DoDo Learning: DOmain-DemOgraphic Transfer in Language Models for Detecting Abuse Targeted at Public Figures

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Abstract

Public figures receive a disproportionate amount of abuse on social media, impacting their active participation in public life. Automated systems can identify abuse at scale but labelling training data is expensive, complex and potentially harmful. So, it is desirable that systems are efficient and generalisable, handling both shared and specific aspects of online abuse. We explore the dynamics of cross-group text classification in order to understand how well classifiers trained on one domain or demographic can transfer to others, with a view to building more generalisable abuse classifiers. We fine-tune language models to classify tweets targeted at public figures across DOmains (sport and politics) and DemOgraphics (women and men) using our novel DoDo dataset, containing 28,000 labelled entries, split equally across four domain-demographic pairs. We find that (i) small amounts of diverse data are hugely beneficial to generalisation and model adaptation; (ii) models transfer more easily across demographics but models trained on cross-domain data are more generalisable; (iii) some groups contribute more to generalisability than others; and (iv) dataset similarity is a signal of transferability.

Content Warning: We include some synthetic examples of the dataset schema to illustrate its contents.

Data Release Statement: Due to institutional guidelines concerning privacy issues surrounding the release of Twitter data, we are unable to release the DoDo dataset.

1 Introduction

Civil discussion between public figures and citizens is a key component of a well-functioning democratic society (Dewey, 1927; Rowe, 2015; Papacharissi, 2004). Social media has opened new channels of communication and permitted greater access between users and public figures (Doidge, 2015; Ward and McLoughlin, 2020); becoming an important tool for self-promotion, message spreading and maintaining a dialogue with fans, followers or the electorate (Farrington et al., 2014), beyond traditional media gatekeeping (Coleman, 1999, 2005; Coleman and Spiller, 2003; Williamson, 2009). However, there is a cost: the immediacy, ease and anonymity of online interactions has routinised the problem of abuse (Suler, 2004; Shulman, 2009; Brown, 2009; Joinson et al., 2009; Rowe, 2015; Ward and McLoughlin, 2020). Public figures attract more intrusive and abusive attention than average users of online platforms (Mullen et al., 2009; Meloy et al., 2008), and abuse directed towards them is both highly-public yet often grounded in highly-personal attacks (Erikson et al., 2021). There are detrimental effects both to individual victims' mental health, which can ultimately result in their withdrawal from public life (Vidgen et al., 2021a; Delisle et al., 2019), and to society as a whole from normalising a culture of abuse and hate (Ingle, 2021). Disengagement is particularly worrisome for the functioning of democracy and political representation as it might be spread unevenly across different groups (Theocharis et al., 2016; Greenwood et al., 2019; Ward and McLoughlin, 2020), for example female representatives being more likely to give up on politics than their male counterparts (Manning and Kemp, 2019).

Tackling abuse against public figures is a pressing issue, but the volume of social media posts makes manual investigations challenging, and conclusions drawn from anecdotal self-reporting or small sample size surveys offer limited and potentially biased coverage of the problem (Ward and McLoughlin, 2020). Automated systems based on machine learning or language models can be used to classify text at scale, but depend on la-

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belling training data which is complex, expensive to collect and potentially psychologically harmful to annotators (Kirk et al., 2022c).

In this context, it is highly desirable to develop general abuse classifiers that can perform well across a range of different abuse types whilst being trained on a minimal 'labelling budget'. However, this may be technically challenging because, while some properties of abuse are shared across settings, different *domains* (e.g., sport, politics or journalism) introduce linguistic and distributional shifts. Furthermore, previous reports reveal that the nature of online abuse is heavily influenced by the identity attributes of its targets, for example gendered abuse against female politicians (Bardall, 2013; Stambolieva, 2017; Erikson et al., 2021; Delisle et al., 2019); so, learnings from different *demographics* may also not transfer.

Despite the promise of generalisable abuse models for protecting more groups from harm, only a limited amount of research has addressed the extent to which models trained in one context can transfer to another. In this paper, we ask how well classifiers trained on one domain or demographic transfer to others, with a view to building more generalisable models that achieve high performance on areas they have been exposed to during training but also perform well on those they have not seen until inference time. Our novel DoDo dataset is collected from Twitter and contains tweets targeted at public figures across two DOmains (UK members of parliament or "MPs" and professional footballer players) and DemOgraphic groups (women and men). Tweets are annotated with four fine-grained labels to disambiguate abuse from other sentiments like criticism. We present results from experiments exploring the impacts of data diversity and quantity on domain-demographic transfer and generalisability.

2 Dataset

2.1 Data Collection

Our data is collected from Twitter. While generally over-researched (Vidgen and Derczynski, 2020), Twitter is a dominant source for interactions between public figures and the general public in the United Kingdom. Most MPs have Twitter accounts and Twitter activity may even have a small impact on elections (Bright et al., 2020).

We compile lists of accounts for UK MPs (590 accounts, 384 men, 206 women) and for players from England's top football divisions (808 from the Men's Premier League, 216 from the Women's Super League). We use the streaming endpoint of v1.1 Twitter API to collect all tweets directed at these accounts (which we label as "audience contact" tweets) in real-time into an unlabelled pool (see Table 1). We define "audience contact" as i) replies directly to a public figure, or ii) top-level tweets that mention a public figure.[1] For women footballers, we also use the full-archive search endpoint due to insufficient real-time data. We only retain tweets in English that contain some text content aside from URLs and tagged mentions of Twitter users.

