ scales faster than $H(L)$. We use n to denote the number of possible length-one tests, and begin by recognizing that the total number of tests scales at a rate of n as the length of the tests increases: $H(L+1)/H(L) = n$. This means that we just need to show that $G(L+1) \geq nG(L) + 1$.
>
> To do so we introduce the concept of a one-step extension and a one-step expansion of any test q'. A one-step extension adds the length-one test ao before q', which we can write as aoq'. In contrast, a one-step expansion adds ao after q', which we write as $q'ao$. All of the one-step extensions and one-step expansions of any zero-probability test also have zero probability. This means that every zero probability L-length test has n one-step extensions, all of which have zero probability. This implies that $G(L+1)/G(L) \geq n$. In the proof we establish that there also exists a one-step expansion of a zero probability L-length test that is unique, and does not replicate any of the one-step extensions of the zero probability L-length tests. This implies that $G(L+1) \geq nG(L) + 1$. See the Appendix for additional details.

To help understand the result we will use an example with two actions and two observations. In our example, $n = 4$, and so there are four length-one tests: “00”, “01”, “10”, “11” and sixteen length-two tests. We can write this as $H(1) = 4$ and $H(2) = 16$. Notice that the number of tests $H(L)$ scales at a rate of n as the length of the tests increases: $H(2)/H(1) = n$.

We will show that the tests with zero probability (at zero history) scale at a faster rate: $G(2)/G(1) > n$. The n one-step extensions of “01” include: “0001”, “0101”, “1001”, “1101”. There are also n one-step expansions of “01”. Because every test has n extensions and n expansions, including tests with zero probability, this might suggest that: $G(2)/G(1) \geq 2n$. However, this overlooks the possibility that an expansion of one test may be equivalent to the extension of another test. For example, the extension of “01” to “1101” is equivalent to the expansion of “11” to “1101”. If we ignore the possibility of these repetitions, we would over-estimate the scaling of $G(L)$.

The proof recognizes that, as long as not all of the one-step tests have zero probability, some of the one-step extensions and expansions will be unique. Assume “01”, “10”, and “11” have zero probability, but “00” does not. We summarize all of the one-step extensions and expansions in the table below. The one-step extensions and expansions of “01”, “10”, and “11” all have zero probability. The grey shading identifies the zero probability extensions and expansions that are replicated by extensions and expansions of other zero probability tests, and are thus subject to double counting. Because 9 of the new tests are replicated, the 3*2n = 24 one-step extensions and expansions of “01”, “10”, and “11” yield only 15 unique new length-two tests. However, notice that not all of the one-step extensions and expansions are replicated. As a result, $G(2) > nG(1)$, or in this example, 15 > 12. In turn, this ensures that $G(L+1)/G(L) > H(L+1)/H(L)$.

Table 1. Expansions and Extensions of Zero-Probability Length-One Tests

	“01”	**“10”**	**“11”**
1-Step Expansions	“0100”	“1000”	“1100”
	▨ “0101”	▨ “1001”	▨ “1101”
	▨ “0110”	▨ “1010”	▨ “1110”
	▨ “0111”	▨ “1011”	▨ “1111”
1-Step Extensions	“0001”	“0010”	“0011”
	▨ “0101”	▨ “0110”	▨ “0111”
	▨ “1001”	▨ “1010”	▨ “1011”
	▨ “1101”	▨ “1110”	▨ “1111”

Notes. The table lists the one-step expansions and extensions of each length-one test in our example. The shading identifies the extensions and expansions of the zero-probability length-one tests that are replicated by extensions and expansions of other zero probability tests.

Result 1 assumes that we know the conditional probabilities in the system dynamic matrix exactly. If we continue to make this assumption we can provide a guarantee on the performance of the algorithm.[11]

> **Result 2:** The core tests identified using this 5-step algorithm are correct if all entries in the system dynamic matrix are known exactly.
>
> Proof. See Appendix.

This result guarantees that the core tests obtained using this method are correct. We will label these linearly independent rows as “core histories”.

In practice, there are likely to be estimation errors in the probabilities in the system dynamic matrix. This raises several issues. First, it can influence how we obtain the rank and linearly independent columns in Steps 2 and 4. We relegate a discussion of these details to the Appendix. Second, it becomes more difficult to provide a guarantee on the performance of the method. To do so we need an asymptotic result that as the time-period (denoted as T) of the data increases to infinity ($T \to \infty$), the set of core tests we find are close to the true core tests. The result will not hold if we consider a general matrix structure. However, one feature in the customer transaction behavior can help with this asymptotic result.

The observation is that customers’ purchase behavior will not depend on events that happened infinite time ago. In practice, there will generally exist a time T_0 (which can be very large) such that customers’ purchase behavior at most depend on T_0 time ago events. The implication is that all histories with length higher than T_0 have the same predicted probabilities of all future events as the corresponding length-T_0 histories. Thus, if we have enough time period data, that is, $T \geq T_0$, we will find the correct core tests using our five-step algorithm based on Result 2. We will also demonstrate in Section 7 that the states that we design using the core tests behave well in practice.

Our discussion of the theoretical properties of this method has also been based upon the assumption that we can learn the entries in the system dynamic matrix exactly. In reality, we need to estimate these probabilities from a finite sample of data. We leave the issue of how to estimate the predicted probabilities $p(q|h)$ until the next subsection.

