Endogenous Macrodynamics in Algorithmic Recourse

***Abstract*—Existing work on Counterfactual Explanations (CE) and Algorithmic Recourse (AR) has largely been limited to the static setting: given some classifier we are interested in finding close, actionable, realistic, sparse, diverse and ideally causally founded counterfactuals. The ability of CE to handle dynamics like data and model drift remains a largely unexplored research challenge at this point. Only one recent work considers the implications of exogenous domain and model shifts. This project instead focuses on endogenous dynamics, that is shifts that occur when AR is actually implemented by a proportion of individuals. Early findings suggest that the involved shifts may be large with important implications on the validity of AR and the overall characteristics of the sample population.**

I. Introduction

Recent advances in Artificial Intelligence (AI) have propelled its adoption in scientific domains outside of Computer Science including Healthcare, Bioinformatics, Genetics and the Social Sciences. While this has in many cases brought benefits in terms of efficiency, state-of-the-art models like Deep Neural Networks (DNN) have also given rise a new type of principal-agent problem in the context of data-driven decision-making. It involves a group of **principals** - i.e. human stakeholders - that fail to understand the behaviour of their **agent** - i.e. the model used for automated decision-making [1].

Models or algorithms that fall into this category are typically referred to **black-box** models. Despite their shortcomings, black-box models have grown in popularity in recent years and have at times created undesirable societal outcomes [2]. The scientific community has tackled this issue from two different angles: while some have appealed for a strict focus on inherently iterpretable models [3], others have investigated different ways to explain the behaviour of black-box models. These two sub-domains can be broadly referred to as **interpretable AI** and **explainable AI** (XAI), respectively.

Among the approaches to XAI that have recently grown in popularity are **Counterfactual Explanations** (CE). They explain how inputs into a model need to change for it to produce different outputs. Counterfactual Explanations that involve realistic and actionable changes can be used for the purpose of **Algorithmic Recourse** (AR) to help individuals who face adverse outcomes. An example relevant to the Social Sciences is consumer credit: in this context AR can be used to guide individuals in improving their creditworthiness, should they have previously been denied access to credit based on an automated decision-making system. A meaningful recourse recommendation for a denied applicant could be: *"If your net savings rate had been 10% of your monthly income instead of the actual 8%, your application would have been successful. See if you can temporarily cut down on consumption."* In the remainder of this paper we will use both terminologies - recourse and counterfactual - interchangeably to refer to situations where counterfactuals are generated with the intent to provide individual recourse.

Existing work in this field has largely worked in a static setting: various approaches have been proposed to generate counterfactuals for a given individual that is subject to some pre-trained model. More recent work has compared different approaches within this static setting [4]. In this work we go one step further and ask ourselves: what happens if recourse is provided and implemented repeatedly? What types of dynamics are introduced and how do different counterfactual generators compare in this context?

Figure 1 illustrates this idea for a binary problem involving a probabilistic classifier and the counterfactual generator proposed by [5]: the implementation of AR for a subset of individuals leads to a domain shift (b), which in turn triggers a model shift (c). As this game of implementing AR and updating the classifier is repeated, the decision boundary moves away from training samples that were originally in the target class (d). We refer to these types of dynamics as **endogenous** because they are induced by the implementation of recourse itself.

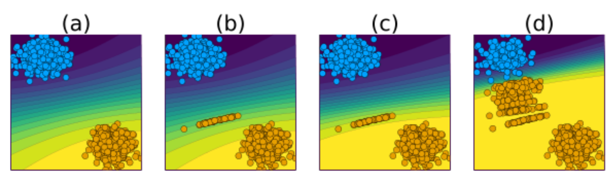


Figure 1: Dynamics in Algorithmic Recourse: we have a simple Bayesian model trained for binary classification (a); the implementation of AR for a random subset of individuals leads to a domain shift (b); as the classifier is retrained we observe a model shift (c); as this process is repeated, the decision boundary moves away from the target class (d).

We think that these types of endogenous dynamics may be problematic. Firstly, model shifts may inadvertently change classification outcomes for individuals who never received and implemented recourse. Secondly and relatedly, we observe in Figure 1 that as the decision boundary moves in the direction of the non-target class, counterfactual paths become shorter: in the consumer credit example, individuals that previously would have been denied credit based on their input features are suddenly considered as creditworthy. Average default risk across all borrowers can therefore be expected to increase. Conversely, lenders that anticipate such dynamics may choose to deny credit to individuals that have implemented AR, thereby compromising the validity of AR.