2.2 Data Sampling

Levels of abusive content 'in-the-wild' are relatively low (Vidgen et al., 2019). In order to evaluate classifiers on realistic distributions while maximising the ability of those classifiers to detect abusive content, we randomly sample the test and validation datasets (preserving real-world class imbalance) but apply boosted sampling for the training dataset (ensuring the model sees enough instances of the rarer abusive class). For each domain-demographic pair, starting with our unlabelled pool, we randomly sample (and remove) 3,000 entries for the test set and 1,000 entries for the validation set. We then randomly sample (and remove) 1,500 entries for training and concatenate these with a further 1,500 entries containing a keyword from a list of 731 abusive and hateful keywords (750 entries with at least one profanity keyword and 750 with at least one identity keyword).[2] Each training set has 3,000 entries in total.

2.3 Data Annotation

We employ crowdworkers and expert annotators to label the sampled datasets.[3] There are 4 classes of sentiment expressed towards public figures: Positive, Neutral, Critical, or Abusive, as defined below.[4] Figure 1 contains synthetic example tweets across domain-demographic pairs and classes.

[1]A similar approach is adopted in prior work that tracks public figure abuse (Gorrell et al., 2020; Ward and McLoughlin, 2020; Rheault et al., 2019).

[2]Compiled from Davidson et al. (2017); ElSherief et al. (2018); Vidgen et al. (2021b); Kirk et al. (2022b), available at github.com/Turing-Online-Safety-Codebase/dodo-learning.

[3]A data statement (Bender and Friedman, 2018) is available in the dodo-learning repository.

[4]Labels are assigned based on the use of language, not the target of sentiment expressed.

Domain	Demographic	Pool Size	Collection Dates		Collection Method	
			Start	*End*	*Streaming*	*Search*
Footballers	Men	1,008,399	12/08/2021	02/02/2022	✓	
	Women	226,689	13/08/2021	28/11/2022	✓	✓
MPs	Men	1,000,000	13/01/2022	19/09/2022	✓	
	Women	1,000,000	13/01/2022	19/09/2022	✓	

Table 1: Dates and pool sizes for each domain-demographic pair.

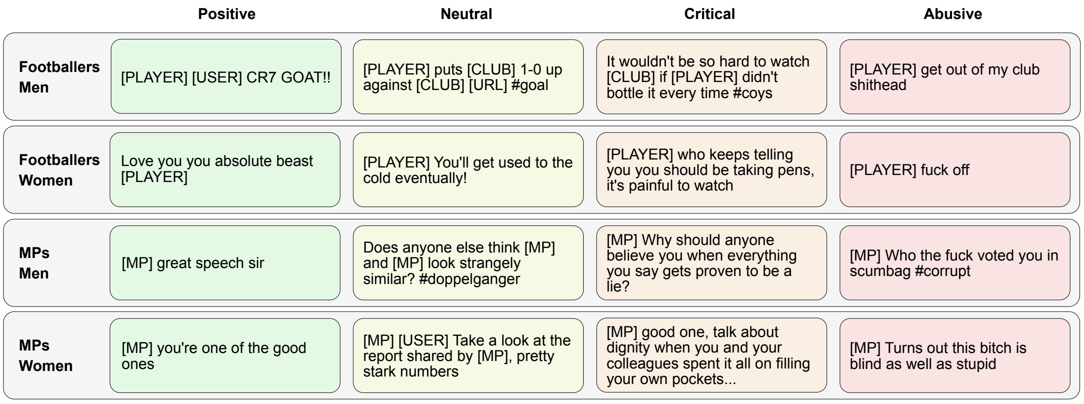

Figure 1: Synthetic example tweets for each class label, loosely based on topics and sentiment of content in the dataset. Entries from the dataset are presented to annotators as shown, with special tokens to represent tagged mentions of public figures, accounts representing affiliations (e.g., football clubs), and other users.

1. **Positive:** Language that expresses support, praise, respect or encouragement towards an individual or group. It can praise specific skills, behaviours, or achievements, as well as encourage diversity and the representation of identities.
2. **Neutral:** Language with an unemotive tone or that does not fit the criteria of the other three categories, including factual statements, event descriptions, questions or objective remarks.
3. **Critical:** Language that makes a substantive negative assessment or claim about an individual or group. Negative assessment can be based on factors such as behaviour, performance, responsibilities, or actions, without being abusive.[5]
4. **Abusive:** Language containing threats, insults, derogatory remarks (e.g., hateful use of slurs and negative stereotypes), dehumanisation (e.g., comparing individuals to insects, animals, or trash), mockery, or belittlement towards an individual, group, or protected identity attribute.

We use two different sets of annotators across the two domains, as we annotated the sets sequentially, and found high rates of annotator disagreement in initial annotation rounds, with a large number of entries requiring expert annotation as a result. We use the same label schema for all domain and demographic pairs but use specific example tweets in the guidelines. We only employ annotators who pass a test of gold questions.

We employed 3,375 crowdworkers for male footballers and 3,513 for female footballers. Each entry was annotated by 3 crowdworkers, with an additional two annotations required if no majority agreement ($\frac{2}{3}$) was reached, then sent for expert annotation if still no majority agreement ($\frac{3}{5}$) was reached. The average annotator agreement per entry was 68%, and the Cohen's kappa was 0.50.[6]

We employed 23 high-quality annotators for both MP datasets. Each entry received 3 annotations, then was sent for expert annotation if no majority agreement was reached ($\frac{2}{3}$). The average entry-wise agreement was 82% and the Cohen's kappa was 0.67.

