# Response Letter

### Reviewer 1:

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| **Reviewer comment 1.1** | The authors need to reason more clearly or state more explicitly in the main text which ALoFT values they consider to be robust indicators for pathogenicity. This would truly help geneticists discriminate between benign and damaging LoFs. Figures 2-4 address that to some degree, but solely presenting these graphs is not informative enough in my opinion for the general genetics reader. It is essential to show the power of LoF values in concrete numbers as the selling point this article; namely that ALoFT can be used by the genetics community to distinguish LoF that are disruptive from benign variation. It is difficult to understand Figure 2 at first glance. |
| **Authors’ response** | We agree with the reviewer that graphs alone are not enough for the general genetics reader. We have clarified this further in the text clearly indicating that ALoFT provides three scores corresponding to the three classes: benign, dominant and recessive. The dominant and recessive scores are the scores relevant to pathogenicity. Moreover, the effect of the variant is clearly output in the prediction file in the last column of the file. For example, in Supplementary Table 6, the last column indicates if the pLoF variant will be benign or have a dominant or recessive effect.  We have included tables of predicted ALoFT scores as Supplementary Tables 6 and 9 and uploaded them as additional files. Given that they are big files, we did not include it in the Supplementary pdf but did include it as Supplemental additional files in our original submission. |
| **Changes in text** | “The prediction output provides three scores for each pLoF variant that correspond to the probability of the pLoF being benign, dominant or recessive disease-causing allele. In addition, ALoFT also provides the predicted pathogenicity. The pathogenic effect of pLoF variant is assigned to the class that corresponds to the maximum score.”  Changes also made to Figure 1. |

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| **Reviewer comment 1.2** | The sections on cancer genomics and autism seem somewhat separate entities in the manuscript. It is not clear how these, especially the section and figure on cancer genomics, integrate and relate to the preceding material. |
| **Authors response** | ALoFT is developed to help identify deleterious pLOF variants in disease studies. The section on cancer genomics and autism are included to illustrate the utility of ALoFT in prioritizing pLOF variants. We used known case studies and show that the ALoFT scores of pLOF variants corroborates previously published research, thus validating the method. In the case of autism, previous studies have shown that *de novo* pLoF variants are associated with autism spectrum disorder based on differences in mutation rates in genes in unaffected individuals versus affected probands. We have been able to show the same results from an entirely different approach. In the case of cancer, ALoFT is able to discriminate between pLoF variants in known driver genes versus other genes. We have made these points clearer in the text. |
| **Changes in text** | We have reorganized the text so that the motivation for the different sections is clear. We now lay out the paper in the following way:  1. Description of the ALoFT classifier, the training datasets used and the evaluation of the classifier using metrics such as AUC, PPV etc.  2. Validating ALoFT predictions by applying them to known disease variants and corroborating published studies.  3. Application of ALoFT to understand pLoFs in personal genomes. |

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| **Reviewer comment 1.3a** | 3) Source databases. It seems somewhat surprising that the authors relied so heavily on 1000 genomes data for this paper. This seems somewhat outdated now that there are other public resources available that comprise data of thousands of exomes (i.e. ExAC with >60,000 exomes). Because the authors specifically mention that allele frequency is the most important feature for ALoFT classification, it would seem that up-to-date integration of allele frequency from as large and well annotated a database as possible is essential to optimize the use of ALoFT as a bioinformatic variant analysis tool. |
| **Authors’ response** | We thank the reviewer for the suggestion. Based on the reviewer’s critique, we requested Daniel MacArthur, the lead member of ExAC consortium and a co-author on this paper if we could use the data. We have revised the manuscript by including ExAC allele frequency as a feature.  Of the 5,495 SNPs that introduce a premature stop codon in the 1000 genomes dataset, we only used 397 homozygous SNPs for the training model. Thus, only a very small fraction of the 1KG variants was used as the training dataset. Also we did not use allele frequencies from 1KG (to avoid potential over-fitting problem), but use ESP6500 and ExAC allele frequencies as prediction features.  We would also like to bring to the attention of the reviewer that we show that while allele frequency improves the accuracy of the prediction, the classifier performs well even in the absence of allele frequency. In fact, prediction accuracy was lower when allele frequency features were the only features used for training the model (this was included in Supplementary table 5, multiclass AUC =0.79 for a prediction model trained based on only allele frequency from ESP6500 and ExAC. Network (AUC=0.81), functional (AUC=0.85) and evolutionary (AUC=0.86) features outperform allele frequency features when used separately to train the classifier). Thus, integration of all features improves prediction accuracy (AUC = 0.97)**.**  It should also be noted that it is not possible to estimate the relative importance of each feature because some features are correlated with each other. Therefore, the effect of permuting the values of one feature will be masked by other features that could compensate for this feature in the random forest method.  We have revised the text by updating the model with allele frequency from ExAC. We have also included ExAC in Figure 3. |
| **Changes in text** | The model is changed to include ExAC allele frequency. Figure 3 is modified to include ExAC. |