To the best of our knowledge this is the first work investigating endogenous dynamics in AR. Our contributions to the state of knowledge are two-fold. Firstly, we introduce an experimental framework extending previous work by [4], which allows for benchmarking different counterfactual generators in terms of the endogenous domain and model shifts they introduce. To this end we propose a number of novel evaluation metrics. Secondly, we use this framework to provide the first in-depth analysis of endogenous recourse dynamics induced by various popular counterfactual generators including [5], [6], [7] and [8].

We find that …

The paper is structured as follows: Section II places our work in the broader context of related literature. Section III presents our methodology and data. Section IV presents our empirical findings which are then discussed in Section V. We also point to some of the limitations or our work as well as avenues for future research in Section V-A. Finally, Section VI concludes.

II. Related Work

In this Section we provide a review of the relevant literature. First, Section II-A discusses the existing research within the domain of counterfactual explanations and algorithmic recourse. Then, Section II-B presents some of the previous work on the measurement of dataset and model shifts.

A. Algorithmic Recourse

A framework for Counterfactual Explanations was first proposed in 2017 by [5] and has served as the baseline for most methodologies that have been proposed since then. Let $M : \mathcal{X} \mapsto \mathcal{Y}$ denote some pre-trained model that maps from inputs $X \in \mathcal{X}$ to outputs $Y \in \mathcal{Y}$. Then we are interested in minimizing the complexity or effort $H = h(x')$ associated with moving an individual x to a counterfactual state x' such that the predicted outcome $M(x')$ corresponds to some target outcome t:

$$\min_{x' \in x} c(x') \quad \text{s. t.} \quad M(x') = t \tag{1}$$

For implementation purposes, Equation 1 is typically approximated through regularization:

$$x' = \arg\min_{x'} \ell(M(x'), t) + \lambda h(x') \tag{2}$$

In the baseline work and many subsequent approaches the complexity function $h : \mathcal{X} \mapsto \mathbb{R}$ is proxied by some distance metric based on the simple intuition that large perturbations of x are costly.

Many approaches for the generation of algorithmic recourse have been described in the literature. An October 2020 survey by Karimi et al. layed out 60 algorithms that have been proposed since 2014 [9]. Another survey published around the same time by Verma et al. described 29 algorithms [10]. Different approaches vary primarily in terms of the objective functions they impose, how they optimize said objective (from brute force through gradient-based approaches to graph traversal algorithms), and how the ensure that certain requirements for CE are met. Regarding the latter, the literature has produced an extensive list of desiderata each addressing different needs. To name but a few, we are interested in generating counterfactuals that close [5], actionable [11], realistic [12], sparse, diverse [7] and if possible causally founded [13].

Efforts so far have largely been directed at improving the quality of counterfactual explanations within a static context: given some pre-trained classifier $M : \mathcal{X} \mapsto \mathcal{Y}$ we are interested in generating one or multiple meaningful counterfactual explanations for some individual characterized by x_i. The ability of counterfactual explanations to handle dynamics like data and model shifts remains a largely unexplored research challenge at this point [10]. We have been able to identify only one recent work that considers the implications of **exogenous** domain and model shifts in the context of AR [14]. Exogenous shifts are strictly of external origin. For example, they might stem from data correction, temporal shifts or geospatial changes [14]. The authors of [14] propose framework for algorithmic recourse (ROAR) that evidently improves robustness to such exogenous shifts.

B. Domain and Model Shifts

Much attention has been paid to the detection of dataset shifts – situations where the distribution of data changes over time. Rabanser et al. suggest a framework to detect data drift from a minimal number of samples through the application of two-sample tests [15]. This task is a generalization of the anomaly detection problem for large datasets, which aims to answer the question if two sets of samples could have been generated from the same probability distribution. Numerous approaches to anomaly detection have been summarized [16]. Another well-established research topic is that of concept drift – situations where external variables influence the patterns between the input and the output of a model [17]. For instance, Gama et al. offer a review of the adaptive learning techniques which can handle concept drift [18]. Less previous work is available on the related topic of model drift - changes in model performance over time. Nelson et al. review how resistant different machine learning models are to the model drift [19]. Ackerman et al. offer a method to detect changes in model performance when ground truth is not available [20].