2.4 Analysis

Terminology We abbreviate pairs of domain-demographic data as: fb-m (footballers-men), fb-w (footballers-women), mp-m (MPs-men), mp-w (MPs-women). We refer to any given domain-demographic pair as a `dodo`. We refer to groups of models that we train by the number of `dodos`

[5] The annotator guidelines focused on distinguishing between abuse and criticism. Criticism must include a rationale for negative opinions on an individual's actions (not their identity)—it is not a form of "soft" abuse.

[6] Calculated using the generalised formula from Gwet (2014) to account for variable # of annotations per entry.

Split	Stance	dodo							
		fb-m		*fb-w*		*mp-m*		*mp-w*	
Train	Abusive	867	*29%*	481	*16%*	1007	*34%*	870	*29%*
	Critical	475	*16%*	282	*9%*	1283	*43%*	1353	*45%*
	Neutral	647	*21%*	719	*24%*	605	*20%*	628	*21%*
	Positive	1011	*34%*	1518	*51%*	105	*3%*	149	*5%*
Test	Abusive	103	*3%*	43	*1%*	392	*13%*	373	*12%*
	Critical	377	*13%*	89	*3%*	1467	*49%*	1471	*49%*
	Neutral	811	*27%*	767	*26%*	985	*33%*	927	*31%*
	Positive	1709	*57%*	2101	*70%*	156	*5%*	229	*8%*
Validation	Abusive	33	*3%*	14	*1%*	140	*14%*	135	*13%*
	Critical	93	*9%*	45	*5%*	484	*48%*	459	*46%*
	Neutral	335	*34%*	267	*27%*	332	*33%*	337	*34%*
	Positive	539	*54%*	674	*67%*	44	*4%*	69	*7%*
Random	Abusive	181	*3%*	75	*1%*	744	*13%*	661	*12%*
	Critical	642	*12%*	197	*4%*	2676	*49%*	2676	*49%*
	Neutral	1677	*30%*	1466	*27%*	1788	*33%*	1741	*32%*
	Positive	3000	*55%*	3762	*68%*	292	*5%*	422	*7%*

Table 2: Tweet counts across splits, `dodos`, and stances, with percentages within the `dodo` split. Includes counts and percentages for tweets from all splits selected by random sampling before annotation (5,500 tweets total per `dodo`).

included in the training data: `dodo1` for models trained using one domain-demographic pair, `dodo2` for models trained using two pairs, etc.

Overview The total dataset has 28,000 annotated entries, 7,000 for each `dodo` pair, with 3K/3K/1K test/train/validation splits. Table 2 shows class distributions across splits and counts of tweets sampled randomly pre-annotation.

Class Distributions The last row of Table 2 contains the randomly sampled entries across each dataset (ignoring keyword sampled entries which would skew the distributions). The majority of tweets in the MPs datasets are either abusive or critical, in contrast to the footballers datasets where the majority class is positive, especially for fb-w. We also see slightly higher proportions of abusive tweets targeted at male demographic groups (fb-m, mp-m). Investigating these differences is outside the scope of this paper, but it is notable how levels of abuse vary.

Tweet Length The MPs data contains longer tweets on average than the footballers data (125 vs. 84 characters), and has over twice as many tweets $\geq$ 250 characters (1,632 vs. 556 tweets). Many of these longer tweets for MPs are critical, implying the presence of detailed political debate.

3 Experiments

We conduct experiments to study how well model performance transfers across domains and demographics, and how the quantity and diversity of training data affects model generalisability. To reflect the focus on generalisability, we evaluate models on: (i) "seen" `dodos` (`dodos` used in training); (ii) "unseen" `dodos` (`dodos` not used in training)[7]; and (iii) the total evaluation set (including evaluation splits from all `dodos`). We train models on data from combinations of `dodo` pairs, and experiment with continued fine-tuning on the resulting models. We repeat experiments across 3 random seeds and 2 labelling budgets. We make predictions using the total test set (12,000), and calculate mean and standard deviation of Macro-F1 across the seeds. The Macro-F1 score represents a macro-average of per class F1 scores, accounting for class imbalance. We also investigate the correlation of Macro-F1 with dataset similarity.

Models We fine-tune deBERTa-v3 (**deBERT**, He et al., 2021) on a Tesla K80 GPU for 5 epochs with an early stopping patience of 2 epochs using Macro-F1 on the validation set.[8]

Dodo Combinations Our dataset has four `dodo` pairs, each with 3,000 training entries. There are 15 combinations of these pairs (if order does not matter): four single pairs (`dodo1`), six ways to pick two pairs (`dodo2`), four ways to pick three pairs (`dodo3`) and all pairs (`dodo4`). For all combinations, we randomly shuffle the concatenated training data before any training commences.

Labelling Budget For each model and training combination, we make two budget assumptions. In the **full budget** condition, we just concatenate the training sets: 3,000 training entries for `dodo1` experiments; 6,000 for `dodo2` experiments; 9,000 for `dodo3`; and 12,000 for `dodo4`. In the **fixed budget** condition, we assume the train budget is fixed at 3,000 entries and allocate ratios according to the `dodo` combinations: each included `dodo` makes up 100% of the budget for `dodo1` experiments; 50% for `dodo2`; 33% for `dodo3`; and 25% for `dodo4`. This allows us to test the effects of training data composition without confounding effects of its size.

[7] All test sets are fully held out from training—by "seen" and "unseen" we only mean the domain or demographic.

[8] We also ran experiments on distilBERT (Sanh et al., 2019), but deBERTa-v3 had consistently higher performance, therefore we only present results for deBERTa-v3.