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| **Reviewer comment 1.3b** | 3) Does the authors' claim that per individual only 2 LoF variants (of  over 100 putative variants) could lead to disease if present in  homozygous state stand up when validated in larger database? This number seems rather low. |
| **Authors Response** | The number of deleterious LoF variants per individual is estimated in a conservative way by excluding any potential erroneous variants (comparing to ESP6500). Also, please note that this number pertains only to pLoFs that result due to the introduction of Stop codon due to a SNP (missense, splice and frameshift indels are not included in this calculation).  We have contacted Daniel MacArthur to calculate the per individual statistic for ExAC data. |
| **Change in text** | [[[ To be added from Daniel’s calculation]]] |

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| **Reviewer comment 1.4** | It would appear that the same population - same samples/variants were used for model training and then used again test the software  performance as shown in Fig 2 and Fig 3. Is this really the case? If  not please clarify. If so, how is this justified? A different an  independent set of data should be used to validate the trained model. |
| **Authors Response** | We apologize for the confusion due to the use of 1KG and HGMD nomenclatures for both training and testing. Completely different variants were used for training and testing***.*** For example, all training variants are removed in Fig 2 (now Fig4) and Fig 3. We have added Supplementary table 3 to clarify this issue***.*** The model was trained on homozygous LoF variants in 1KG. The rest of the analysis is done on heterozygous LoF variants. For the training sets comprising of disease mutations, only HGMD mutations in genes that could be assigned to either the dominant or the recessive category based on OMIM were used. Of the 1,800 disease genes in HGMD with pLoFs, only 932 genes could be assigned to dominant or recessive category. Thus, we only used a subset of HGMD variants (those in these 932 genes) for training and the remaining variants for other analysis. Please note that the legend to Figure 3 clearly stated that training variants are not included in the figure. |
| **Changes in text** | We have added Supplementary table 3 that includes the number of variants that were used for training from the 1KG and HGMD datasets. Besides training the classifier, all other reported analyses exclude variants that were used to train the model. |

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| **Reviewer comment 1.5** | 5) Some known dominant or recessive disease causing mutations from CMG were tested by ALoFT, as well as GERP and CADD shown in Fig 4. However, the number of tested variants is very small at the end of the day (4 dominant, 9 recessive); with even a few outliers, these variants could also overlap with HGMD variants in training dataset. In such small testing sample set, it seems 1 out of 4 dominant mutations was an outlier and low; while 2 out of 9 recessive mutations were outliers, and LoF scores were overlapping with dominant mutations, which were very high. A larger number of tested samples would increase overall confidence in the robustness of the approach. |
| **Authors response** | We realize that the number of testing variants is small. To address the reviewer’s point about the robustness of the approach, we used ALoFT to classify pathogenic variants from ClinVar that do not overlap with the training variants. This is now shown as new Figure 4a. The CMG figure is moved to the supplement as supplementary figure 4 |
| **Change in text** | We have added a new Figure 4a which shows the performance of ALoFT on an orthogonal dataset of disease mutations obtained from ClinVar. |

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| **Reviewer comment 1.6** | 6) A major shortcoming is that the software predictions were not  supported by any experimental studies of function or mechanism. This would provide ultimate validation and confidence in the ALoFT approach. Overall, to follow other examples of new bioinformatics approaches that produced findings that have tangible translation and utility, it might have been more compelling to have started from the outset with a dominant and/or recessive disease family with an unknown mutation, test a list of nonsense mutations, use dominant LoF score and inheritance pattern to evaluate the mutations, and use filtered gene (mutation) list and perform functional study to find causative mutation. This would provide the ultimate validation of the robustness and utility of the approach. |
| **Authors response** | We understand the reviewers point about validating predictions, however experimental validation is beyond the scope of this work. To show the robustness of the method, we applied the classifier to several known case studies and show that our prediction results agree with published results. |

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| **Reviewer Comment 1.7** | 7) The authors discuss that while LoF variants are still observed in  healthy controls, they tend to affect minor isoforms. They then  demonstrate this by showing 12 isoforms of NF2 with premature stop mutations in all 12 isoforms in the HGMD cohort and only 2 premature stop mutations in healthy controls. In the HGMD cohort however, there are multiple premature stop mutations in 'minor isoforms' such as isoform 7, 8, and 10 yet they are still classified to be 'disease-causing'. Further, the authors make this point using only one gene as an exemplar. The authors are encouraged to use multiple genes in support of this isoform trend. |
| **Authors Response** | We provide three reasons that could explain the presence of pLoF variants in known disease genes in healthy controls. One of the three reasons is that some pLoF variants in known disease-causing genes in healthy controls affect isoforms that are different from the isoforms that carry disease-causing pLoF variants. Thus, disease-causing pLoFs and pLoFs in healthy controls occur in mutually exclusive isoforms. The major and minor classification is not related to the length of the isoforms. So we cannot conclude that isoforms 7, 8 and 10 are ‘minor’ isoforms. Perhaps the use of the term ‘minor’ and major isoforms is probably confusing. We have modified the text to make it clearer. We also include Supplementary figure xx that shows the occurrence of 1KG pLoFs and disease-causing pLoFs in mutually exclusive transcripts. However, due to concerns raised by reviewer 2, we have removed the discussion pertaining to NF1. |
| **Change in text** | [[[SB look into making a collage of some examples.]]] |