We finish the discussion in this subsection with three additional comments. First, while the five-step algorithm and Results 1 and 2 are both new, we want to acknowledge that they build upon a similar

[11] If we know the probabilities in the system dynamic matrix exactly, another implication is that if a test q' has zero probability at zero-length history, test q' will not be one of the core tests. The reason is that for any history h, $\mathrm{p}(q'|h)$ will be zero. It seems that then we can exclude q' in our five-step algorithms. However, we choose not to do this because under our probability estimation method (introduced in next subsection), $\mathrm{p}(q'|h)$ may not be zero even if $\mathrm{p}(q'|\emptyset)$ is zero. This adjustment helps reduce the effect of empirical errors in probability estimation on the finding of core tests.

method proposed by James and Singh (2004). A difference is that James and Singh (2004) propose submatrix expansion on <u>both</u> histories and tests in each iteration (we just expand the tests). This method is not guaranteed to find the correct core tests even if all entries in the system dynamic matrix are known. Their method also faces computation challenges, which we alleviate by exploiting the sparsity in customer transaction data. In turn, sparsity makes the first step of our algorithm possible, which helps establish the theoretical guarantees in Result 2. Wingate and Singh (2008) propose using randomly selected tests as core tests, but this approach is also not guaranteed to find the correct core tests.

Second, the approach we propose may have difficulties dealing with tests that have long lengths, or settings with large action spaces. We discuss this issue in Section 8.

Finally, we clarify that the algorithm is sequential not iterative. Once the core tests are identified they do not change. As a result, the design of the state space, together with the transition probabilities and rewards for each state-action pair, are static. Although the design of the MDP is static, customers transition dynamically through it. Implicitly this assumes a batch learning environment, in which there is a fixed sample of training data. In an online learning environment, the algorithm may need to be adapted in response to non-stationarities in the design of the MDP.

Calculating Predicted Probabilities

Once we obtain the core tests, we can derive the state for each customer at each period by calculating the (conditional) probabilities of the core tests. Customers with the same probabilities will be assigned to the same state group. Estimating these probabilities from data is conceptually straightforward, but there are several details that deserve comments.

The goal is to estimate the predicted probabilities $p(q|h)$ for the core tests. A straightforward approach is to simply count how many h and hq appear in the dataset, and then take the fraction of hq over h.[12] For example, suppose h = "01" $= \{a_1 = Not\ mail, o_1 = Buy\}$ and q = "11" $= \{a^1 = Mail, o^1 = Buy\}$. Then we can just count how many times "01" and "0111" appear in the dataset, and calculate the "0111" occurrences as a proportion of "01" occurrences.

In order to do this, we need to define time period 0. Notice that in our method, each period's data has a different length of history. This makes the definition of time period 0 important, because the calculation of the probabilities could be sensitive to the choice of starting time. Moreover, if we define time period 0 to always start with the first time period in our data, we will place more reliance on the data at the start of

[12] It is possible that the length of hq is larger than the total time period in the data. For this reason, when we search for the core tests, we restrict attention to submatrices for which we can directly estimate the predicted probabilities $p(q|h)$.

our data period, which places more reliance on the least recent data. This could make our estimation very sensitive to non-stationarity.

To address this concern, we propose the following sampling method. Suppose we have data on T periods and N customers. We divide the N customers randomly into M groups, and for each group we randomly assign time period 0 to different calendar time periods. Each of the customers in the same group are assigned the same calendar time period as period 0, but different groups are assigned different calendar time periods as period 0. In our Validation Section, we use six email experiments, and a sample of 136,262 customers that participated in all six experiments. We would randomly divide these customers into six groups. In one group, period 0 would correspond to the first experiment, another group would treat experiment two as period 0, etc. For the customers for which experiment two is period 0, period 1 would be experiment 3, period 2 would be experiment 4, and so on. The benefit of this approach is that it ensures the estimated probabilities are less sensitive to temporal variation in outcomes. A limitation is that we do not use all of the data; in groups for which there are experiments before Period 0, we do not use these earlier experiments to estimate the probabilities.

Notice that we use the predicted probabilities in the system dynamic matrix in two stages: (a) to find the core tests in the system dynamic matrix, and (b) after finding the core tests we group customers into states according to the conditional probabilities associated with these core tests. We can either re-use the same assignment of customers to period 0 in both places, or we can re-sample after finding the core tests (to re-allocate period 0 across customers). In our implementation, we choose to resample. This has the advantage of reducing the relationship between estimation errors in the two stages. However, it may also increase the risk that random variation results in breaches of the Markov assumption in some states.

Using our data, we can observe which tests happen after which histories. For example, suppose we have a dataset with 3 time periods. Consider a specific customer A who is assigned so that period 0 is the first period in the data. Suppose the data we observe for customer A is "011100" $= \{a_1 = Not\ mail, o_1 = Buy, a_2 = Mail, o_2 = Buy, a_3 = Not\ mail, o_3 = Not\ buy\}$. For customer A, after history $h = "01"$, tests $q = "11"$ and $q = "1100"$ are observed. For some tests our data length is too short to observe whether the test succeeds or not. In these situations, we use the *predicted* probabilities, which we can estimate using the state-update function (provided at the end of Section 4).[13]

Because probabilities are continuous numbers, we discretize the conditional probabilities to ensure that we have multiple observations in each state. It is important that the discretization occurs <u>before</u>

[13] To use the state-update function, we also need to know the parameter vector m_q for test q. This vector can be obtained through $p^{-1}(Q|H)p(q|H)$ where H includes all possible histories. We calculate the matrix inverse using the Moore-Penrose pseudoinverse.

identifying the core tests. In this way, the discretization does not interfere with the Markov property of the states. If we discretize after we derive the core tests, we risk introducing errors that may breach the Markov assumption in some states.

When choosing how finely to discretize the conditional probabilities we face a tradeoff. Discretizing more finely potentially provides the MDP with more information. However, the transition probabilities and rewards in each state become less precise. In our application, we investigated varying the level of discretization, and found that the results were robust to this decision, with little variation in the results.

We may also encounter covariate shift. Covariate shift arises if the distribution of the data used for training a targeting model is different than the data used for implementation (see for example Simester et al. 2019a). In our setting, the probability vector at the last period, which will guide the targeting policy in a sequential targeting problem, might not occur in any prior period. For those probability vectors, we have no observed outcomes. To address this issue, we assign customers with these probability vectors (in the last period) to the nearest state.[14]

In this section we have discussed how to use a sample of historical transaction data to create Markov states. In the next section we (briefly) discuss how standard Reinforcement Learning methods can use these states to estimate value functions and identify optimal policies.