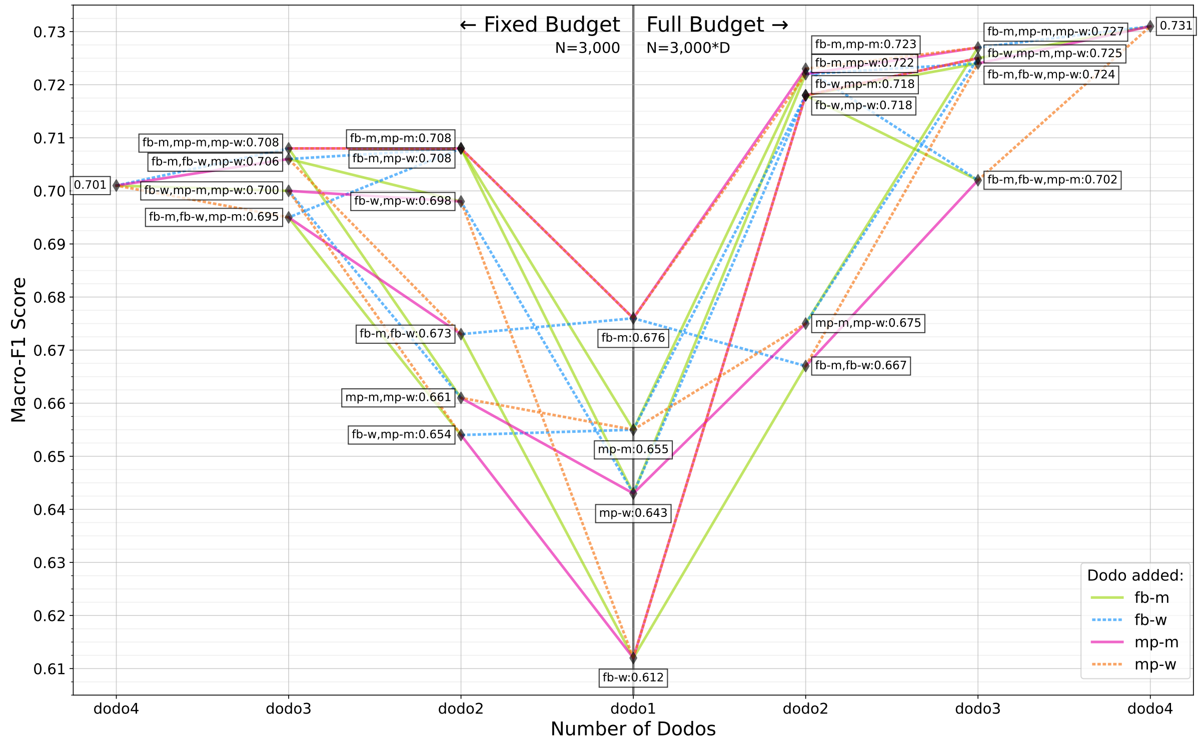

Figure 2: Performance of models trained on different `dodo` combinations and evaluated on the total test set (Macro-F1 averaged across seeds). We show both the fixed (train size = 3,000) and full budget (train size = 3,000 · number of `dodos`). The lines indicate which `dodo` is added at each step. Starting from the initial `dodo1` models, the progression of fixed budget models goes to the left, and full budget models to the right. To get from a `dodo1` model to a `dodo4` model requires an additional `dodo` to be added 3 times, meaning a line representing a journey from `dodo1` to `dodo4` will consist of 3 different coloured/patterned lines.

4 Results

4.1 Small amounts of diverse data are hugely beneficial to generalisable performance.

Figure 2 provides an overview of the performance of models trained on all combinations of `dodo` data. It illustrates that the increase in performance from adding data from new domains or demographics is not linear: the full budget `dodo2` models only attain a one percentage point (pp) average increase in Macro-F1 Score for an additional 3,000 training entries. We also see the two `dodo4` models are only separated by 3pp despite the full budget version being exposed to 4 times the amount of training data as the fixed budget version. This shows that gains from data diversity outweigh those from significantly greater quantities of data in training generalisable models.

4.2 Cross-demographic transfer is more effective than cross-domain.

Table 3 shows the comparisons for domain transfer and demographic transfer by Macro-F1 score on the seen and unseen portions of the test set, using the `dodo2` models. For domain transfer, training on footballers gives a 0.65 F1 on the footballers dataset and 0.58 F1 on the MPs dataset. This is symmetric with training on MPs and testing on footballers. For demographic transfer, training on the male pairs and testing on female pairs faces no drop in performance. In contrast, training on women and testing on men leads to a small reduction in performance on the male data. In general, this demonstrates that transferring across domains is more challenging than transferring across demographics while keeping the domain fixed.

Train on	**Test on**			
	Seen		*Unseen*	
fb-m; fb-w	Footballers	0.67	MPs	0.59
mp-m; mp-w	MPs	0.68	Footballers	0.56
fb-m; mp-m	Men	0.72	Women	0.72
fb-w; mp-w	Women	0.74	Men	0.70

Table 3: Cross-domain and cross-demographic transfer with mean Macro-F1 for `dodo2` models. We train on two `dodos` and evaluate on concatenated portions of the test set, e.g., we train *fb-w; fb-m* then test on *fb-w; fb-m* (seen) and *mp-m, mp-w* (unseen).

