To Reviewer M8jr

We thank you for your reviews, and address your concerns as follows.

Q1: The invariance assumption of the model being conditionally independent of the spurious attribute given the label, how is it different from Conditional domain adaptation [1] which matches the feature distribution given the labels and the domain. It seems like the same assumption is used here.

A1: Thank you for pointing out the reference [1], we have added this reference in the revised version. After checking this paper, we find their main idea is creating feature representation $f$ and classifier prediction $g$ that are uniformly unidentifiable under an adversarial trained domain discriminator. In this regime, the $f$ and $g$ together can be viewed as the output of model $f\_{\theta}(X)$ in our paper. Thus they aim to capture model that is invariant over domain i.e., invariance of $f\_{\theta}(X)\mid Z$ over $Z$. We have discussed such invariance in the last paragraph in page 4, which states that such invariance can not handle correlation shift in this paper.

Q2: This work lacks important citations and comparisons. They state that they can even estimate their CSV without ground truth group values but there already exists approaches which work without the ground truth labels [2,3,4] but many of these works have either not been cited or compared with. It would be good to have these comparisons included and also say what is different about their approach.

A2: Thank you for pointing out these references. Before clarifying the difference between our paper and these references, we should point out that we have compared with the recent SOTA methods [6, 7] to mitigate correlation shift in absence of observed spurious attributes, and our method consistently improve their methods. In a word, the differences are, 1): the spirit of [2,4] is prediction the label of spurious attribute which we do not need such classifier, 2): [3] use extra validation set that has observed spurious attributes. The detailed differences are summarized as follows:

**Compared with** [2], they first train a label classifier, then labeled the spurious attribute by clustering algorithm, and finally apply the GroupDRO [5] under each group consists of the data with same predicted spurious attribute and class label. Their method clearly depends on the correctness of predicted spurious attribute. While obtaining a high-quality spurious attributes classifier is hard in practice. Compared with their method, our RCSVU measures the intra-variation of model over the same class, and does not need predicting spurious attribute as they do. Empirically, our RCSVU beats their method in terms of worst-case group accuracy in both CelebA and Waterbirds, i.e., 76.9 v.s. 52.4 and 81.2 v.s 76.2. We have added this comparison in the revised version in Section 6 and Section 2.1.

Compared with [3], we have clarified in the last paragraph of page 2, their method use an annotation validation set with observable spurious attributes during training. Thus, comparing our method with theirs is unfair.

**Compared with** [4], the spirit of their method is similar to [3], that is, learning a soft-spurious attribute classifier to predict the spurious attributes. Empirically, our RCSVU beats their method in terms of worst-case group accuracy in Waterbirds and CivilComments i.e., 81.2 v.s. 78.7 and 68.7 v.s. 67.0. We have added this comparison in the revised version in Section 6 and Section 2.1.

Q3: The empirical results are marginal. This algorithm improves the accuracy by a small amount on worst groups but reduces the average weighted performance on all the datasets. Hence, it is not clear if they are actually getting rid of the spurious feature or just trading some of the majority group accuracy with the minority group.

A3: To show our improvements over the worst-case group is not “trading some of the majority group accuracy with the minority group”, we report the averaged accuracies over groups, and our method consistently improves the baseline methods on all experiments. As for “average weighted performance”, that is “total” accuracy on the test data, our method beat other methods on Waterbirds and MultiNLI instead of “reduces the average weighted performance on all the datasets.”. Compared with total accuracy on the test set, we think the averaged accuracies over groups is a better criteria to check the performance of model, as the latter does not depends on the number of data in each group. Finally, as we have clarified in the last paragraph in Section 6, all methods capture some spurious correlation, while our method can mitigate such overfitting.

Q4: It is also not clear how different this approach is from the group distributionally robust approaches which also try to reduce the gap between losses across different groups.

A4: We have made a comparison with GroupDRO in the second paragraph in page 7, please check it. In a word, we split the goal of “perfect in-distribution test accuracy” and “robustness over different spurious attributes”, which makes our training process more stable.