6. Dynamic Optimization

Recall that in the Introduction, we mention four steps in a standard Reinforcement Learning algorithm. Given that we have designed a discrete state space (the first step), we now discuss the remaining three steps. There are many standard methods to use for these steps. Because this is not the focus of the paper, we use the simplest approach, which is easy to understand and implement. This method also has well-established theoretical guarantees.

Estimating Current Period Rewards and Transition Probabilities

Just as the runner in baseball needs to know the transition probabilities from one state to another state for both “not steal” and “steal” actions, we need to estimate the transition probabilities for both the “mail” and “not mail” actions. In terms of an MDP, we need to estimate the stochastic transition probabilities $P: S \times A \rightarrow S$.

We use a simple nonparametric approach to accomplish this. For each state and mailing decision, we observe from the historical data the proportion of times customers transitioned to each of the other states.

[14] We measure distance using the L2 norm on predicted probability vectors.

Care must be taken if the training data is based on historical data. The observed variation in outcomes across different actions (within a state) may not be causal. For this reason, it is a standard practice to train these targeting models using data from randomized experiments.

Estimating the Value of Each State for a Given Policy

This step is normally called "Policy Evaluation" in the Reinforcement Learning literature. In our baseball example in the Introduction, the number of expected runs in States 1, 2 and 3 were calculated as a simple average of the number of runs scored from the states in past Major League Baseball games. However, these outcomes depend upon how the game will be played in future states. In particular, the value function under an arbitrary policy $\pi(s)$ can be written as:

$$V^{\pi}(s) = E_{r,s'}\left[r\big(s,\pi(s)\big) + \delta V^{\pi}(s')\big|s,\pi(s)\right] \qquad \forall s \in S.$$

We adopt the notation introduced in Section 3. Suppose we use $\bar{r}(s,a)$ to represent the expected rewards in state s when the firm chooses mailing action a, then we can rewrite the value function as:

$$V^{\pi}(s) = \bar{r}\big(s,\pi(s)\big) + \delta \textstyle\sum_{s'} p\big(s'\big|s,\pi(s)\big)V^{\pi}(s').$$

With a slight modification of notation, we can express the above equation in vector form. Let $\boldsymbol{V^{\pi}}$ denote the vector with elements $V^{\pi}(s)$, $\boldsymbol{\bar{r}^{\pi}}$ with elements $\bar{r}\big(s,\pi(s)\big)$ and $\boldsymbol{p^{\pi}}$ (a matrix) with elements $p\big(s'\big|s,\pi(s)\big)$. Thus, $\boldsymbol{V^{\pi}} = \boldsymbol{\bar{r}^{\pi}} + \delta\boldsymbol{p^{\pi}}\boldsymbol{V^{\pi}}$, and we can obtain the value of each state under an arbitrary policy from $\boldsymbol{V^{\pi}} = (\boldsymbol{I} - \delta\boldsymbol{p^{\pi}})^{-\boldsymbol{1}}\boldsymbol{\bar{r}^{\pi}}$.

To estimate $V(s)$, we also need to estimate the one-period expected rewards $\bar{r}(s,a)$ for each state-action pair. Similar to estimating the transition probabilities, we estimate the one-period rewards using a simple nonparametric approach; we calculate the average one-period reward for each state and mailing decision.

Choosing the Optimal Policy

In our baseball example, the runner chooses the optimal action in state 1 using the value function estimates associated with the two state-action pairs in state 1: the runner only steals if 0.5088 < 0.6675 * β + 0.0986 * (1 - β). When there are many states, for any given policy we can calculate the value function for each state-action pair. We can then improve the policy by choosing the action with the highest value in each state.

Notice that when we change the policy (the "Policy Improvement" stage) this recursively changes the value function estimates for each state-action pair, and so we must then re-evaluate the improved policy (the "Policy Evaluation" stage). We can then try to further improve the policy. This is the idea behind the classical policy-iteration algorithm, which we use in this paper to find the optimal policy. If the current policy already chooses the action with the highest value in each state, then the current policy is optimal.

In practice, the policy-iteration algorithm starts with any arbitrary policy for which we estimate the value function. We use this value function to improve the policy, which yields a new policy with which to begin the next iteration. The sequence of policies improves monotonically until the current policy is optimal. The policy-iteration algorithm is guaranteed to return a stationary policy that is optimal for a finite state MDP (Bertsekas, 2017). However, this guarantee only holds if the states satisfy the Markov property.

In the next section we validate our proposed method for constructing states by investigating whether it is robust to the introduction of non-Markov distortions in the data.

7. Validation

We start by describing our data setting and validation approach. The first set of results investigate the robustness of our method to non-Markov distortions in the state space used to generate the data. We then investigate the impact of errors in the estimated (conditional) probabilities. Finally, we evaluate the proposed approach using model-free field data.

Data

All of the validation results (both simulation and model-free) are based on email experiments provided by a large company. We first briefly summarize these experiments. We use historical transaction data for customers involved in a sequence of six experiments. The experiments were all designed to cross-sell new products to existing customers by advertising the benefits of the new products. In each experiment, customers were randomized into two experimental conditions. Customers in the treatment condition received an email advertisement, while customers in the control condition did not. The randomization was conducted at the individual customer level. We summarize the sample size in each of the six experiments in Table 2.

There were 136,262 customers that participated in all six experiments. We will restrict attention to these 136,262 customers when we create the primitives for our simulated data and generate the model-free results. The outcome from each experiment was measured by calculating the profit earned in the 90 days after the mailing date less mailing costs. The distribution of outcomes includes a large mass of customers from which the firm received no revenue. For these customers, the profit was either zero or negative, depending upon whether they were in the Treatment condition (in which case the firm incurred a mailing cost). There was also a long tail of outcomes, with a handful of customers contributing a very large amount of revenue.