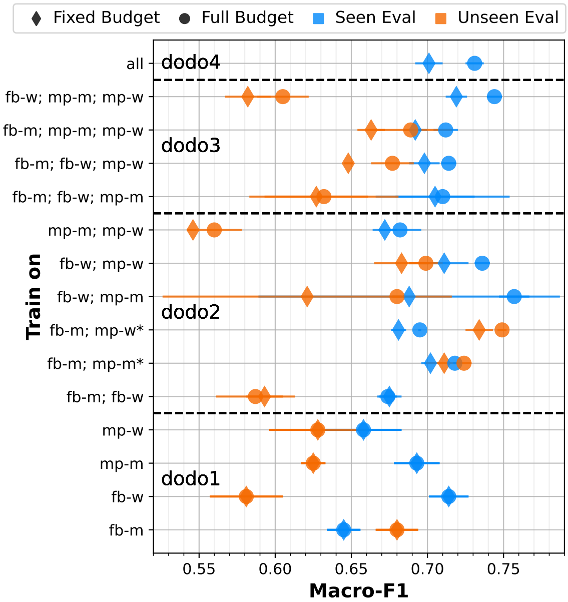

Figure 3: Mean and std-dev Macro-F1 across seeds for models trained on `dodo` combos, for fixed and full budgets, on test sets from seen and unseen `dodos`. *We removed one degenerate training seed (s=2).

4.3 Cross-domain within-demographic training produces more generalisable models than cross-demographic.

Figure 3 shows that, as expected, performance on test sets from seen `dodo` is generally higher than on those from unseen `dodos`. Within the `dodo2` models, cross-demographic within-domain models (e.g., `fb-m;fb-w`) perform 10pp better on average on seen `dodo` evaluation sets than unseen ones, compared to a much narrower gap of 1pp on average for cross-domain models (e.g., `fb-w;mp-w`). We also see from Figure 2 that cross-domain within-demographic `dodo2` models outperform all cross-demographic within-domain `dodo2` models on the total test set. These findings provide evidence that models trained on a single domain (e.g., just football data) struggle to deal with out-of-domain examples, and that cross-domain models are more generalisable.

4.4 Not all dodos contribute equally to generalisable performance.

The average Macro-F1 increase provided by including each `dodo` in training is summarised in Figure 4. We see that fb-m provides the biggest boost in a fixed budget scenario, and mp-w in a full budget scenario. In some cases, including fb-w data during training can detract from overall performance across both budget scenarios. A `dodo1` model trained only on fb-m also outperforms all other `dodo1` models on the total test set (see Figure 2), and fb-m data is included in the training dataset for the top ranking model for each dodo size across both labelling budgets. These findings suggest that training with fb-m is more important for good model generalisation than other `dodos`.

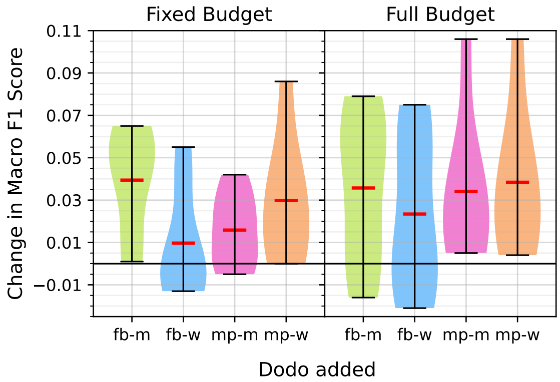

Figure 4: Violin plot displaying distribution of change in Macro-F1 score when adding a `dodo` to the training data (N=7), with mean represented by red marker.

We now consider the situation of leaving out one `dodo` pair during training. We compare this left out case (`dodo3`) to training on all pairs (`dodo4`) in Table 4. We show the change in Macro-F1 on the total test set and change in number of training entries. For the full budget, leaving out mp-w from training leads to the largest reduction in performance. In contrast, removing all fb-w or mp-m entries does not significantly degrade performance even with 3,000 fewer training entries. For the fixed budget setting (with no confounding by training size), leaving out the two male pairs leads to a larger drop in performance, than leaving out two female pairs.

	Raw size		**Fixed size**	
	Δ F1	**Δ N**	**Δ F1**	**Δ N**
all dodos	0.731	12,000	0.701	3,000
leave out fb-m	-0.006	-3,000	-0.001	0
leave out fb-w	-0.004	-3,000	0.007	0
leave out mp-m	-0.007	-3,000	0.005	0
leave out mp-w	-0.029	-3,000	-0.006	0

Table 4: Comparing model trained on all pairs (`dodo4`) with models trained on 3 pairs (`dodo3`). Shows relative change in mean Macro-F1 on total test set, and relative change in N of training entries.