Q5: I am also wondering whether the authors needed to regularize their models as well to make this method work because the authors in [5] claim that if there is no regularization, these overparameterized methods fit well on all the groups in the training set and hence, there is no effect of distributionally robust optimization.

A5: In [5], the authors use a large weight-decay regularizer to create a stable training process that avoids overfitting the spurious correlation. However, as we use an explicit regularizer to avoid the overfitting, we do not need such a large weight-decay regularizer, and has stable training process, see the second paragraph in page 7 for more details.

Reference: [1] Conditional Adversarial Domain Adaptation

[2] No Subclass Left Behind: Fine-Grained Robustness in Coarse-Grained Classification Problems

[3] Just Train Twice: Improving Group Robustness without Training Group Information

[4] Environment Inference for Invariant Learning

[5] DISTRIBUTIONALLY ROBUST NEURAL NETWORKS FOR GROUP SHIFTS: ON THE IMPORTANCE OF REGULARIZATION FOR WORST-CASE GENERALIZATION

[6] Predicting with high correlation features.

[7] Simple data balancing achieves competitive worst-group-accuracy.

To Reviewer bzT6

We thank you for your reviews, and address your concerns as follows.

General Response: Thank you for pointing out many important reference, and some mathematical issues. We have made revisions to address your concerns in the revised version. The revision can be summarized as follows:

1. We add the comparison with the Makar et al., 2021, Makar et al., 2022, and Veitch et al., 2021. Please see A1, A2, A3 for details.
2. We have corrected the Lemma 1 and Theorem 3. The revised Theorem 3 does not change the result we want to convey. Please see A5, A6, A7 for details.

Q1: The comparison with Makar et al., 2021, AISTATS

**A1**: Thank you for pointing out this important reference, we have cited this paper, and added the comparison in the revised version in Section 2.2. After checking this paper, we summarize the difference as follows:

1. Their paper relies on a causal DAG, and assumes the existence of a sufficient statistic $X^{\*}$ such that Y only affects $X$ through $X^{\*}$, and it can be recovered via $X$. But we do not need such causal DAG and such sufficient statistic.
2. They characterize the correlation shift as by imposing the invariance on $P(X\mid X^{\*}, Z), P(X^{\*}\mid Y)$, and $P(Y)$, while we only impose invariance on $P(X\mid Y, Z)$, and $P(Y)$.
3. Their method is built upon reweighting strategy i.e., change of measure, to get the risk under unconfounded distribution. Beside that, the regularizer they used is to get unconfounded distribution. However, we do not aim to get such risk by change of measure, we directly regularize the training with the conditional invariance of model over spurious attributes.
4. Our method can be implemented in absence of spurious attributes, while their can not.

Q2: The comparison with Makar et al., 2022.

**A2**: Thank you for pointing out this important reference, we have cited this paper, and added the comparison in the revised version after Definition 2. After checking this paper, we summarize the difference as follows:

1. As in Makar et al., 2021, their results rely on a causal DAG, and the existence of sufficient statistic.
2. Though they prove that the similar result that conditional independence breaks the correlation shift as we do, their regularizer in (3) is only applied to spurious attributes with two classes, while we have no such constraint.
3. The regularizer they proposed, as they said, “has some practical limitations, especially when training using mini-batches of data in stochastic gradient descent”. Thus, the training objective they used is similar to the one in Makar et al., 2021. However, our regularizer is independent of the number of spurious attributes, and we develop the algorithm with provable convergence rate to solve the regularized mini-max optimization problem.
4. Our method can be implemented in absence of spurious attributes, while their can not.

Q3: Similarly, there needs to be more discussion of the relationship between the CSV penalty and the conditional MMD penalty given here and the conditional MMD penalty suggested in Veitch et al 2021 (cited in the paper).