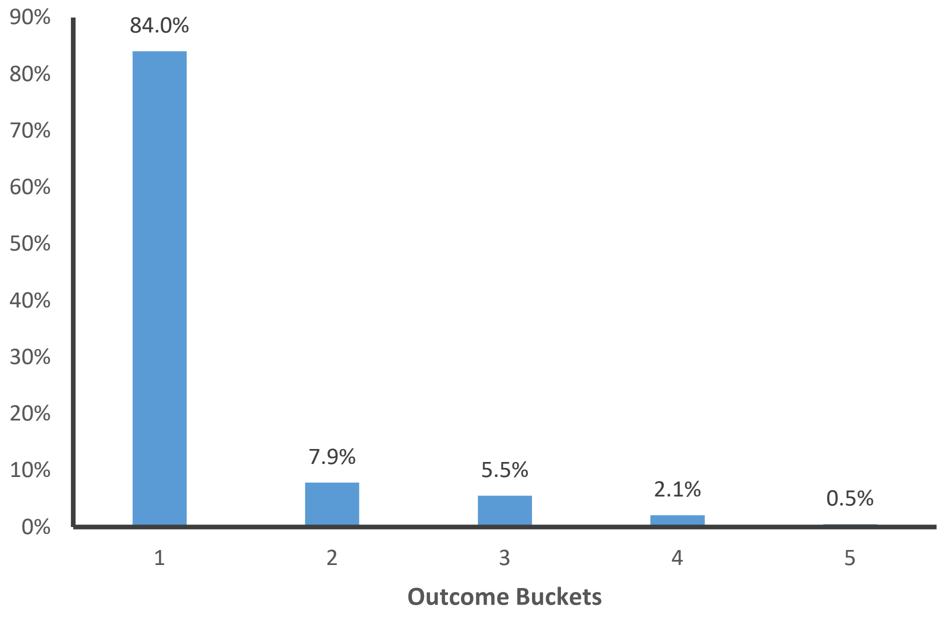
Table 2. Sample Sizes in Six Experiments

Experiment	1	2	3	4	5	6
Treatment	68273	68176	67797	68145	85609	69998
Control	67989	68086	68465	68117	50653	66264

Notes. The table reports the number of customers in the treatment and control groups in each experiment. The experiments are numbered in the sequence they were implemented (Experiment 1 was first).

We summarize the distribution of outcomes by grouping the outcomes into five discrete buckets. The first bucket included the customers for whom profit was zero or negative. The customers in the next four outcome buckets all contributed positive profits. Because the profit levels are confidential we do not disclose the cutoff limits. In Figure 2 we report a histogram of the customer outcomes across the six experiments. The unit of observation is a customer in an experiment and the columns add to 100%. We see from Figure 2 that in any period many customers do not purchase. This is the "sparsity" that our method utilizes.

Figure 2. Histogram of Customer Outcomes in Each Experiment



Notes. The figure reports a histogram of the outcomes from each of the six experiments (the columns add to 100%). The outcomes are grouped into five discrete buckets and we report the proportion of observations in each bucket. The unit of analysis is a customer in an experiment and the sample sizes are reported in Table 2.

Validation Approach

We design a validation environment in which any error in the design of the state space is solely attributable to non-Markov distortions that we control. We start by designing a state space that we know is Markov. We then introduce two types of distortions; distortions in either the current period rewards, or distortions in the transition probabilities.

To accomplish this, we need to know the ground truth of the data. For this reason, we use simulated data, which is constructed based on the actual email experiments. Consistent with the discretization of the outcomes in each time period, we consider an MDP with five states and two actions: $s = \{1,2,3,4,5\}$ and $a = \{0,1\}$. The reward $R^M: S \times A \rightarrow \mathbb{R}$, transition probabilities $P^M: S \times A \rightarrow S$ and policy $\pi^M: S \rightarrow A$ to generate the MDP are all constructed from the actual experimental data.

In particular, the rewards are the midpoint of the profit earned in each of the discrete outcome buckets. If the outcome in period *t* corresponds to the third outcome bucket, the rewards for that customer in period *t* is equal to the midpoint of the rewards in that bucket. The transition probabilities for each state-action pair are calculated by counting the proportion of times a customer in that state that received that action (mail or not mail) transitioned to each of the other state in period *t* + 1. Because the actions (mail and not mail) were randomized, the policy is a stochastic policy, and reflects the percentage of times a customer in that state received each action. We describe this procedure in more detail in the Appendix, using an example.

The non-Markov deviations in the transition probabilities are constructed as follows:

- With probability α, the state at time period *t* is determined by the state at time period *t* - 2: $P^M(s_t|s_{t-2}, a_{t-1})$; and
- With probability 1 - α, the state at time period *t* is determined by the state at time period *t* – 1: $P^M(s_t|s_{t-1}, a_{t-1})$.

With probability 1 – α the transition probabilities only depend upon the state in period *t* - 1 and so the states $s = \{1,2,3,4,5\}$ satisfy the Markov property. However, with probability α the transition probabilities depend upon the state in period *t* - 2 and so the states $s = \{1,2,3,4,5\}$ do not satisfy the Markov property.

To introduce non-Markov deviations in the rewards we use a similar approach:

- With probability α, the reward at time period *t* is determined by the state at time period *t* - 1: $(\frac{R^M_{t-1}(s_{t-1},1)+R^M_{t-1}(s_{t-1},0)}{2})$; and
- With probability 1 - α, the state at time period *t* is determined by the state at time period *t* : $R^M_t(s_t, a_t)$.

We present results using data generated under different values of α. In particularly, we allow α to vary from 0 to 0.9 in increments of 0.1, and separately evaluate deviations in the rewards and the transition probabilities. For each value of α, we simulate a dataset with 200,000 customers and 8 time periods.