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| **Reviewer comment 1.8** | 8) Dominant gain of function mutations are not included in this study.  Perhaps a brief comment on GoF variants in the overall landscape might  be valuable. |
| **Authors response** | In addition to LoF effects, truncating mutations can also lead to gain of function. However, gain of function mutations are difficult to model systematically as the effect of variant is very context dependent. It depends on the biological context of the gene that can vary widely. In order to minimize errors that might arise due to inadequate modeling of GoF effects and focus only on LoF, we chose to only use predicted haploinsufficient genes as the training data for dominant model. While this is clearly mentioned in the Supplementary text, we have moved this detail to the main text. We also revised the manuscript to elaborate a bit more on this point. |
| **Change in text** | “In addition to loss-of-function effects, truncating mutations can also lead to gain of function. However, gain of function mutations are difficult to model systematically as the effect of variant is very context dependent depending on the biology of the gene and can vary widely for different genes and gene classes. In order to minimize errors that might arise due to inadequate modeling of gain-of-function effects and focus only on LoF, we only use predicted haplo-insufficient genes as the training data for dominant model.” |

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| **Reviewer comment 1.9** | 9) The Supplemental section is poorly structured. Supplemental  information is merged into a Supplemental Methods file wherein it is  not always immediately clear what methods description and legends are.  Readability would probably improve when each Supplemental Figure/  Table is presented on a separate page. Please, add a list of all used  abbreviations to the Supplemental Data. Abbreviations are not always  explained. |
| **Authors Response** | We have made the requested modifications as per the reviewers’ suggestion. We have added a list of all the abbreviations as well as presented each Supplemental Figure and Table on separate pages. |

### Reviewer 2:

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| **Reviewer 2 Comments (General Points)** | |
| **Reviewer Comment 2.1** | The authors have sought to provide 'real-life' examples of how  ALoFT could be useful. However, their limited expertise in these areas (e.g. cancer, NF2, clinical genomics) has led to some simplistic  assumptions and I would recommend that they seek input from experts in the relevant fields, of which there must be many in their local environment. |
| Authors Response | ALoFT is developed to help identify deleterious pLOF variants in disease studies. The section on cancer genomics and autism are included to illustrate the potential use of ALoFT in prioritizing pLOF variants. We have consulted Lajos Pusztai (director of Yale Cancer Center Genetics, Genomics and Epigenetics Program) and Mark Rubin (Weill Cornell Medicine), they are very interested in using ALoFT for pLoF variants discovered in cancer studies. |

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| **Reviewer Comment 2.2** | The use of terminology in the manuscript is loose and sometimes  confusing. Please review this carefully throughout. In particular the  authors should reconsider their use of the term LoF, as they are  evaluating variants that cause premature truncation which can cause  reduction, loss or gain of function. At the very least 'putative  loss-of-function' should be consistently used throughout, including in  the name of the tool. |
| **Authors Response** | We agree with the reviewer that variants that cause premature truncation can do so by LoF, GoF or reduction of function. Therefore, we prefer to label such variants as putative loss-of-function variants (pLoFs) In this revision, we have defined and consistently labeled them as pLoFs. Biesecker et al. have also used this terminology in their paper titled “Individualized iterative phenotyping for genome-wide analysis of loss-of-function mutations“ (PMID: 26046366). |
| **Changes in text** | We have consistently used the term putative loss-of-function (pLoF) throughout the manuscript. |

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| **Reviewer 2 Comments (Major Points)** | |
| **Reviewer comment 2.3** | The training sets are clearly present in some of the data analyzed  in the results, this should be noted and potential caveats addressed,  e.g. overfitting. As an example, the authors note that ALoFT performs well at distinguishing HGMD disease-causing variants in the last exons of genes, but it is unclear from the methods if any of these were used for training. Some explanation of the overlap between the sets is necessary to provide confidence in the robustness of these analyses. |
| **Authors Response** | We apologize for the confusion. Completely different variants are used for training and the following analysis***.***  We have added Supplementary table 3 to clarify this issue. The model was trained on homozygous LoF variants in 1KG. The analysis was done on heterozygous LoF variants. For the training sets comprising of disease mutations, only HGMD mutations in genes that could be assigned to either the dominant or the recessive category based on OMIM were used. Of the 1,800 disease genes in HGMD with pLoFs, only 932 genes could be assigned to dominant or recessive category. Thus, we only used a subset of HGMD variants for training and used the remaining variants for analysis.  All training variants are excluded in the analysis pertaining to Figure 3 where we show that ALoFT performs well in distinguishing, disease-causing HGMD mutations in last exons. This had been included in the Figure 3 legend. |
| **Change in text** | We have added Supplementary table 3 that includes the number of variants that were used for training from the 1KG and HGMD datasets. All analyses exclude variants that were used to train the model. |

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| **Reviewer comment 2.4** | HGMD designations of pathogenicity are universally known to not be  robust, and this has been documented many times. Whilst the authors may not have any other datasets to hand, this fact should at least be highlighted, else the unfortunate use of these classifications, as truth sets will be propagated. |
| **Authors Response** | We have made a reference to this by describing confounders due to imperfect training datasets in our initial submission. However, we have elaborated and made this clearer in the revised text. Also to restrict to a more confident set of HGMD, we used variants that are annotated as “DM” (disease-causing mutations) only. |
| **Change in text** | The estimation and disease-causing potential of deleterious alleles can be affected by a number of confounding factors that include incomplete penetrance of disease alleles, variable expressivity, compensatory mutations, marginal variant calls and imperfect training datasets.  Changes made in the paragraph below the heading “Application to 1KGP1: Estimating the number of pathogenic pLoFs in a healthy genome or understanding pLoFs in an individual genome”. |