**A3**: As we have clarified in A1 and A2, the regularizers in Makar et al., 2021 and Makar et al., 2022 are used to get the probability weights under unconfounded distribution. In Veitch et al., 2021, there are two regularizer. For the marginal regularization, it measures the invariance of $f(X)\mid Z$ over $Z$, which has been discussed in the last paragraph in the page 4. The other regularizer is conditional regularization, which measures the conditional invariance of $f(X)\mid Y, Z)$ over $Z$. The invariance is similar to the one our CSV regularizer aims to capture. However, compared with their regularizer, our improvements are three-folds 1): their probability measure only applies to spurious attributes with two classes. 2): Their metric is defined on a Reproducing Hilbert Kernel Space, which depends on the choice of reproducing kernel, thus effect the performance of model. 3): In contrast to theirs, our regularized training objective has provable convergence rate.

Thank you for pointing out this, we have added these comparison in the revised version after Definition 2.

Q4: The proof of Theorem 1 does not really make sense. For example, equation (39) is vacuous as written (the final expression is symbolically equivalent to the LHS). There is an obvious way to improve this. You want to show P(f(X),Y)=Q(f(X),Y) under the given assumptions.

A4: The equation (39) is to prove our first conclusion that $Y\mid f(X)$ (which decides the prediction error) is invariant over distributions. We prove $P(f(X),Y)=Q(f(X),Y)$ as you said in equation (40).

Q5: Lemma 1 is incorrect. Consider the counter-example: U=W, V=−W. Then I(W;U+V)=I(W;0)=0 but I(W;U)=H(W)≥0 (and I(W;V|U)=0). The change of variables from U+V to (U,V) is done incorrectly in the proof; the domain of integration would need to change.

A5: Thank you for pointing out this, lemma is used to derive the equation (50) in Theorem 3. We notice this mistake and refine the Lemma 1 and Theorem 3. The revised Theorem 3 does not change the result we want to convey, that is “the conditional independence is not in contradiction with in-distribution generalization”. Please check it in the Theorem 3 in the revised version.

Q6: I have difficulty parsing Theorem 3. It is not clear what the mutual information term involving θ means. The proof references a distribution over θ, which does not make sense to me. Similarly, Theorem 3 relies on the incorrect Lemma 1. Also, M is not defined.

A6: Please check the revised Theorem 3 in the revised version. The $M$ is the upper bound of loss function defined in Section 2.2

Q7: The prose around Theorem 3 seems to argue that there is no tradeoff between robustness and in-distribution prediction performance, which is not the case in general In fact, at best, the theorem would indicate that the generalization gap between the empirical risk and population risk is small, but says nothing about whether the minimized population risk subject to the conditional independence constraint is small (which is what I might interpret as "generalization capability").

A7: Theorem 3 does not mean there is no tradeoff between robustness and in-distribution performance. It provides an improved in-distributional generalization bound with the imposed conditional independence constraint. But the improved bound does not necessary means the conditional independence model has better in-distribution generalization error than the one obtained by ERM. However, it states that at least, capturing conditional independence will not result in an extremely large in-distribution generalization error. Plugging this observation into Theorem 2 indicates that conditional independence model has guaranteed OOD generalization error.

Reference:

Veitch et al., 2021, Counterfactual Invariance to Spurious Correlations: Why and How to Pass Stress Tests.

Makar et al., 2021, Causally Motivated Shortcut Removal Using Auxiliary Labels

Makar et al., 2022, FAIRNESS AND ROBUSTNESS IN ANTI-CAUSAL PREDICTION

To Reviewer fPjQ

We thank you for your reviews, and address your concerns as follows.

Q1: “I feel the final proposal i..e, (10) is over-restrictive. For e.g., why not perform sparse feature selection etc. to eliminate the spurious features.”

