We thank each reviewer for reading our paper and sharing your thoughts and suggestions. We address each review in turn:

Review #1 We understand your observation regarding the mention of "standard transportability algorithms" and "s-Thicket" which may be distracting for many readers, and share this concern with you. Our motivation to keep such references was to acknowledge relevant work. Still, we do appreciate your suggestion to move these references to footnotes to improve readability.

Also, thank you for pointing out the possible confusion with Figures 2 and 3, and the description of π^1 and π^2. We will certainly make the appropriate changes around this section to improve readability.

Regarding your particular questions:

- line 61: The distribution of Z (secondary condition) may be different in π^2 compared to π^*, for example, if it represents hypertension and due to the different in average age of the populations, the condition is more prominent in π^2.

- line 177: $\mathbf{d}$ is an instantiation of the set of variables $\mathbf{D}$.

- In Equation (3) it is not necessary to include W. One way to see why is to look at Fig. 2b an notice that W does not have any causal influence on any other variable under σ_X. Further, under this regime the latent confounding between W and both R and Z turns out to be irrelevant at this point of the derivation.

- In Fig. 2b the influence of R (characteristics of the property) on X is motivated by the description in line 138-139 that goes as "increase the percentage X by offering loaners insurance from default, while the percentage ought is above the regular threshold, for properties within certain locations", where location is a characteristic of the property. We will make this more explicit.

Review #2

It seems that the primary concern posed in the review is that the large number of results presented in the form of lemmas, theorems and corollaries without explanation or motivation.

As pointed out, we did not include proofs in the main text, mainly due to space constraints. However, we did intent to explain, at least in terms of statements and implications, each one of the presented results. For subsequent versions of the manuscript, we will try to make such explanations and motivation more prominent and understandable, within the space constraints.

The reviewer calls attention on the possibility of having other results such as lemma 1 not stated but moved to the appendix and explained with an example. It is evident that this is a valid choice, yet in this particular case at least, there is no much space that can be saved. Nevertheless, we could certainly improve the paper with this point in mind, bringing into the main text a proof sketch or main ideas of the proofs for some of the results.

Regarding your specific comments and suggestions:

1. The reference to Pearl and Robins 1995 is motivated by the fact that it is seminal work in dynamic plans literature in the context of causal inference where non-atomic interventions play a crucial role. We are not aware of further work by James Robins with a relevant relationship with the topic of our paper (transportability, multiple domains, experimental conditions) beyond the soft intervention aspect already addressed. Still, we would appreciate it if you would like to share specific references of this or any other author in the epidemiology/public health literature, thank you.

2. Please note that although the term "domain discrepancy" is not used, the symbol $\Delta^{i,j}, \Delta^i$ that entails the concept is widely used in the paper later (e.g., Definition of Selection Diagram, Lemma 3, Alg. 1, and Sec. 4). We were trying to save space leveraging the definition we introduced, instead of having to repeat this relatively long expression, "domain discrepancy."

3. We will certainly look for a better way to convey the point regarding previously proposed transportability algorithms (lines 180-187). Nevertheless, we addressed this point in more detail in section B.3 of the supplemental material.

5. We acknowledge that the density of results in the paper is high. However, we believe that there is also high cohesion between them such that presenting them in parts might not be a better alternative.

Review #3

We are glad you find our work sound and belonging to an important line of work, thank you. We agree with your appreciation that the paper requires a certain level of familiarity with the topic to be well understood. Still, we believe this is the case for most papers that introduce new theoretical results in any area in a premier venue such as NeurIPS.