In the context of algorithmic recourse, domain and model shifts were first brought up by the authors behind ROAR [14]. In their work they refer to model shifts as simply any perturbation Δ to the parameters of the model in question: M. While this also sets the baseline for our analysis here, it is worth noting that in [14] these perturbations are mechanically introduced. In contrast we are interested in quantifying model shifts that arise endogenously as part of a dynamic recourse process. In addition to quantifying the magnitude of shifts Δ, we aim to also analyse the characteristics of changes to the model, such as the position of the decision boundary and the overall decisiveness of the model. We have not been able to identify previous work on this topic.

C. Benchmarking Counterfactual Generators

Despite the large and growing number of approaches to counterfactual search there have been surprisingly few benchmark studies that compare different methodologies. This may be partially due to limited software availability in this space. Recent work has started to address this gap: firstly, [21] run a large benchmarking study using different algorithmic aproaches and numerous tabular datasets; secondly, [4] introduce a Python framework that can be used to apply and benchmark different methodology; finally, [22] provides an extensible and language-agnostic implementation in Julia. For the experiments conducted in this work we relied on the latter.

III. Methodology

In the following we first set out a generalized framework for gradient-based counterfactual search in Section III-A to introduce the various counterfactual generators we have chosen to use in our experiments. We then describe the experimental setup in Section III-B and introduce several evaluation metrics used to benchmark the different generators.

A. A Generalized Framework for Gradient-Based Counterfactual Search

In this work we have chosen to focus on a number of gradient-based counterfactual generators to investigate the endogenous dynamics we introduced in Section I. Gradient-based counterfactual search is well-suited for differentiable black-box models like deep neural networks. We can restate Equation 2 in a more general form that encompasses most gradient-based approaches to counterfactual search:

$$\mathbf{s}' = \arg\min_{\mathbf{s}' \in \mathcal{S}} \left\{ \sum_{k=1}^{K} \ell(M(f(s'_k)), t) + \lambda h(f(s'_k)) \right\} \tag{3}$$

Here $\mathbf{s}' = \{s'_k\}_K$ is the stacked K-dimensional array of counterfactual states and $f : \mathcal{S} \mapsto \mathcal{X}$ maps from the counterfactual state space to the feature space. In the case of the baseline counterfactual generator [5] f is just the idendity function and the number of counterfactuals K is equal to one. This generator, which we shall refer to as **Wachter** in the following, shall serve as the baseline against which all other gradient-based methodologies will be compared. In particular, we include include the following generator in our benchmarking exercises: REVISE [6], CLUE [8], DICE [7] and a greedy approach that relies on probabilistic models [12].

Both **REVISE** and **CLUE** search counterfactuals in some latent embedding $S \subset \mathcal{S}$ instead of the feature space directly. The latent embedding is learned by a separate generative model that is tasked with learning the data generating process (DGP) of X. In this case f in Equation 3 corresponds to the decoder part of the generative model, in other words the deterministic function that maps back from the latent embedding to the feature space. Provided the generative model is well-specified, traversing the latent embedding typically results in realistic and plausible counterfactuals, because they are implicitly generated by the (learned) DGP [6]. CLUE distinguishes itself from REVISE and other counterfactual generators in that it aims to minimize the predictive uncertainty of the model in question M. To quantify predictive uncertainty the authors rely on entropy estimates for probabilistic models. The **Greedy** approach proposed by [12] also works with the subclass of models $\tilde{\mathcal{M}} \subset \mathcal{M}$ that can produce predictive uncertainty estimates. The authors show that in this setting the complexity penalty $h(\cdot)$ in Equation 3 is redundant and meaningful counterfactuals can be generated in a fast and efficient manner through a modified Jacobian-based Saliency Map Attack (JSMA). Finally, **DICE** distinguishes itself from all other generators considered here in that it aims to generate a diverse set of $K > 1$ counterfactuals. To this end the authors use a complexity penalty $h(\mathbf{s}')$ that favours diverse outcomes, in the sense that $s_1, ..., s_K$ look as different from each other as possible.

Our motivation for including these different generators in our analysis, is that they all offer slightly different approaches to generate meaningful counterfactuals for differentiable black-box models. We hypothesize that generating more **meaningful** counterfactuals should mitigate the endogenous dynamics illustrated in Figure 1 in Section I. This intuition stems from the underlying idea that more meaningful counterfactuals are generated by the same or at least a very similar data generating process as the training data. All else equal, counterfactuals that fulfill this basic requirement should be less prone to trigger domain and model shifts.