We compare two state space designs:

Benchmark the five states in the MDP: $s = \{1,2,3,4,5\}$

Our Method states generated using the method proposed in this paper.

Because the Benchmark state space uses the five states in the data-generating MDP, we can isolate the effects of non-Markov deviations on the value function estimates. If we use other methods to generate states, we cannot isolate whether errors are due to the non-Markov distortions, or from other failings in the state space design. We expect that the performance of the five Benchmark states should worsen as the parameter α increases due to the introduction of the non-Markov distortions. In contrast, the states generated by the method proposed in this paper should be robust to non-Markov distortions, and so the results should be stable when α increases.

Validation Results Using True Probabilities

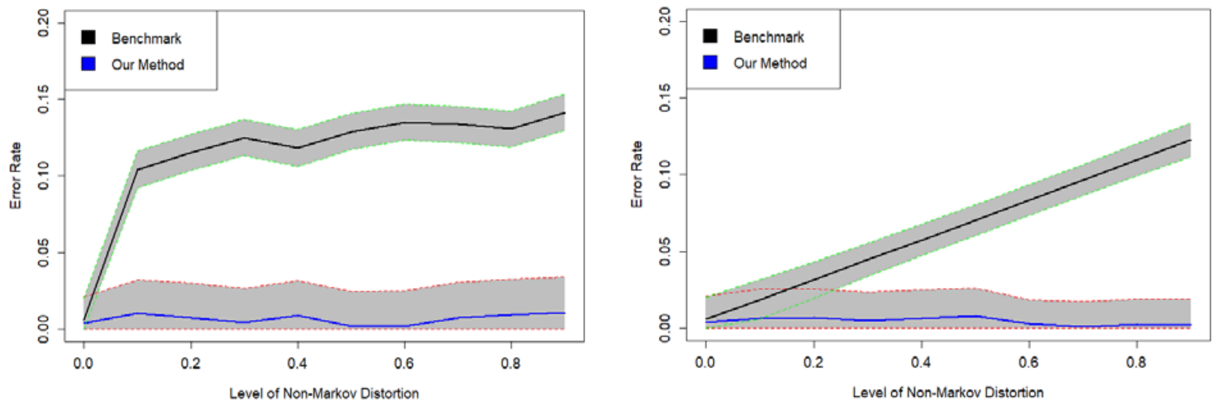
We first present results using the true probabilities used to generate the simulated data. We also initially focus on the "current policy" reflected in the data (recall that this policy was randomized). We will later also investigate how well the state space designs perform when they are used to design "optimal" policies.

In Figure 3 we report the error rates of the estimated value functions. For both state space designs, we weight the value function estimates for each state by the number of visits to each state in the data. Our error measure is the absolute difference in the average performance compared to the true value function estimate. In the datasets with deviations, the true value function estimate is calculated using a state space with 25 states to account for whether the transitions (or rewards) depend upon period *t* -1 (*t*) or period *t* – 2 (*t* -1) (this is a 2-history state space). Standard deviations are estimated using the method proposed in Mannor et al., (2007). The shaded areas in the figure identify 95% confidence intervals. Additional details, including the true value function estimates for the different values of α, are provided in the Appendix.

In Figure 3 we see that, without any deviations α = 0, both state spaces perform well and yield value function estimates for the current policy that are close to the true values. However, with the introduction of distortions in the transition probabilities α > 0, there is an immediate and large deterioration in the accuracy of the benchmark state space. The reduction in accuracy is dramatic even for α = 0.10. This deterioration is completely attributable to the non-Markov distortions, and highlights the cost of breaching the Markov assumption in the design of state spaces. The performance continues to deteriorate as the size of the distortions increases beyond α = 0.10. In contrast, while there is some volatility, the overall performance of the proposed method is relatively robust to the non-Markov dynamics in the data.

Figure 3. Error Rates Under the Current Policy Using the True Probabilities

(a) Deviations in Transition Probabilities **(b) Deviations in Rewards**



Notes. The figures report the error rates of value function estimates under the current policy for both the benchmark state spaces and the PSR states produced by our proposed method using the true probabilities $p(q|h)$. In panel (a) the x-axis is the level of non-Markov distortion in the transition probabilities (measured by α). In panel (b) the x-axis is the level of distortion in the rewards. The performance measure is the average absolute deviation from the true performance of the current policy. Additional details are provided in the Appendix.

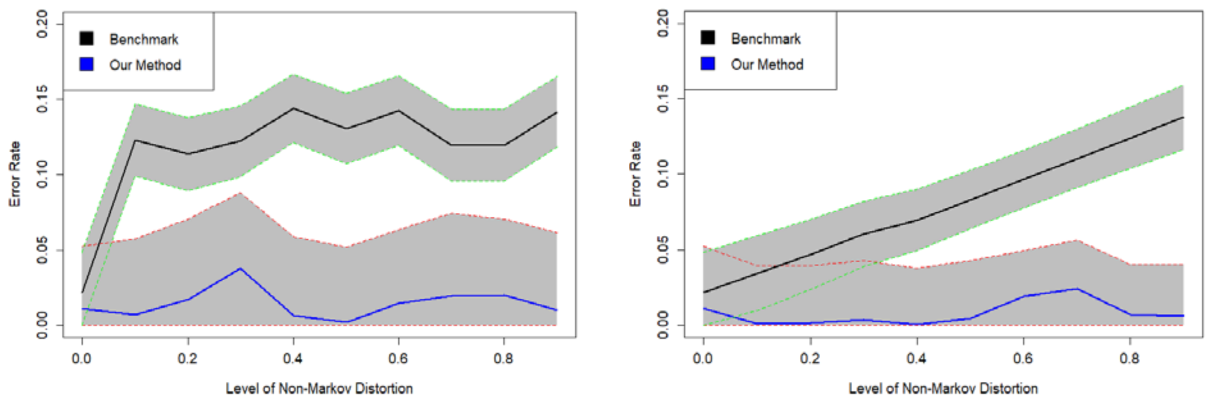
We observe a similar pattern when we investigate deviations in the rewards. However, an important difference is that the performance of the benchmark method decreases linearly with increases in the non-Markov distortions (measured by α). The reason for this is that the reward matrix enters the value function estimates in linear form. When we vary α, there is α probability of getting errors in the reward matrix. As a result, the accuracy of the Benchmark state space decreases linearly.