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| **Reviewer comment 2.5** | The impact of strandedness of gene transcripts on annotation was  not explicitly addressed, but is essential to incorporate. For example  in the sentence 'The pipeline also includes features to help identify  erroneous LoF calls, potential mismapping, and annotation errors,  because LoF variant calls have been shown to be enriched for  annotation and sequencing artifacts' are annotation discrepancies due to strandedness included within this? |
| **Authors’ response** | Our annotation algorithm (VAT, vat.gersteinlab.org) used in ALoFT takes into account the strandedness of gene transcripts and correctly annotates variants based on their strand direction. We have updated the method to make it clearer. |

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| **Reviewer comment 2.6** | 4. I was unconvinced about the assumptions drawn on isoforms. i.e.  that LoFs in healthy individuals affect minor isoforms. Although as a  trend it seems reasonable, much more data needs to be presented and evaluated before proposing that, for example, some truncating NF2 mutations are not disease-causing. It clearly cannot be the only  reason; e.g. many LoFs in disease genes in healthy individuals cause disease in other individuals, often within the same family. |
| **Authors’ response** | We agree with the reviewer that isoform-specific effect of pLOFs cannot be the only reason for seeing pLoFs in disease genes in both healthy controls and cases. In our previous submission, we have included other potential reasons such as incomplete penetrance, compensatory mutations etc.  We observe the presence of pLoF variants in known disease genes in healthy controls. We speculate one of the reasons is that some pLoF variants in known disease-causing genes in healthy controls affect isoforms that are different from the isoforms that carry disease-causing pLoF variants. Thus, disease-causing pLoFs and pLoFs in healthy controls occur in mutually exclusive isoforms. We tried to use the NF2 as an example to show the case. Nonetheless, we understand the reviewers critique in the context of NF2 and we have removed the discussion pertaining to NF2 and restructured the manuscript.  The reviewer also agrees, that “as a trend, it seems reasonable”. To this point, we also had included in the previous submission manuscript that ~12% of pLOFs in known disease genes fall in this category (disease-causing and potential benign pLoFs in mutually exclusive isoforms). In this revision, we have now included some observed cases (Supplementary Figure XX) to show this. |
| **Changes in text** | Modified NF2 section. Supplementary Figure XX is added. |

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|  | **Reviewer 2 Comments (Other Points)** |
| **Reviewer comment 2.7** | The focus in the introduction on specific truncating variants in  genes seems partial both in terms of mechanism and disease (all heart disease related). In addition to not being representative of the  overall knowledge base in this area it is at odds with the focus of  the paper, which includes autism and cancer, but not heart disease.  There are cancer examples that could be cited. |
| **Authors’ response** | The intention of the introductory paragraph was to showcase protective pLOFs reported in literature that happen to be cardiovascular-related. However, we have included examples of pLOFs and their relevance in cancer in the new revised version. We also would like to clarify that the focus of the paper is not on autism or cancer. Examples from autism and cancer were used to show that ALoFT predictions corroborate published results. Therefore, the method is robust and can be used in various disease contexts. |
| **Changes in text** | In page 2, the following sentence along with citations to two papers have been included.  pLoF variants are also prioritized in cancer studies where various filtration schemes are used to narrow down causal mutations 15,16. |

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| **Reviewer comment 2.8** | 2. Please don't use the term 'premature stop-causing SNPs'. I assume these are stop-gain (nonsense) variants. In fact I was confused as to whether 'premature stop' was being used to collectively describe all variant classes that could lead to premature truncation (i.e. including frames-shifting indels)' or just stop-gain variants (i.e.base substitutions that result in a stop codon) or both at different times in the paper. Please make this clear and define and then consistently use terms. The authors used: nonsense, premature stop variant, premature stop mutation, premature stop-causing SNPs,premature stop alleles and premature stop codon in the manuscript. |
| **Authors’ response** | We thank the referee for pointing this out. We have modified the terminology. SNPs that can introduce a premature Stop codon, indels that result in framshifts, and variants that affect splice sites have been collectively referred to as LoF variants. Some have correctly argued that the molecular functional effect of such variants need not always be LoF. Hence they are also referred to as premature truncating variants. To make it clear and consistent throughout the manuscript, we have modified the manuscript to refer to them as pLoFs (putative loss-of-function variants). |
| **Changes in text** |  |

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| **Reviewer comment 2.9** | In turn this meant I was unclear whether some of the analyses were  restricted to stop-gain variants or also included the putative  loss-of-function variants due to indels. This needs to be clear. I  think it is essential that the tool is fully evaluated on indels as  well as stop-gain variants. |
| **Authors’ response** | The training model was parameterized using variants that introduce a premature Stop codon since indel calling indel calling methods are not robust. However, ALoFT works on indels and we have updated the text to make this clear. We have also included the performance of ALoFT on indels in this revised manuscript. 99.4% of HGMD ‘DM’ frameshift indels are predicted as pathogenic, whereas only 19.4% in 1KG are predicted as pathogenic. |
| **Changes in text** | 99.4% of HGMD disease-causing (‘DM’) frameshift indels are predicted as pathogenic based on the ALoFT maximum score. |