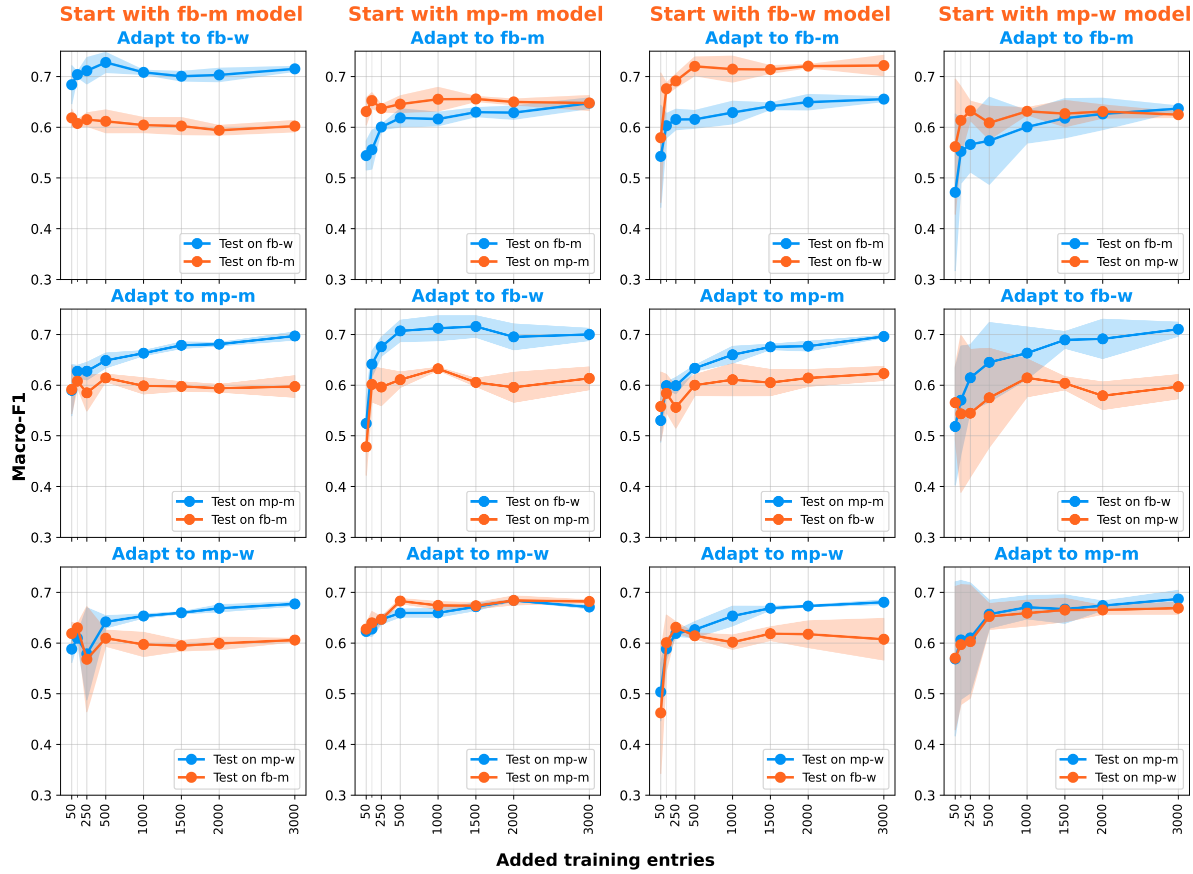

Figure 5: Learning curves for starting with a dodo1 model trained on a single dodo pair and adding increments from the training set of a new dodo pair. We show mean and std-dev Macro-F1 (across 3 seeds) on the new adapt dodo and source start dodo at each increment.

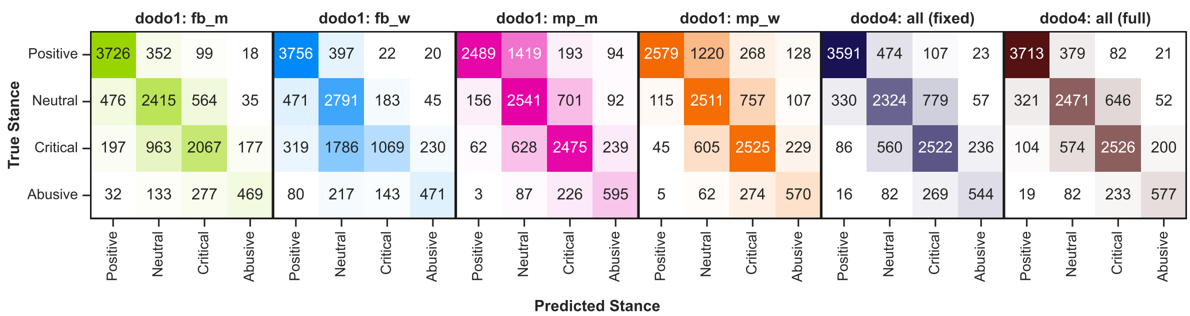

Figure 6: Confusion matrices for dodo1 and dodo4 models evaluated on the total test set (12,000 entries).

4.5 Only small amounts of data are needed to effectively adapt existing models to new domains and demographics.

The final experiment assumes we *start* with a fine-tuned specialist dodo1 model (i.e., a model fine-tuned on a single dodo) and wish to *adapt* this model to a new dodo. We take each fine-tuned dodo1 model (with three seeds) and do continued fine-tuning on increments added from the adapt dodo train split.[9] For the models trained using each budget increment, we calculate Macro-F1 on test sets of both the start and adaption dodos (see Figure 5) so that we record both performance gains in adapting to new dodos alongside performance losses (forgetting) in seen dodos.

For almost all cases, the performance gain is notable after adding just 125 entries from the new dodo and increases with more entries. There is not a prominent performance gain after 500 entries except when adapting from fb-m to mp-m. This suggests that a small amount of data is efficient and cost-effective for testing how well ex-

[9]The increments are [50, 125, 250, 500, 1000, 1500, 2000, 2500, 3000]. We train a separate model for each increment.

isting models generalise. The importance of data composition over data quantity aligns with the fixed/full budget findings from §4.1. On catastrophic forgetting, we generally do not find major performance drops. In some cases, adapting models to new data even helps classification in the source pair (e.g., mp-w to mp-m). Future work can explore where adaptation helps or hurts performance in source domains or demographics.