In Figure 3 we focused on the policy represented in the data. We can also investigate the performance of the "optimal" policies designed using each state space. These results are reported in Figure 4. An additional issue arises when comparing value function estimates for an optimized policy. The choice of the optimal action in the Policy Improvement step tends to favor positive errors. This issue is discussed in detail in Mannor et.al, (2007). We use the cross-validation approach proposed in Mannor et.al, (2007) to de-bias the estimates of the value function. In particular, we divide the training data into two subsamples: calibration data and validation data. We use the calibration sample to estimate the rewards and transition probabilities and identify the optimal policy. We then evaluate this optimal policy using the validation sample. The independence of the errors between the two samples de-biases the value function estimates.

We weight the value function estimates by the frequency of the states in the data. Our performance measure is the average absolute deviation from the true performance of the true optimal policy.

Figure 4. Error Rates Under the Optimal Policy Using the True Probabilities

(a) Deviations in Transition Probabilities **(b) Deviations in Rewards**



Notes. The figures report the error rates of value function estimates under the optimal policy for both the benchmark state spaces and the PSR states produced by our proposed method using the true probabilities $p(q|h)$. In panel (a) the x-axis is the level of non-Markov distortion in the transition probabilities (measured by α). In panel (b) the x-axis is the level of distortion in the rewards. The performance measure is the average absolute deviation from the true performance of the optimal policy. Additional details are provided in the Appendix.

When optimizing the firm's mailing policy, our proposed method is again robust to non-Markov distortions in both transition probabilities and rewards. Notice that the standard deviations are larger in Figure 3 than in Figure 4. This is because we only use half of the data (validation data) to evaluate the optimal policy. If we increase the size of the validation data to match Figure 3, the confidence intervals shrink to a similar size.

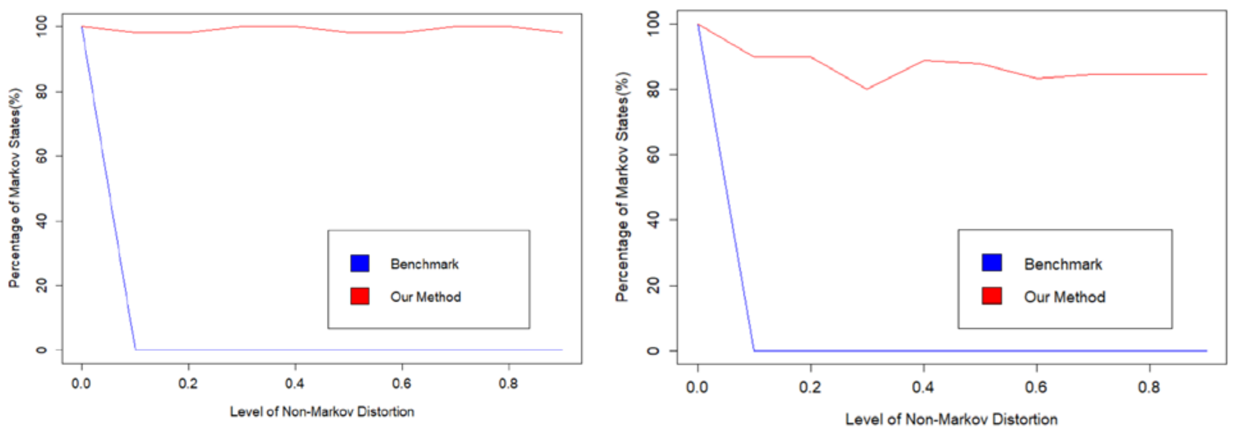
Are the States Markov?

We can also use the test proposed in Section 4 to calculate the percentage of states that satisfy the Markov property. Recall that a necessary (though not sufficient) condition for satisfaction of the Markov property is that a state satisfies the following conditional independence test: for all observations in the state, the probability of o_t conditional on a_t is independent of h_t. The proportion of states that satisfy this test under the different values of α are reported in Figure 5.[15] We compare our proposed method with states generated using the benchmark state space.

[15] To minimize finite sample errors, we restrict attention to histories of length 2.

Figure 5. Proportion of States that Satisfy the Markov Property

(a) Deviations in Transition Probabilities **(b) Deviations in Rewards**



Notes. The figures report the proportion of states that satisfy the Markov property for the benchmark state space and the PSR states generated by our proposed method using true probabilities. In panel (a) the x-axis is the level of non-Markov distortion in the transition probabilities (measured by α). In panel (b) the x-axis is the level of distortion in the rewards. Additional details are provided in the Appendix.

Under all of the values of α, essentially all of the states constructed using our proposed method satisfy the Markov property. In contrast, once we introduce non-Markov distortions, none of the states constructed using the benchmark method satisfy the Markov property (by construction).

Effect of Estimated Probabilities

In the previous subsection, we evaluated our proposed method using the true probabilities that we used to generate the simulated data. This is equivalent to estimating the results using a dataset with a very large (infinite) number of observations. In practice, our sample sizes are often small enough to introduce estimation errors in the (conditional) probabilities. In this subsection, we explore the effect of these estimation errors on the performance of the proposed method.