A1: We are not sure if the sparse feature selection here means the technique such as LASSO or sure independent screening. If so, to the best of our knowledge, there is no existing literature working on eliminating spurious feature via variable selection. We think this is because sparse feature selection builds upon the assumption that the ground model is sparse, while in OOD generalization

tasks such as image classification problems, useful features and spurious features can both be high dimensional, as in the example in Proposition 1 with both $d\_{1}$ and $d\_{2}$ very large. Thus, sparse feature selection may not find the ground truth model in this case.

Besides, for many OOD problem, designing the feature selection criteria is a problem, as spurious attributes may be more informative than the useful feature in then training data when fitting the class label. For example, in the example in Proposition 1, when $\sigma\_{YZ}(P)\rightarrow 1$, the sparse feature selection method will not necessary select feature in the first $d\_{1}$-dimensions (which are useful features), as both spurious features and useful features are informative to predict the label $Y$ in training set.

Q2: For resultant predictions, clearly (10) is restrictive, while they will alleviate the issue of spurious correlations?

A2: Our $CSV\_{U}$ impose the constraint that model should have uniform performance on data with the same class label. It is not contradicted to the ground truth model provided the ground truth model can classify the data perfectly. In this case, the ground truth model tends to have uniform good performance on data with the same class label and we add the term $CSV\_{U}$ to encourage the trained model to share such a property. Our regularizer is not restrictive as the objective model satisfies such constraint.

Q3: Even in simulations I think simple baselines that perform feature selection are very important for understanding the usefulness of the proposal. Also, since a plethora of diverse methodologies for feature selection exist, this may make the baseline very competitive.

A3: In the simulation i.e., toy example, we use the linear models, and we try to select the useful features i.e., the first 5 dimensions, thus all methods can be viewed as feature selection methods in this regime. The cosine-similarity in table 5 indicates how well the selected features are close to the useful one. To further address your concern, we consider two classical feature selection method, i.e., regularizing training with $l\_{1}$ (LASSO) and $l\_{2}$ regularizers (ridge regression), the results are summarized in Table 4 in the revised version. Our method beats the two feature selection methods.

We also add $l\_{1}$ regularizer in the experiments of CelebA, Waterbirds, MNLI, CivilComments in Section 6 of the revised version, the results show that our methods beats these baselines as well.

Q4: “While the proposal in observable case is straight-forward (why not use direct ways of measuring conditional independence than via (3)?)”

A4: As claimed in Section 4.1, the proposed CSV (3) can be computed via training distribution so that we can regularize training with it to improve OOD generalization. More than that, the proposed CSV can directly upper bound the OOD generalization error as in Theorem 2.

Q5: The proposal in more pratical case of unoservable suprious correlation seems restrictive making the contribution less strong.

A5: The proposed $CSV\_{U}$ is used to estimate CSV in absence of observed spurious attributes, which can be computed without any other information. When we have observed spurious attributes, we can estimate CSV via (7) as we claimed in the paper.

Q6: Also I prefer avoiding associating spurious features with non-causal features. because one can have anti-causal learning that is not spurious.

A6: Thank you for pointing out this. The expression that $Z$ is not causal to predict $Y$ does not mean we aim to find the causal to predict $Y$. We only chasing for the stable predictors which are not necessarily causal.

Q7: In view of the issues desribed above like over-restirctedness of (10) and missing comparisons with feature selection based methods, I tend to recommend a reject.

A7: The restrictedness of (10) has been addressed in A5, the comparison with feature selection methods has been discussed in A3.

To Reviewer Lqom

We thank you for your reviews, and address your concerns as follows.

Q1: some terms may not be well known. I would suggest adding explanations. This includes: "domain labels”

**A1:** This notation is from domain generalization in which data are from different environment i.e., domain, the domain label indicates which domain is the data from. We have added this explanation in the Section 2.1 in the revised version.

Q2: paragraph 3 on page 3: what is the difference between "input's label" and "class label"?