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| **Reviewer comment 2.10** | I think the cancer driver analysis part is weak. A version of this  strategy is already incorporated as standard in cancer driver  discovery pipelines. I would advise omitting this, or at least  checking with some of the major cancer institutes whether they would use ALoFT in that context. |
| **Authors’ response** | ALoFT discriminates disease-causing somatic pLOFs from benign somatic LoFs. We applied ALoFT to a variety of diseases to illustrate the fact that the method works well and produces meaningful results. One of the most-used cancer driver gene detection methods - OncodriveFM - has used the idea that driver genes may accumulate variants with high functional impact possibly due to positive selection. It uses deleterious scores of missense somatic mutations. Here, we showed that cancer driver genes also accumulate more deleterious pLoFs, which may be useful to detect novel driver genes. We have checked with a number of cancer center researchers for interest in our pipeline Lajos Pusztai (director of Yale Cancer Center Genetics, Genomics and Epigenetics Program) and Mark Rubin (Weill Cornell Medicine), and they have expressed enthusiasm. |

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| **Reviewer comment 2.11a** | 5a. The authors have a tendency to be simplistic in places: For  example: the statement 'premature stop codons in the last exon are not subject to NMD', is an oversimplification. |
| **Authors’ response** | We agree with the reviewer. NMD doesn't always occur in this case. We've slightly changed the wording. |
| **Changes in text** | Change to “generally”. |

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| **Reviewer comment 2.11b** | 5b. And, "The effect of such variants is the production of truncated proteins that are functional." needs to be tempered as it seems to state unequivocally that all variants in the last exon or after the last reported disease-causing variant will produce a functional truncated product, which is not true. |
| **Authors’ response** | We agree with the reviewer on the simplistic verbiage and have modified the text appropriately. |
| **Changes in text** | The effect of such variants could be the production of truncated proteins that are likely sufficiently functional. |

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| **Reviewer comment 2.11c** | 5c. Similarly, the authors mention that prediction at the clinical level  is confounded by issues such as penetrance, expressivity but then say. 'Therefore, we do not expect to observe any LoF variant in NF2 in the presumed healthy individuals'. In fact healthy individuals can have NF2 LoFs, particularly if sampled at younger ages, as many of the features are non-specific and have insidious onset, only about half get tumors, usually benign ones.  'While the occurrence of LoF variants in known disease genes in  healthy individuals might be surprising' - It has been known and  extensively documented for over 20 years that healthy individuals can carry LoFs in disease genes. It may be unexplained but it should not be surprising. |
| **Authors’ response** | We agree with the reviewer about NF2 and have removed this example to illustrate the point that pLoFs in disease genes that occur in presumed healthy individuals could be isoform-specific. We observed healthy individuals can carry LoFs in known disease genes. Besides, issues like penetratrance, we show that isoform-specific truncation may be another reason for this observation. We understand this is not the only explanation, as there are many other factors, stated in the manuscript: incomplete penetrance of disease alleles, variable expressivity, compensatory mutations, marginal variant calls and etc.. |
| **Changes in text** | “The estimation and disease-causing potential of deleterious premature stop alleles can be affected by a number of confounding factors that include incomplete penetrance of disease alleles, variable expressivity, compensatory mutations, marginal variant calls and imperfect training datasets.” |

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|  | **Reviewer 2 Comments (Figures)** |
| **Reviewer comment 2.12** | 1. Figure 2b - the clinical used transcript of NF2 is defined (RefSeq  NM\_000268.3) and should be identified as such, as this most likely  corresponds to those reported in HGMD and displayed in the figure. |
| **Authors’ response** | Due to concerns raised about this figure, we have removed this Figure now and included a new supplementary Figure xxx. |
| **Changes in text** |  |

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| **Reviewer comment 2.13** | 2. Figure 3a - this figure and accompanying description in the text essentially show that HGMD and exome datasets will differ in reporting variants at ends of genes; surely this is expected due to the very different nature of how these datasets are collected? |
| **Authors’ response** | We agree with the reviewer that this is expected. |

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| **Reviewer comment 2.14** | 3. Figure 4a - the contrast between the dominant score and the GERP and CADD scores is striking. As a minor point, it would be good to note in either the legend or figure something about the  recessive/tolerated score. Did the recessive variants also have a low  tolerated and thus high recessive score, as one might automatically  assume? |
| **Authors’ response** | Yes, the recessive variants also have low benign scores and high recessive scores. We have added in the legend that the recessive variants also have low tolerated scores. We also tested the dominant vs. recessive trend using ClinVar, shown as new Figure 4a. The CMG figure is moved to the supplement due to the low number of variants tested (requested by other reviewers). |
| **Changes in text** | Change made in the Figure 4a legend. |

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| **Reviewer comment 2.15** | 4. Figure 4c - minor point, is the labelling of the y-axis correct?  I.e. 0.14% of somatic variants with high disease-causing score were in  known cancer genes? Or should the range be from 0 to 14? |
| **Authors’ response** | We thank the reviewer for pointing out this error. We have made the appropriate correction. |
| **Changes in text** | Correction made. |