We start by investigating the variation in probability estimates calculated from our field experiment data. In particular, we use the sampling method described in Section 5 to estimate the conditional probabilities in the system dynamic matrix 50 times. For each entry, we derive the average, minimum and maximum value across these 50 samples. We use the absolute difference between the average and maximum (minimum) as the positive (negative) estimation errors for each entry. The average (weighted) positive estimation error is approximately 38%, while the average negative error is approximately 4%. We use these two numbers to guide the magnitude of the errors we will introduce to the true probabilities in our simulated data.

To introduce errors, we allow a proportion of entries in the system dynamic matrix to deviate from their true probabilities. We vary the proportion of deviated entries from 10% to 50%, in 10% increments: $\tau \in$

{0.1,0.2,0.3,0.4,0.5}. For each deviation, there is a 50% probability of a positive deviation (38%), and a 50% probability of a negative deviation (-4%). Recall that these are the largest observed deviations in our sample of 50 draws from the field data, and so these represent relatively large distortions.

Recall that our proposed approach uses the conditional probabilities in two stages when designing the state space. It first uses them to design the core tests, and then after identifying the core tests, it uses them to group customers into states (according to the conditional probabilities associated with these core tests). To decompose how errors in these probabilities affect the performance of the method, we separately introduce the distortions just when designing the core tests, just when grouping customers into states, or in both stages. We report the findings in Table 3.

Table 3. Effect of Estimated Probabilities

Proportion (τ)	Current Policy (%)			Optimal Policy (%)		
	Designing Core Tests	Grouping Customers	Both	Designing Core Tests	Grouping Customers	Both
0.1	3.19	9.10	14.58	2.49	0.47	16.18
0.2	3.19	4.83	37.05	2.49	0.77	27.14
0.3	3.19	8.50	54.41	2.49	15.34	45.87
0.4	3.19	5.47	61.57	2.49	1.28	50.27
0.5	3.19	2.09	64.92	2.49	7.89	59.89

Notes. The table reports the error rates of value function estimates for the PSR states produced by our proposed method when introducing errors in the conditional probabilities. Across the different rows in the table we vary the proportion of entries in which we introduce error (τ). The second and fifth column report results where we only use deviated probabilities in finding core tests. The third and the sixth column report results where we only use deviated probabilities in grouping customers into different states. The fourth and the seventh column report results where we use deviated probabilities in both stages.

We obtain several insights from Table 3. First, our core-test finding algorithm proposed in Section 5 is robust to errors in the conditional predicted probabilities. This can be seen from the stability of the results when varying the estimation errors in designing core tests. Second, when we introduce estimation errors in either designing core tests or grouping customers, the performance of the proposed method does not dramatically worsen. However, when we introduce estimation errors in both stages, the estimation errors in each stage reinforce each other. The reason is that our algorithm finds more core tests than the true set; recall that estimation errors in the conditional probabilities can make linearly independent columns appear dependent. When we identify additional core tests, there are more conditional probabilities to estimate. If these probabilities are estimated with error, this amplifies the errors in the system. As a result, the combination of too many core tests and errors in the probabilities used to group customers undermines the performance of the method.

Notice also that we do not observe any changes in the error rates when we only introduce probability errors in the designing of core tests. The example in the Appendix can help to understand why. Estimation errors can easily make dependent columns become independent which will enlarge the number of independent columns. However, it is much less likely that errors will make the linearly independent columns become dependent. Thus, the errors simply increase the size of the core test set, but this has little impact on performance.

In Table 3 we evaluate the impact of varying how *frequently* there are errors in the estimates of the conditional probabilities. The performance of the algorithm may also depend upon the *magnitude* of the errors. Therefore, as a robustness check, we repeated this analysis when varying the magnitudes instead of the frequencies of the estimation errors. These findings are reported in the Appendix. They reveal a similar pattern of findings.

These findings suggest that the performance of the proposed method depends upon having sufficient data to reduce errors in the estimation of the conditional probabilities. We investigate this issue next using the raw data from the field experiments.

Model-Free Evidence

In this final sub-section, we step away from the simulated data, and use the field experiment data to provide model-free evidence of the performance of the method. Because we only have a sequence of six experiments to work with, the evidence is admittedly preliminary. However, we will use the six experiments to evaluate how effective our proposed method is at designing a state space that summarizes the information in a short history of historical data. In particular, we use two periods of history to predict two, three or four periods of future outcomes.

When selecting benchmarks methods, we restrict attention to other tabular RL models, and hold Steps 2 through 4 constant across all the methods (see the four steps listed in the Introduction). The benchmark methods only vary in how they construct state spaces in Step 1.[16] In particular, we compare our method with three alternative methods for designing state spaces.

The first alternative state space design method translates a customer's complete two period history into a state space, with no loss of information. In particular, we have two actions and five possible outcomes. Therefore, we can fully represent all of the possible 2-period combinations of actions and outcomes using

[16] This is consistent with our validations using simulated data earlier in this section.

$(2 * 5)^2 = 100$ states. This represents an upper bound on the design of the state space, but it is not scalable. The complete history state space is only practical if the history is short and the range of outcomes is small, so that the number of states is small relative to the size of the training sample. In this case, we have only 100 states and will use a training sample of 100,000 customers, and so we can confidently interpret the performance of the complete history state space as an upper bound. We will evaluate how closely our method comes to this benchmark.

The second alternative state space design method is motivated by the RFM method, which is widely used in industry to segment customers for direct mail and email campaigns (see for example Bult and Wansbeek 1995; and Bitran and Mondschein 1996). The RFM method groups customers according to the Recency, Frequency and Monetary value of each customer's historical transactions. "Recency" measures the number of periods since the customer's last purchase. "Frequency" measures the number of prior purchases. "Monetary value" is the average value of the prior purchases. We describe how we design the RFM states in greater detail in the Appendix (including an example).

The final alternative state space design uses only a single state. This is equivalent to a static model that does not condition on history. Instead, the state-action rewards each period are just the average under the mail and no-mail treatment conditions. We include this benchmark simply to see how much the other state spaces improve predictions by conditioning on history and incorporating dynamics.