**A2:** They are the same, we have unified the notation as class label.

Q3: I would suggest a comparison of the proposed condition with the condition in this paper: Smale, Steve; Zhou, Ding-Xuan. Online learning with Markov sampling. Anal. Appl. (Singap.) 7 (2009), no. 1, 87--113. For example, I see many works in the literature assuming the invariant of conditional distribution of labels instead. I am curious about the difference between this setting, and the setting used in the current paper.

A3: After checking the suggested paper Smale et al., 2009, we think it studies a different problem. The paper mainly focus on the online learning problem while the data stream are not i.i.d. Their goal is learning a model that performances well on these non-i.i.d. training data, instead of generalizing on some unseen data which are from distribution in different with training distribution. However, the problem of generalizing on unseen OOD data is mainly considered in this paper.

As for the condition of invariant conditional label distribution $P\_{Y\mid X}$, this is used to obviate the label shift. For example, under different environments, the data should have the label i.e., $P\_{Y\mid X} = Q\_{Y\mid X}$ for different $P$ and $Q$ with represents the probability under different environments.

Q4: Some small typos: "quantify" before Theorem 4 should be "quantifies"

**A4**: Thank you for pointing out this, we have fixed it.

Q5: Please define the big-O notation in Theorem 4. In particular, what is the limit process?

A5: The big-O notation is defined in Section section 2.2, and in Theorem 4, the limit process is $n\rightarrow \infty$.

Q6: I hope that the author(s) can specify the definition of P, Q, X, Y, and Z, which I believe would greatly help the readers. In particular, I have the following questions directly related to the paper: (a). Is "P" (and "Q") a distribution of (X,Y) or (X,Y,Z)?

**A6**: The $P$ without subscript is the distribution of $P\_{X, Y, Z}$ and we write it as $P$ for simplification. Thank you for pointing out this, we have make it clear in the Section 2.2 in the revised version.

Q7: In general, X and Y are not independent. Are X and Z independent? Are Y and Z independent? Are (X,Y) and Z independent?

A7: $X$ and $Z$ are not independent, $Y$ and $Z$ are not independent, $(X, Y)$ and $Z$ are not independent.

Q8: If Z and Y are not independent, then Z has the prediction power on Y. Would this still fit the definition of "spurious correlation" in this paper?

A8: As claimed in paragraph 3 in page3, the spurious correlation means the feature $Z$ is not the casual to the label, but it does not necessary that $Z$ is independent of label. For example, in Figure 1, the celebrity’s gender (label Y) is not a causal of celebrity’s hair color (spurious feature $Z$), while the correlation between gender and hair color may vary from training to test data e.g., most males have dark hair in training set and vice-versa in test set.

Q9: “In this paper the label set script-Y is discrete (Assumption 1). For the time being, let me assume the label set is just {0, 1}…”

**A7**: Thank you for pointing out this, in Assumption 2, the loss function $L(f\_{\btheta}(X), Y)$ should be $R^{|Y|}\times |Y|\rightarrow R^{+}$ with $f\_{\btheta}(X)$ takes values in $ R^{|Y|}$. Then the discontinuous problem you mentioned is addressed in Section 2.2 by refining the $L(f\_{\btheta}(X), Y)$. This is a typical setting in machine learning community. For example logistic regression the loss on data $(x\_{i}, y\_{i})$ is $-\log\left(\exp{f\_{\btheta}(x\_{i})(y\_{i})} / \sum\limits\_{y\in [K\_{y}]}exp{f\_{\btheta}(x\_{i})(y)}\right)$.

**Q8**: Right after Theorem 4, I guess the expression "A decreases with B" is not clear: what is its precise meaning?

A8: It should stated as CSV is upper bounded by $\hat{CSV}$, it has been corrected in the revised version.

Q9: The definition (11) is confusing. It is obvious that when u runs through Delta\_m, the maximum of u dot F is the maximum coordinate of the vector F. Why not directly use max-coordinate but employing Delta\_m and writing (11) in the current complicated way?