B. Experimental Setup

The dynamics illustrated in Figure 1 in Section I were generated through a simple experiment that aims to simulate the process of algorithmic recourse in practice. We begin in the static setting at time time $t = 0$: given some pretrained classifier M we generate recourse for a random batch of B individuals in the non-target class. Note that we focus our attention on classification problems, since classification poses the most common practical use-case for algorithmic recourse. In order to simulate the dynamical process we suppose that the model M is retrained following the actual implementation of recourse in time $t = 0$. Following the update to the model, we assume that at time $t = 1$ recourse is generated for yet another random subset of individuals in the non-target class. This process is repeated for a number of time periods T. To get a clean read on endogenous dynamics we keep the total population of samples closed: we allow existing samples to move from factual to counterfactual states, but do not allow any entirely new samples to enter the population. The experimental setup is summarized in Algorithm 1

Algorithm 1 Experiment

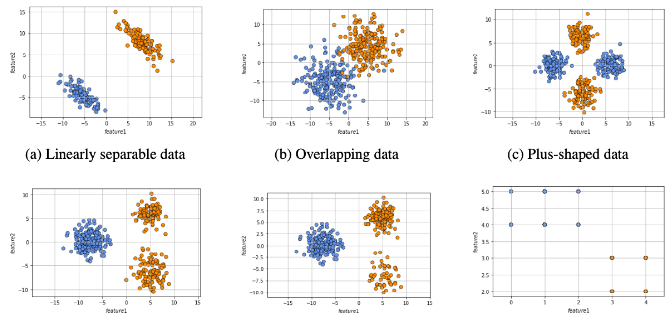
```
procedure EXPERIMENT(M, D, G)
    t ← 0
    while t < T do
        D_B ⊂ D
        D_B ← G(D_B)          ▷ Generate counterfactuals.
        M ← M(D)                      ▷ Retrain model.
    end while
    return M, D
end procedure
```

A noteworthy practical consideration is the choice of T and B. The higher these values, the more factual instances undergo recourse throughout the entire experiment. Of course, this is likely to lead to more pronounced domain and model shifts by time T. At the same time, it is generally improbable that a very large part of the population would request an explanation of the algorithm's decisions. In our experiments, we choose the values such that $T \cdot B$ corresponds to the application of recourse on $25 - 50\%$ of the negative instances from the initial dataset. As we collect data at each time t, we can also verify the impact of recourse when it is implemented for a smaller number of individuals. Using our framework the experiment can be conducted on an arbitrary number of algorithmic recourse generators. As all generators make use of the same initial model and initial dataset, the differences in domain and model shifts observed throughout the rounds depend solely on the employed generator.

C. Data

We have chosen to work with both synthetic and real-world datasets. Using synthetic data allows us to impose distributional properties that may affect the resulting recourse dynamics. Following [14] we generate synthetic data in $\mathbb{R}^2$ to also allow for a visual interpretation of the results. Real-world data is used in order to assess if endogenous dynamics also occur in higher-dimensional settings.

1) Synthetic data: We use 6 synthetic binary classification datasets consisting of 200-400 samples grouped in normally-distributed clusters.[1] The datasets are presented in Figure 2 (see also Appendix A for a formal description). Samples from the negative class are marked in blue while samples of the positive class are marked in orange.



(a) Linearly separable data (b) Overlapping data (c) Plus-shaped data
(d) Two balanced clusters (e) Two unbalanced clusters (f) Categorical data

Figure 2: PLACEHOLDER: A visualization of the synthetic classification datasets used in our experiments.

Ex-ante we expect to see that Wachter will create a new cluster of counterfactual instances in the proximity of the initial decision boundary. Thus, the choice of a black-box model may have an impact on the paths of the recourse. For generators that use latent space search ([6], [8]) or rely on (and have access to) probabilistic models ([8], [12]) we expect that counterfactuals will end up in regions of the target domain that are densely populated by training samples. Finally, we expect to see the counterfactuals generated by DiCE to be uniformly spread around the feature space inside the target class.