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|  | **Reviewer 2 Comments (Methods)** |
| **Reviewer comment 2.16** | 1. Return of near\_start, near\_end defined as within first/last 5% of  gene - is this appropriate? Does it lend undue weight to an arbitrary  value? If not an arbitrary value, please provide justification for  selection of 5%. |
| **Authors’ response** | The 5% value is chosen as it has been shown in MacArthur et al that there is an enrichment of pLoFs at either end of the genes. We have also seen this trend in 1KGP1 as well as ESP6500 and ExAC datasets. However, given that these are simplistic filters, we wish to bring to the attention of the reviewer that we do not use this as a feature in our prediction algorithm. This is part of the annotation pipeline and is only an annotation flag that provides the user information about the pLoF. |

### Reviewer 3:

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|  | **Reviewer 3 Comments** |
| **Reviewer comment 3.1** | I have a number of technical concerns. The most important concern is that this works attempts to solve two problems at once: 1) predict whether LoF variants in a given gene are pathogenic, and 2) predict whether a particular putative LoF mutation is truly a LoF. These two problems require differentapproaches to development and validation of a classifier. For the combined problem, the proposed classifier likely results in overfitting due to simple labeling. |
| **Authors’ Response** | We understand the reviewer’s criticism about making the distinction between determining a molecular loss of function from its effect on phenotype. ALoFT has been developed to address point 1: inferring the pathogenic effect of pLOF variants. While elucidating effects at a molecular level is feasible to some extent, as we have noted above, understanding the exact mechanism can be tricky (LoF, GoF, change of function etc). Moreover, in practical terms we are ultimately interested in understanding the effect of genotype on phenotype. Therefore, to date, all prediction programs (there are numerous such programs for missense variants) developed to understand genetic variations have been trained on benign versus disease models. ALoFT elucidates the pathogenic potential of pLoF variants**.** ALoFT is similar to missense variant prediction programs where variants are classified as benign or pathogenic, but not whether it leads to LoF, GoF etc. Compared to missense variants, the likelihood that pLOF variants will affect the phenotype by molecular loss-of-function is much higher. Moreover, we also maximized the probability of modeling LoF effects by choosing variants in haploinsufficient genes as a model for the dominant class. This will minimize errors that might arise due to inclusion of gain-of-function mutations in the training set. However, we don’t believe it is essential to model molecular LoF. Rather we are more interested in understanding the effect of pLoF variant on the phenotype. To this end, we also trained our model based on variants in genes known to cause disease due to dominant mode of inheritance. We show that ALoFT has good predictive power (AUC = 0.934), albeit a bit lower than the classifier trained on haploinsufficient genes (please check Supplementary table 4). |

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|  | **Reviewer 3 Comments (Minor)** |
| **Reviewer comment 3.2** | 1. Figure 1 shows the ALoFT pipeline, but does not specify what the  output data is. Please specify the "output" in more detail; i.e. the  authors could certainly mention the LoF scores in the Figure 1 output.  The authors should also discuss which LoF scores are indicators for  pathogenicity, as mentioned above |
| **Authors’ response** | We agree with the reviewer. We have modified Figure 1 and the legend to include more details of the output. ALoFT will output annotations for all pLoFs (premature stop, frameshift indel and splice variants) and also give pathogenicity scores and predicted pathogenicity (benign, recessive or dominant) for premature stop and frameshift indels. We also modified the text to clarify the pathogenicity indicator of LoF scores. |
| **Changes in text** | “The prediction output provides three scores for each pLoF variant that correspond to the probability of the pLoF being benign, dominant or recessive disease-causing allele. In addition, ALoFT also provides the predicted pathogenicity. The pathogenic effect of pLoF variant is assigned to the class that corresponds to the maximum score.” |

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| **Reviewer comment 3.3** | References 6 and 7 are letters to the editor that can be deleted.  Note that a treatment to knock-down APOC3 has been reported – see PMID: 25470695, which could be cited to parallel the structure of the discussion of PCSK9 LoF variants. |
| **Authors’ response** | We thank the reviewer for pointing this out. We have updated the reference and text accordingly. |
| **Changes in text** | Other examples include nonsense and splice mutations in APOC3 associated with low levels of circulating triglycerides |

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| **Reviewer comment 3.4** | Please ensure that all gene annotations are in italics |
| **Authors’ response** | We have modified the text. |

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| **Reviewer comment 3.5** | Supplementary Tables 4 and 7 seem absent from the Supplement. |
| **Authors’ response** | Supplementary Tables 4 (now supplementary table 6) and 7 (now supplementary table 9) were uploaded as additional files. Given that they are big files, we did not include it in the Supplementary pdf. |
| **Changes in text** |  |

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| **Reviewer comment 3.6** | Page 1 (space): ....Finnish population in LPA that protect against  coronary > ....Finnish population in LPA that protect against coronary |
| **Authors’ response** | We have modified. |
| **Changes in text** |  |

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| **Reviewer comment 3.7** | Please check for consistency in the manuscript and figures that you  refer to premature stop mutations (no capital S in stop). |
| **Authors’ response** | We have modified. |
| **Changes in text** |  |