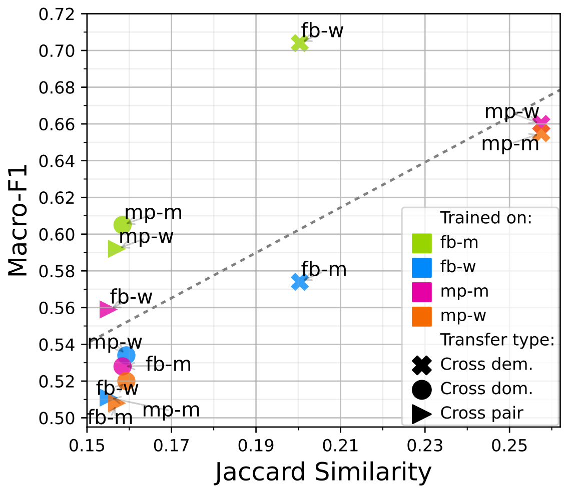

Figure 7: Jaccard similarity and mean 0-shot Macro-F1 for dodo1 deBERT models with line of best fit. On graph annotations represent evaluation dodo. Shows positive correlation ($\rho = 0.7$) and effectiveness of cross-demographic vs. cross-domain transfer.

4.6 Dataset similarity is a signal of transferability.

Using the specialist dodo1 models, we examine if dataset similarity signals transferability, i.e., the Macro-F1 score that a dodo1 model can achieve on unseen dodos. We compute three classical text distance metrics with unigram bag-of-words approaches: Jaccard and Sørensen-Dice similarity, and Kullback-Leibler divergence. In Figure 7, we plot Macro-F1 scores (of unseen single dodos) against Jaccard similarity for each pair of dodos. The correlation coefficient is 0.7, demonstrating a positive relationship between dataset similarity and unseen dodo performance.[10] Greater similarity between demographic pairs versus domain pairs results in better cross-demographic transfer versus cross-domain transfer. Using these metrics could help estimate transfer potential before investing in an expensive labelling process.

[10]Correlation coefficients are 0.7 for Dice Similarity and -0.66 for KL Divergence, confirming Jaccard robustness.

4.7 Error Analysis

Analysing errors made by each dodo1 model reveals that the fb-m model performs best on positive tweets and less well on abusive tweets, with a similar pattern for the fb-w model (see Figure 6). The mp-m model performs best on critical tweets and less well on positive and neutral tweets, with a similar pattern for the mp-w model.

These errors reflects the class imbalance outlined in §2.4, as well as some inherent similarity between classes which border one another i.e., positive vs. neutral, neutral vs. critical, and critical vs. abusive, demonstrating the value of fine-grained labels for identifying model weaknesses. Recurring errors reveal several types of tweet that are challenging to classify: tweets that (i) contain a mixture of both positive and critical language; (ii) use positive or sarcastic language to mock; (iii) rely on emoji to convey abuse; (iv) contain niche insults; or (v) short, ambiguous tweets that lack context.

5 Discussion

5.1 Limitations

Targets of Abuse It is sometimes hard to disentangle the target of sentiment in tweets directed at public figures—some tweets praise public figures while simultaneously criticising another figure or even abusing identity groups (such as an praising an MP's anti-immigration policy while abusing immigrants). Our label schema does not tag target-specific spans nor flag when it is a non-public figure account or abstract group is being abused. We also do not use further conversational context during annotation. Furthermore, we are limited by gender distinctions in UK MPs statistics and football leagues—the dataset does not cover non-binary identities or other identity attributes.

Types of Abuse While our dataset is more diverse than most abuse datasets in including four class labels, it does not disaggregate abusive content into further subcategories such as identity attacks. Our preliminary keyword analysis suggested that identity attacks comprise a relatively small proportion of all abuse (especially for female footballers) but can nonetheless cause significant harm. Further investigation on abuse across demographic groups is needed to understand how women and men are targeted differently, and to assess distributional shifts of specific homophobic, racist, sexist or otherwise identity-based abuse.

Language and Platform Focus Our dataset contains English language tweets associated with UK MPs and the top football leagues in England (though players come from a variety of nationalities). Prior studies suggest politicians face online abuse in other countries (Theocharis et al., 2016; Ezeibe and Ikeanyibe, 2017; Rheault et al., 2019; Fuchs and Schäfer, 2020; Erikson et al., 2021); and that the English football social media audience is a global one (Kilvington and Price, 2019). However, shifting national or cultural context will introduce further distributional and linguistic shifts. Furthermore, our data is only collected from Twitter though abuse towards public figures exists on a variety of social media platforms (Agarwal et al., 2021) such as YouTube (Esposito and Zollo, 2021) or WhatsApp (Saha et al., 2021).

Evaluation Robustness Aggregate evaluation metrics may obscure per `dodo` and per class weaknesses (Röttger et al., 2021). The Macro-F1 score across the combined test set from all `dodos` does not equal the averaged Macro-F1 across each `dodo` test set (the former is 4.7pp higher on average). This is due to different class distributions across `dodos` skewing the total Macro-F1 calculation. The ranking of models was consistent across these two metrics. We have not investigated the relative dataset difficulty (Ethayarajh et al., 2022) of individual `dodo` test sets, which may influence measures of generalisibility.

5.2 Areas of Future Work

Despite these limitations and the challenging nature of annotating social media content, our study provides ample opportunity for areas of future work. Expanding demographics and adding more complexity to the labelling schema would provide a broader basis for understanding generalisability in abuse classification. Other promising avenues include investigating whether active learning techniques (Vidgen et al., 2022; Kirk et al., 2022c) aid more efficient cross-domain/demographic transfer, or whether architectures better suited for continual learning can assist in the addition of new groups without forgetting those previously trained-on (Hu et al., 2020; Qian et al., 2021; Li et al., 2022). We shuffled entries during training and used all four class labels but future work could assess whether performance is affected by order of training on different groups, and the impact of training on binary versus multi-class labels on transfer performance. Finally, our experiments only use fine-tuning on labelled data, but in-domain continued pre-training could be explored as a budget-efficient way to boost performance (Gururangan et al., 2020; Kirk et al., 2023).