2) Real-world data: Additionally, we use two real-world datasets from the Finance domain. Firstly, we use the Give Me Some Credit dataset which was open-sourced on Kaggle for the task to predict whether a borrower is likely to experience financial difficulties in the next two years [23]. Originally consisting of 250,000 instances with 11 numerical attributes, the dataset was randomly undersampled to result in a balanced subsample made up of 3000 individuals. Secondly, we the German Credit dataset which involves the task of predicting if bank customers are credit-worthy or not [24]. It consists of 700 positive and 300 negative instances charaterized by 7 numerical and 13 categorical attributes. We process the dataset in two ways: (1) the values of the "Personal status and sex" feature are

[1] To see how the data is generated see here: https://github.com/pat-alt/AlgorithmicRecourseDynamics.jl/blob/main/notebooks/synthetic_datasets.ipynb

aggregated by the two represented genders; (2) the most common values are calculated for all categorical features such that a feature x_d with the mode $\bar{x}_d$ is transformed into a new binary feature $\tilde{x}_d = \mathbb{I}_{x_{d,i}>=\bar{x}_d}$. Binarization ensures that we can use all counterfactual generators in the benchmark.

D. Evaluation Metrics

We formulate two desiderata for the set of metrics used to measure domain and model shifts induced by recourse. First, the metrics should be applicable regardless of the dataset or classification technique so that they allow for the meaningful comparison of the generators in various scenarios. As the knowledge of the underlying probability distribution is rarely available, the metrics should be empirical and non-parametric. This further ensures that we can also measure large datasets by sampling from the available data. Moreover, while our study was conducted in a two-class classification setting, our choice of metrics should remain applicable in the future research on multi- class recourse problems. Second, the set of metrics should allow to capture various aspects of the previously mentioned magnitude, path, and tempo of changes while remaining as small as possible.

1) Domain Shifts: To quantify the magnitude of domain shifts we rely on an unbiased estimate of the squared population **Maximum Mean Discrepancy (MMD)** given as:

$$
\begin{aligned}
MMD_u^2[F, X', \tilde{X}'] = &\frac{1}{m(m-1)} \sum_{i=1}^{m} \sum_{j \neq i}^{m} k(x_i, x_j) \\
&+ \frac{1}{n(n-1)} \sum_{i=1}^{n} \sum_{j \neq i}^{n} k(\tilde{x}_i, \tilde{x}_j) \\
&- \frac{2}{mn} \sum_{i=1}^{m} \sum_{j=1}^{n} k(x_i, \tilde{x}_j)
\end{aligned} \tag{4}
$$

where $\mathcal{F}$ is a unit ball in a Reproducing Kernel Hilbert Space H [27], and X, $\tilde{X}$ represent independent and identically distributed samples drawn from probability distributions p and q respectively [28]. MMD is a measure of the distance between the kernel mean embeddings of p and q in RKHS $\mathcal{H}$. An important consideration is the choice of the kernel function $k(\cdot, \cdot)$. In our implementation we make use of the radial basis function (RBF) kernel with a constant length-scale parameter of 0.5. As RBF captures all moments of distributions p and q, we have that $MMD_u^2[F, X, \tilde{X}] = 0$ if and only if $X = \tilde{X}$.

The evaluation metric in Equation 4 is computed after every round $t = 1, ..., T$ of the experiment. To assess the statistical significance of the observed shifts under the null hypothesis that samples X and $\tilde{X}$ were drawn from the same probability distribution we follow [25]. To that end, we combine the two samples and generate a large number of permutations of $X + \tilde{X}$. Then, we split the permuted data into two new samples X' and $\tilde{X}'$ having the same size as the original samples. Then under the null hypothesis we should have that $MMD_u^2[F, X', \tilde{X}']$ be approximately equal to $MMD_u^2[F, X, \tilde{X}]$. The corresponding p-value can then be calculated by counting how these two quantities are not equal.

We calculate the MMD for both classes individually based on the ground truth labels, i.e. the labels that samples were assigned in time $t = 0$. Throughout our experiments we generally do not expect the distribution of the negative class to change over time – application of recourse reduces the size of this class, but since individuals are sampled uniformly the distribution should remain unaffected. Conversely, unless a recourse generator can perfectly replicate the original probability distribution, we expect the MMD of the positive class to increase. Thus, when discussing MMD, we generally mean the shift in the distribution of the positive class.

Finally, **feature mean and feature standard deviation** are also calculated to verify how the implementation of recourse impacts every attribute in the dataset. Although MMD already captures information about the expected value and variance, we may also be interested in a more granular look at individual features.