We use the following four-step procedure to evaluate the performance of different state-design methods:

1. We randomly split the 136,262 customers into two sub-samples of 100,000 and 36,262 customers. We use the 100,000 customer sub-sample as training data and the 36,262 sub-sample as validation data.
2. We use the training data to implement each state-design method. For example, under our method, we use the training data to identify core tests, group customers using probabilities on these core tests, and calculate rewards and transitions for each state-action pair.
3. For each customer in the validation data, we identify a length-T observation ($T = 2, 3, 4$), and calculate the actual observed profit for each customer in this length-T observation.[17]

[17] Specifically, for the length-4 observations, we use the first two experiments (actions and outcomes) to locate customers in the state space, and use the outcomes from the next four experiments to calculate the actual profit. For the length 3-observations we randomly select either Experiment 1 and 2 or Experiments 2 and 3 as the two-period history. The associated length-3 profits are then calculated either from Experiments 3, 4, and 5, or Experiments 4, 5 or 6 (respectively). For the length-2 observations we randomly select from the first four experiments to start the 2-period history, and then use the subsequent two experiments to calculate the length-2 profits. In all cases we use the log of the midpoint of the profit in each of the five profit buckets. This implicitly interprets the treatment effects from each experiment as a proportional (rather than additive) effect.

4. For each customer, we compare the actual length-T outcome with the length-T outcome predicted under each state-design method, and calculate the mean absolute error as a percentage of the actual value.

The procedure is designed to evaluate how well each state space summarizes the information in the two-period raw transaction history that is relevant to the prediction of the length-T profits. We report the findings in Table 4.

Table 4. Model-Free Evidence

Mean Absolute Percentage Error of Length-*T* Profit Predictions

Profit Length (T)	Proposed Method	Full History	RFM	Single State
2	86.41% (0.80%)	86.33% (0.79%)	89.17% (0.79%)	165.49% (1.14%)
3	86.50% (0.81%)	86.42% (0.81%)	88.97% (0.82%)	158.66% (1.05%)
4	85.17% (0.86%)	85.08% (0.86%)	87.97% (0.86%)	151.32% (0.99%)

Notes. The table reports the average absolute error between the actual and predicted length-T profits for the 36,262 customers in the validation sample ($T = 2,3,4$). The number of customers used in the training data is 100,000. Bootstrapped standard errors are in parentheses.

We see that the performance of our proposed method is very close to the complete history state space. Moreover, across different profit lengths, our method consistently out-performs the RFM states. This suggests that the states designed by our method do a good job of summarizing the relevant information in the raw transaction data. However, as we acknowledged at the start of this section, this validation should be viewed as preliminary. A more thorough model-free validation of the method would require a much longer sequence of experiments (using a common sample of customers). Despite this limitation, the performance of our proposed method is reassuring.

As a robustness check, we also repeated the analysis when using smaller subsets of the training data (these findings are reported in the Appendix). There was little difference in the performance of any of the methods, including the full history states. This reinforces our interpretation that the performance of the full history state space is an upper bound. Even with as few as 20,000 customers in the training data there appears to be sufficient information to accurately calculate the transition probabilities and rewards in the full history model.

Finally, we note that the error rates tend to decrease for all methods as the profit length increases. This presumably reflects the aggregation of positive and negative errors when measuring performance over a longer sequence of outcomes. Although not surprising, this is relevant to the theoretical guarantees on the performance of our proposed method. The guarantees assume an infinite horizon. Given that we only have finite-period data, even with the true state-space design (e.g. the full history state space) the error rates can be large.

8. Conclusions and Limitations

In this paper, we study the problem of optimizing a sequence of marketing actions. We highlight the importance of constructing states that satisfy the Markov property when using traditional Reinforcement Learning methods, and propose a method for designing states that satisfy this requirement. Our findings can be used with traditional Reinforcement Learning methods, or with related methods such as POMDPs and Approximate Dynamic Programming techniques.

The method begins with the insight that states that satisfy the Markov property contain the same information about the probability of future outcomes as the raw training data. By constructing predictions over how customers will respond to a firm's marketing actions, and grouping customers together if their predicted behavior is similar, we can guarantee that the resulting states will be Markov. There is no obvious way to obtain the same guarantee if we instead group customers directly according to the similarity of their past transactions (rather than grouping them based on the probabilities of future events).

While this approach is theoretically appealing, a practical challenge is that there is an infinite combination of future customer behaviors (future events) to predict. One of our contributions is to recognize that transactions by individual customers are often relatively sparse. This can greatly reduce the range of future events that we need to consider, and considerably lower the computational burden. We turn a problem that is often impractical, into a problem that is relatively straightforward to solve in many marketing settings.

We validate the proposed method by showing that it is robust to non-Markov distortions in the data-generating process. In contrast, state spaces that do not adjust for these distortions suffer immediate performance losses, even when the distortions are small.

We see three important limitations in our proposed method. First, our model-free validation only has access to a sequence of six experiments. As we explain in Section 7, a more thorough model-free validation of the method would require a much longer sequence of experiments (using a common sample of customers). A second limitation is that the method is designed for batch learning, where the training data is fixed and already available. It is not clear how the method could be used for online learning, because incremental data could lead to changes in the design of the state space. The third limitation is scalability. As the number of observed time periods increases, the length of the core tests grows, and the algorithm we use for finding core tests faces increasing computational challenges. Similarly, as the size of the action space grows, the number of core tests will increase. As a result, the number of state-action pairs will grow due to both more actions and more states. We see this as an important direction for future research. There are two directions that might help to improve the scalability of the algorithm. First, we can try to exploit methods in computer science designed to handle high-dimensional data. Deep learning, transfer learning, and spectral learning, are all methods that can be considered. Second, we can use more domain knowledge to simplify the problem. Currently, we exploit the sparsity in the customer transaction data. However, it is likely that there are other ways to further improve the use of sparsity in the algorithm.

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