A9: Yes you are correct. However, the function $\max\_{1\leq i\leq K}f\_{i}(\theta)$ w.r.t. $\theta$ is usually discontinuous. Thus we should consider its surrogate as we did in this paper i.e., convert it into $u^{\top}F^{k}(\theta)$ for some $u$. This is a classical method to handle such problem, more details can be referred to Section 5.2.4 in Bubeck et al., 2015.

Q10: Page 14, right after (17), how does one derive P\_{X|Y}=Q\_{X|Y} without assuming P\_Z=Q\_Z? Also in Example 1, all the numbers 0 should be -1.

A10: Right after (17), as in (2), we have verified that the difference among explored distributions only originates from the variation of $P\_{Z\mid Y}$ and $P(Y)$ (label shift) which are all unrelated to $P\_{X\mid Y}$, thus $ P\_{X|Y}=Q\_{X|Y}$. The typos in Example 1 are fixed in this paper.

Q11: 11. I feel that the loss function in Example 1 is not designed properly. Indeed, L(1, -1)=3 while L(1, 1) = 4 is even larger

A11: It does not matter, we only need $1 = L(-1, -1) \leq L(1, -1) = 3$ and $4=L(1, 1) \leq L(-1, 1)=6$ to make the loss proper to find the right prediction.

Q12: Also, the inequality (19) is derived based on the assumption that f(X)=1 almost surely, so the claim that (19) holds for any f is wrong.

A13: (19) is a lower bound for any $f$ takes values in $\{-1, 1\}$, so it holds for any $f$, for example for $f=-1$, the loss difference becomes $5TV(P\_{Y}, Q\_{Y}) \geq TV(P\_{Y}, Q\_{Y})$.

Q14: Proof of Propsition 2: please provide section or page number when citing a book. In this proof however, since script-Y is discrete, one may simply choose A=Script-Y and there is no need to take supremum.

A14: Thank you for pointing out this, we have added it. In this proof, we directly adopt the definition of total variation.

Q15: Equation (24) is wrong on the treatment of sup\_{A in script-Y}.

A15: Thank you for noticing it, actually there is a typo in the first equality in (24) where the sum should inside of absolute value from the definition of total-variance distance. After fixing it, the (24) is right on the treatment of $\sup\_{A}$.

Q16: Equation (25) is wrong. For example in the last equation, the left-hand side depends on y, while the right-hand side does not.

A16: Please note from the definition of $w^{\*}(y) = 1 / (|Y|P\_{Y}(Y = y))$, so that $w^{\*}(y)P\_{Y}(Y = y)$ does not depend on the value of $y$. The (25) is obtained via the explanation right after it, please check it.

Q17: Proposition 2 is wrong. Obviously, since Q is in script-P, Q\_Y=P\_Y, so the only function w that achieves the minimum TV is a constant function w(y)=1, which makes the TV equal to zero.

A17: Proposition 2 gives the optimal reweighted label probability distribution under the criteria of **minimax** total variance distance with a series of distributions $Q$. In fact the loss will never becomes zero, the loss is $\sup\_{Q}TV(P^{w}\_{Y}, Q\_{Y})$ for fixed $P^{w}\_{Y}$, so that we can simply choose another $Q$ in-different with $ P^{w}\_{Y}$ to make the loss larger than zero.

Q18: In the proof of Proposition 1, the classifier does not fit the framework proposed in this paper. In particular, the first argument of the loss function script-L is defined on script-Y, a discrete space. However, the classifier here outputs a real number. I guess a reasonable solution is to change the definition of the loss function to accept real first argument (at the top of page 3). This change may help to resolve the issue on Assumption 2.

**A18**: Thank you for pointing out it, we have change the definition of $L(\cdot, \cdot)$ as a function from $R^{K}\times Y\rightarrow R^{+}$ to fix both the problem in Assumption 2 and Proposition 1. Please check it in Section 2.2 in the revised version.