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| **Reviewer comment 3.8** | Figure 1: transcript-specifc > transcript specific |
| **Authors’ response** | Modified accordingly. |
| **Changes in text** |  |

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| **Reviewer comment 3.9** | Figure 1, legend: The authors mention that ALoFT can also be used  to annotate a 5-column tab delimited file. I wonder whether the  position needs to be according to a specific genome browser version? |
| **Authors’ response** | Currently we support GRCh37 (hg19) and make it clear in the supplement and website. |
| **Changes in text** |  |

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| **Reviewer comment 3.10** | "Current estimates of genetic burden of disease alleles in an  individual vary widely, ranging from 1.1. recessive alleles per  individual to 31 deleterious alleles33-37. It should be noted that the  prediction can be affected by a number of confounding factors that  include incomplete penetrance of disease alleles, variable  expressivity, compensatory mutations, marginal variant calls and  imperfect training datasets." Please, note that the whole genome/exome has not been sequenced in all referenced studies, which further impacts the predicted number of deleterious alleles. |
| **Authors’ response** | [[Do we want to include 1KG Phase 3 results ????  observed in Phase 3: HGMD-DM: median: ~16; ClinVar: median: ~28]] |
| **Changes in text** |  |

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| **Reviewer comment 3.11** | Figure 2a: This figure and its legend are somewhat confusing at  first glance.  - Please, explain what Benign LoFs are in the legend.  - Please rename 1KGP1 LoFs (other) > 1KGP1 LoFs in non-HGMD genes  - Please rename 1KGP1 LoFs in Genes > 1KGP1 LoFs in HGMD genes  - Please rename HGMD LoFs in Genes > Genes with LoFs in 1KGP1 and HGMD  - Please rename HGMD LoFs (other) > Genes with HGMD LoFs only |
| **Authors’ response** | We thank the reviewer for the suggestions. We have modified with Figure 2 (now Figure 4) and the legend. |
| **Changes in text** |  |

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| **Reviewer comment 3.12** | Page 3: "Secondly, some variants predicted to be benign in 1KGP1  occur in the last exon or later in the protein-coding transcript  relative to the disease-causing variant in the same transcript. The  effect of such variants is the production of truncated proteins that  are functional" > suggest > Secondly, some variants predicted to be  benign in 1KGP1 occur in the last exon or later in the protein-coding  transcript relative to the disease-causing variant in the same  transcript. The effect of such variants is the production of truncated  proteins that are sufficiently functional. |
| **Authors’ response** | The reviewer is correct. Those variants might produce functional products. |
| **Changes in text** |  |

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| **Reviewer comment 3.13** | Figure 4a: Please include common variants into the graphs as references. |
| **Authors’ response** | We have included common variants from 1KGP1, ExAC and ESP6500. |
| **Changes in text** |  |

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| **Reviewer comment 3.14** | Page 5: "When multiple cancer genomes are not available, somatic  LoFs that are predicted to be disease-causing by ALoFT can be used to  identify potential tumor suppressors." Can the authors indicate what  ALoFT score would indicate that a somatic mutation is (likely) a tumor  suppressor? |
| **Authors’ response** | Here we consider pLoFs that are more deleterious to be more likely cancer-related. Similar concept as OncodriveFM is used to test for functional bias. The score used would be 1 – benign score (that is the probability to be pathogenic). |
| **Changes in text** |  |

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| **Reviewer comment 3.15** | Supplement 1.1. Functional features: "The 3D structure of the  protein is essential for proper folding and function of proteins." >  The 3D structure of a protein is essential for proper folding and  function of proteins.  Supplement 1.1. "annotated based on data from PhosphositePlus 3" > annotated based on data from PhosphositePlus3  "For all functional features, we assessed 1. Does the premature  stop variant affect a functional feature? 2. Are..." > For all  functional features, we addressed the following questions: 1) Does the  premature stop variant affect a functional feature? and 2) Are... |
| **Authors’ response** | We have modified it. |
| **Changes in text** |  |

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| **Reviewer comment 3.16** | "We also identified transcripts containing a premature Stop as  candidates for nonsense-mediated decay (NMD) if the distance of the  premature Stop from the last exon-exon junction was greater than 50  base pairs." Please, provide a reference to support the choice of 50  bp. |
| **Authors’ response** | We have included three references regarding to the 50bp choice. |
| **Changes in text** |  |

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| **Reviewer comment 3.17** | "We also identified transcripts containing a premature Stop as  candidates for nonsense-mediated decay (NMD) if the distance of the  premature Stop from the last exon-exon junction was greater than 50  base pairs." This Supplemental 1.1. section is about "Functional  features" and then ends with this NMD sentence. Why is this sentence  incorporated in a section about functional features? The rest of the  section discusses disruptions of protein domains. |
| **Authors’ response** | [[SB ???] |
| **Changes in text** |  |

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| **Reviewer comment 3.18** | Supplement 1.4 Mismapping errors. Please, provide the reader with  insights into how many 1) segmentally duplicated regions there are in  the genome?; 2) how many genes have paralogs?; and 3) how many genes have pseudogenes? |
| **Authors’ response** | [[SB ??? ]] |
| **Changes in text** |  |