2) Model Shifts: As our baseline for quantifying model shifts we measure perturbations to the model parameters at each point in time t following [14]. We define $\Delta = ||\theta_{t+1} - \theta_t||^2$, that is the euclidean distance between the vectors of parameters before and after retraining the model M. We shall refer to this baseline metric simply as **Perturbations**.

We extend the metric in Equation 4 for the purpose of quantifying model shifts. Specifically, we introduce **Predicted Probability MMD (PP MMD)**: instead of applying Equation 4 to features directly, we apply it to the predicted probabilities assigned to a set of samples by the model M. If the model shifts, the probabilities assigned to each sample will change; again, this metric will equal 0 only if the two classifiers are the same. It is worth noting that while we apply the technique to samples drawn uniformly from the dataset, it can also be employed on arbitrary points in the entire feature space (or a subspace). The latter approach is theoretically more robust. Unfortunately, in practice this approachs suffers from the curse of dimensionality, since it becomes increasingly difficult to select enough points to overcome noise as the dimension D grows.

As an alternative to PP MMD we use a pseudo-distance for the **Disagreement Coefficient** (Disagreement). This metric was introduced in [26] and estimates $p(M(x) \neq M'(x))$, that is the probability that two classifiers do not agree on the predicted outcome for a randomly chosen sample. Thus, it is not relevant whether the classification is correct according to the ground truth, but only whether the sample lies on the same side of the two respective decision boundaries. In our context, this metric quantifies the overlap between the initial model (trained before the application of recourse) and the updated model. A Disagreement Coefficient unequal to zero is indicative of a model shift. The opposite is not true: even if the Disagreement Coefficient is equal to zero a model shift may still have occured. This is one reason for why PP MMD is our our preferred metric.

Finally, we introduce **Decisiveness** as a metric that quantifies the likelihood that a model assigns a high probability to its classification of any given sample. We define the metric simply as $\frac{1}{N}\sum_{i=0}^{N}(\sigma(M(x))-0.5)^2$ where $M(x)$ are predicted logits from a binary classifier and σ denotes the sigmoid function. This metric provides an unbiased estimate of the binary classifier's tendency to produce high-confidence predictions in either one of the two classes. Although the exact values for this metric are not important for our study, they can be used to detect model shifts. If decisiveness changes over time, then this is indicative of the decision boundary moves towards either one of the two classes.

IV. Experiments

1. Shift of focus from individual to group of individuals (related: https://www.researchgate.net/publication/353073138_Generating_Collective_Counterfactual_Explanations_in_Score-Based_Classification_via_Mathematical_Optimization)
2. Convergence criterium matters: terminating once threshold probability is reached may not be optimal (see e.g. REVISE)
3. Optimizer choice matters: dimensionality is typically low, so no obvious benefit to using ADAM.
   - This might be better placed in JuliaCon proceedings, perhaps backed by small blog post on the matter.
4. Mitigating strategy: penaliye distance from centroid.

V. Discussion

A. Limitations and Future Work

The experimental setup proposed here is designed to mimic a real-world recourse process in a simple fashion. In practice, models are in fact updated on a regular basis [14]. We also find it plausible to assume that the implementation of recourse happens periodically for different individuals, rather that all at once at time $t=0$. That being said, our experimental design is a vast over-simplification of potential real-world scenarios. In practice, any endogenous shifts that may occur can be expected to be entangled with exogenous shifts of the nature investigated in [14]. We also make implicit assumptions about the utility functions of the involved agents that may well be too simple: individuals seeking recourse are assumed to always implement the proposed counterfactual explanations; conversely, the agent in charge of the model M is assumed to always treat individuals that have implemented valid recourse as if they were truly now in the target class. Relating this back to the consumer credit example, we assume that the would-be borrowers are always willing and able to implement recourse and the bank is always willing to provide credit as would-be borrowers move across the decision boundary. In practice it is doubtful that agents behave according to such simple rules. Nonetheless, we think that our simple framework offers a starting point for future work on recourse dynamics (both endogenous and exogenous dynamics).

1) Data: The number of real-world datasets considered in this work is limited, constrained by the fact that at the time of writing `CounterfactualExplanations.jl` only supported continuous features.

VI. Concluding Remarks

Acknowledgment

P. A. thanks ...

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VII. Tables

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VIII. Figures

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IX. Code