Q19: I think the first part of the proof of Theorem 1 does not work. We need to prove Q(Y|f(X))=P(Y|f(X)), but the current version of the proof still can not bridge to P(Y|f(X)). Please give explicit calculation that leads to this bridge.

A19: As we have claimed in equation (2), the difference in distribution $P\_{X, Y}$ and $Q\_{X, Y}$ **only originates** from the variety of distribution $P\_{Z|Y}$, as we have shown $Q(Y|f(X))$ is independent of $Q\_{Z | Y}$, similarly $P\_{Y|f(X)}$ is independent of $P\_{Z|Y}$. So $P\_{Y|f(X)}$ should be invariant under different $P\_{Z | Y}$, so that $P\_{Y|f(X)} = P\_{Y|f(X)}$. To make it more clear, $Q\_{Y|f(X)}(y) = Q\_{Y, f(X)}(y) / int\_{y}Q\_{Y, f(X)}(y, f(X))dy$ whose variety of $Q$ can only originates from the variety of $Q\_{Z|Y}$, while we have proved $Q\_{Y|f(X)}(y)$ is invariant over $Q\_{Z|Y}$. Thus, it is invariant of $Q$.

Q20: I feel confused about the treatment around Equation (40). In particular, the set script-P of distributions only provide Q\_{X|Y,Z}=P\_{X|Y,Z}, and here it seems to me that one is using Q\_{X,Z|Y}=P\_{X,Z|Y}. Please provide more details for this proof.

A20: In the integral term of (40), according to the conditional independence, $\int\_{Z}Q\_{X, Z|Y}(f(X, z|Y))dz = Q\_{X|Y}(f(X) | Y)int\_{Z} Q\_{Z|Y}(z| Y)dz = Q\_{X|Y}(f(X) | Y)$ which is independent of $Q\_{Z|Y}$, thus $Q\_{X, Y}$ is invariant over $Q$. We have added this part in the revised version.

Q21: My questions on Theorem 3 and its proof: is theta a random vector or a parameter? Seems neither way fits the proof. For the inequality "a", if all script-L stay close to M, then the log expectation term can not be bounded by O(1/n), is that right?

A21: $\theta$ here is the parameters obtained on training set, thus it depends on training data and has randomness. Please notice that we assume $E[L(f\_{\theta}(\tilde{x\_{i}}), y\_{i})] = 0$, with out loss of generality, so that $L(f\_{\theta}(\tilde{x\_{i}}), y\_{i})$ will not stay close to zero. This is a classical technique to derive informatic generalization bound. Please check Xu et al., 2017 for details.

Q22: There is a general question I feel confused: is Z observed as data? If yes, then Z goes to learning process and we do not have the OOD problem. In the scenarios that the distribution P\_{Z|Y} changes from training to testing, the boundary between X and Z may not be easy to specify, and therefore the CSV penalty is still not easy to define.

A22: Usually $Z$ **can not be** an observed vectorized data. For example, in Figure 1, the celebrity’s hair color is spurious attribute $Z$. Usually we do not have the pixel value of hair color, but we may have its label e.g., dark hair during training. In this paper, we mainly treat the spurious attributes as label as claimed right after Assumption 2. But our results before section 4.2 can be generalized to $Z$ is not a label, so we do not impose this Assumption before section 4.2.

Q23: On the other hand, I feel very difficult to imagine a case where Z is not observable but still needs treatment.

A23: Let us back to Figure 1, to classify celebrity’s gender with celebrity’s hair color as spurious attribute. The label of hair color may be unobservable (uncollected) in training set. However, overfitting the correlation shift in training set may deteriorates the performance of the model on OOD test data. For example, if most males have dark hairs in training set, and the model overfits such spurious correlation, that means dark hair becomes a feature to predict the celebrity is a male. Then dark hair may becomes an interference feature if the most male in test data have blonde hair.

Ref:

Bubeck 2015, Convex Optimization.

Xu et al., 2017, Information-theoretic analysis of generalization capability of learning algorithms.