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| **Reviewer comment 3.19** | Supplement 1.5 Annotation errors. Generally the authors poorly  support based on what they annotate as parameters in a-f to  distinguish promising from false positive LoFs. Please, provide  explanations/references in this section to help the reader understand  why you have taken these cutoffs. |
| **Authors’ response** | [[SB ???] |
| **Changes in text** |  |

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| **Reviewer comment 3.20** | Supplement 1.5.a. "lof\_anc: Indicates that the LoF variant allele  is the same as the ancestral allele and is likely to be a functional allele." I do not completely understand what you mean here. Is the LoF  in this case the stop codon that is normally used?! Please, explain  more clearly. |
| **Authors’ response** | Yes, lof\_anc means that the stop codon is the ancestral allele, which is normally used based on the evolution. |
| **Changes in text** |  |

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| **Reviewer comment 3.21** | In Section 2. Pathogenicity prediction for LoF mutations, you  could refer to section 2.1. when you mention that "benign variants  were detected from 1KGP1". That said, I do wonder about the occurrence  of these variants in ExAC? What is the frequency of benign variants?  How sure can one be that these variants are truly benign? |
| **Authors’ response** | ???  We considered homozygous variants in 1KG as benign. As they are homozygous in healthy individual, they do not cause any phenotypic changes. Usually they have very high allele frequency in healthy populations. Majority of them should be benign. |
| **Changes in text** |  |

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| **Reviewer comment 3.22** | Section 2 (and Supplementary Table 3): "In total, we used 106  features to train our model.  (<http://aloft.gersteinlab.org/features/#prediction_features>)". Suggest  that the excel file of these 106 features be included as a  supplementary excel document. The reader should have access to this  information through Nature Genetics. |
| **Authors’ response** | We have included this in the supplementary table 2. |
| **Changes in text** |  |

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| **Reviewer comment 3.23** | Section 2.2. Three-class classification. "The average number of  dominant mutations per gene is 20." What is this value of 20 based on?  Please, provide an explanation or reference. Also, I wonder whether it  is a statistically validated method to pick 3 variants per gene for  the dominant class? |
| **Authors’ response** | This is based on the HGMD database. We have updated to a newer version of HGMD. In the HGMD, there are 3300 pLoFs in 136 classified dominant genes. In average there are 24 LoFs per gene.  Because of the imbalance of training sets, we pick average 3 variants per gene for the dominant class to match the benign sets (380 genes). For cross-validation, we specifically avoid splitting variants from the same dominant genes into training and testing. |
| **Changes in text** |  |

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| **Reviewer comment 3.24** | Supplementary Figure 2: Please, add in Supplementary Figure 2 at  the Y-axis that this is the Precision score, and explain more clearly  what you define as a 'true positive' and a 'false positive'. |
| **Authors’ response** | We have added accordingly. |
| **Changes in text** |  |

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| **Reviewer comment 3.25** | Supplementary Table 2: Please, explain the reasoning for removing  olfactory receptors, randomly picking transcripts and analysis of all  dominant genes in the Table legend. |
| **Authors’ response** | We tested the robustness of our method regarding to different training sets. We suspect olfactory receptor genes and picking the longest transcript might bias our results. Base on Supplementary Table 2 (now Supplementary Table 4), they don’t have impact the method.  In addition to LoF effects, truncating mutations can also lead to gain of function. However, gain of function mutations are difficult to model systematically as the effect of variant is very context dependent. It depends on the biological context of the gene that can vary widely. In order to minimize errors that might arise due to inadequate modeling of GoF effects and focus only on LoF, we chose to only use predicted haploinsufficient genes as the dominant training set.  In Supplementary Table 4, setting 4, we tested the influence of not using haplo-insufficiency filter. |
| **Changes in text** |  |

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| **Reviewer comment 3.26** | Section 2.3.1. Applied to known disease-causing mutations from the  Center for Mendelian Genomics studies  (<http://data.mendelian.org/CMG/>). Why did the authors use CMG data and  not much larger databases such as HGMD or LOVD to support their ALoFT zygosity claims? |
| **Authors’ response** | We realize that the number of testing variants is small. To address the reviewer’s point about the robustness of the approach, we used ALoFT to classify pathogenic variants from ClinVar that do not overlap with the training variants. This is now shown as new Figure 4a. CMG figure is moved to the supplement as supplementary figure 4 |
| **Changes in text** |  |

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| **Reviewer comment 3.27** | Supplementary Figure 4: Distribution of predicted dominant and  recessive premature stop alleles in the 1KGP1 individuals. The authors  say that they made their calculations based on 246 individuals of  African ancestry and 379 individuals of European ancestry. Together  these numbers represent only 625 genomes of the 1000. Why did the  authors not include more individuals? |
| **Authors’ response** | When comparing 1KG and ESP6500 call sets, we found there are many variants with >1% frequency in either European or African American population of the 1KG that are absent in the ESP6500 cohort. We suspect these might be erroneous calls in 1KGP1. (As we know, LoFs prone to have more false positives). When calculating the per individual statistics, we filter those variants out based on ESP6500, thus per individual statistics are only based on individuals that have the same ancestry as ESP6500 – African and European. |
| **Changes in text** |  |

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| **Reviewer comment 3.28** | Supplementary Table 6, legend: "de novo" is not consistently  displayed in italics |
| **Authors’ response** | We modified it. |
| **Changes in text** |  |