6 Related Works

Abuse Against MPs Academics and journalists account abuse against politicians, which may cause politicians to withdraw from their posts (Parliament, 2023; Manning and Kemp, 2019; James et al., 2016). Empirical work commonly studies Twitter (Binns and Bateman, 2018; Gorrell et al., 2020; Ward and McLoughlin, 2020; Agarwal et al., 2021), including across national contexts such as European Parliament elections (Theocharis et al., 2016), Canadian and US politicians (Rheault et al., 2019) and members of the UK parliament (Gorrell et al., 2020). Other studies focus on gender differences in abuse (Rheault et al., 2019; Erikson et al., 2021) though some datasets only contain abuse against women (Stambolieva, 2017; Delisle et al., 2019) which limits comparison across genders (unlike DoDo). Various techniques are employed to identify abusive tweets including rules-based or lexicon approaches and topic analysis (Gorrell et al., 2018, 2020; Greenwood et al., 2019); traditional machine learning classifiers (Stambolieva, 2017; Rheault et al., 2019; Agarwal et al., 2021) or pre-trained language models and off-the-shelf classifiers like Perspective API (Delisle et al., 2019).

Abuse Against Footballers Sport presents a good case for studying public figure abuse due to the influence of athletes (Carrington, 2012), as well as the heightened symbolic focus on in-out groups and race-nation relations (Bromberger, 1995; Back et al., 2001; King, 2003; Burdsey, 2011; Doidge, 2015). Several studies track the change from racist chants at football stadiums, to the more pernicious and harder to control online abuse (King, 2004; Cleland, 2013; Cleland and Cashmore, 2014; Kilvington and Price, 2019). Civil society organisations track social media abuse as far back as the 2012/2013 season, but are limited by a focus on manual case-by-case resolution and suffer from chronic underreporting (Bennett and Jönsson, 2017). The most similar work to our large-scale automated analysis is Vidgen et al. (2022), who use some of the same data as the male footballers portion in DoDo but also label additional data using active learning.

Abuse Datasets and Detection Developing robust abuse classifiers is challenging (Zhang and Luo, 2019). Surveys on abuse detection cover various aspects such as algorithms (Schmidt and Wiegand, 2017; Mishra et al., 2019), model generalisability (Yin and Zubiaga, 2021), and data desiderata (Vidgen and Derczynski, 2020). Many studies curate data from mainstream platforms, focusing on abuse against different identities such as women (Fersini et al., 2018; Pamungkas et al., 2020) and immigrants (Basile et al., 2019). Recent approaches to developing abuse classifiers predominately fine-tune large language models on labelled datasets directly (Fortuna et al., 2021) (our approach) or in a multi-task setting (Talat et al., 2018; Yuan and Rizoiu, 2022), as well as incorporate contextual information (Chiril et al., 2022). Abuse detection datasets mostly focus on binary classification, and few cast the predictions as a multi-class problem. Similarly, cross-domain classification is under-explored to address generalisability (Glavaš et al., 2020; Yadav et al., 2023). The dataset we use in this paper rectifies some of these issues, as a cross-domain and demographic dataset with fine-grained labels.

Domain Adaptation Several NLP techniques have been explored for model generalisation in abuse detection, including feature-based domain alignment (Bashar et al., 2021; Ludwig et al., 2022), regularisation methods (Ludwig et al., 2022), and adaptive pre-training (Faal et al., 2021). Systematic evaluation of model generalisability is limited, focusing on dataset features (Fortuna et al., 2021) and multilinguality (Pamungkas et al., 2020; Yadav et al., 2023). To our knowledge, no prior work has quantified the effects of cross-domain *and* cross-demographic transfer.

7 Conclusion

Using our new DoDo dataset, we trained language models to detect abuse against public figures for two domains (sports, politics) and two demographics (women, men). We found that (i) even small amounts of diverse data provide significant benefits to generalisable performance and model adaptation; (ii) cross-demographic transfer (from women to men, or vice-versa) is more effective than cross-domain transfer (from footballers to MPs, or vice-versa) but models trained on data from one domain are less generalisable than models trained on cross-domain data; (iii) not all domains and demographics contribute equally to training generalisable models; and (iv) dataset similarity is a signal of transferability.

There are broader policy implications of our work. Online harms are in constant evolution, both in terms of the targets and nature of abuse. Policymakers, NGOs and others with an interest in independently monitoring harms face a constant challenge in terms of building models that are both broad enough to capture a wide range of harms but also specific enough to capture the distinctive nature of abuse (e.g., the difference between hate speech targeted at male and female MPs), all whilst remaining within the tight resource constraints typical of policy settings. Our work contributes to the policy landscape by providing fresh perspective on the feasibility of transferring models created to detect harm for one target to other targets. It thus gives a roadmap and resources for developing automated systems that are cost-effective, generalisable and performative across domains and demographics of interest.

Ethics and Harm Statement

We present our limitations section in §5.1. In addition to these limitations, engaging with a subject such as online abuse raises ethical concerns. Here we set out the nature of those concerns, and how we managed them. Creation and annotation of a dataset focusing on abuse risks harming the annotators and researchers constructing the dataset, as repeated exposure to such material can be detrimental towards their mental health (Kirk et al., 2022a). Mitigating these risks is easier with a small trained team of annotators (like those we used for the MPs dataset) and harder with crowdworkers (like those we used for the FBs dataset). With the trained group of annotators, we maintained an open annotator forum where they could discuss such issues during the labelling process, and seek welfare support. For crowdworkers, we had very limited contact with them but include on our guidelines and task description extensive content warnings and links to publicly-available resources on vicarious trauma.

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