DCDM-ECG: Demographic-Conditional Diffusion Model for 12-Lead ECG Generation

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Abstract

Conditional 12-lead ECG generators are increasingly used as drop-in synthetic cohorts, yet they quietly distort patient demographics, producing cohorts that are systematically younger and demographically narrower than the real data they stand in for. The cause is the conditioning interface, which exposes diagnostic labels (or free text) but not the patient's numeric demographics, so any downstream use inherits the distortion. We introduce **DCDM-ECG**, a 10.1M-parameter conditional latent diffusion model whose 76-dimensional condition concatenates 71 multi-hot SCP codes with five z-normalised numeric demographic axes (age, sex, height, weight and heart rate); on PTB-XL it tracks the real age distribution to within one standard deviation, follows specified heart rate to within ~ 3 bpm per sample and reaches TSTR macro-AUROC 0.885 ± 0.012, 4.5 points above the strongest reported label-only baseline. Holding the architecture and training recipe fixed but removing the demographic axes drops macro TSTR to 0.561 ± 0.021, a like-for-like gap of 0.324 that isolates the contribution of the demographic conditioning.

1. Introduction

Synthetic 12-lead ECGs are increasingly used as stand-ins for real patient data: to augment classifier training sets, to stress-test demographic subgroups and to share data without releasing real records (Thambawita et al., 2021; Alcaraz & Strodthoff, 2023; Lai et al., 2025; Chung et al., 2023). For any of these uses the synthetic cohort must resemble a real one not only in its diagnoses but in who the patients are. Current conditional generators capture the first but not the second. The official SSSD-ECG checkpoint (Alcaraz & Strodthoff, 2023), the strongest open label-conditional model, produces ECGs whose Ribeiro-predicted age (Lima et al., 2021) is roughly eight years younger than real PTB-XL (Wagner et al., 2020), with the height and weight standard deviations compressed by 30 to 40 per cent; the text-conditioned DiffuSETS (Lai et al., 2025) reports a heart-rate MAE roughly 5× the Pan–Tompkins (Pan & Tompkins, 1985) floor on the same dataset.

The common cause is the conditioning interface. A downstream practitioner can only steer a generator through the channel it exposes, and current channels carry diagnosis but not demographics: SSSD-ECG accepts only the 71-code SCP label vector, while text-conditioned models accept a free-text prompt that may mention age or weight but, in practice, loses those numbers before they reach the denoiser. The main idea of this paper is that demographics should enter the generator through a structured numeric channel, conditioned alongside the diagnostic labels, so a user can request a specific age, sex, height, weight and heart rate, not whatever the model infers.

Contributions.

- We introduce **DCDM-ECG**, a 10.1M-parameter conditional latent diffusion model whose condition concatenates the 71 multi-hot SCP codes with five z-normalised numeric demographic axes.
- On PTB-XL fold 10, DCDM-ECG reaches TSTR macro-AUROC 0.885, 4.5 points above SSSD-ECG and within 0.022 of the same-size real-data ceiling (Section 3.1).
- It matches the real marginal age distribution to within one standard deviation and recovers specified heart rate to within ~ 3 bpm per sample (Sections 3.2 and 3.3).
- A same-recipe ablation that removes the five demographic axes drops macro TSTR from 0.885 ± 0.012 to 0.561 ± 0.021, isolating the contribution of the demographic channel (Section 4).
- The four numeric axes split into three fidelity tiers (HR followed, age and sex directional, height and weight ignored), tracking their PTB-XL coverage (Section 3.3).

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Table 1. Cross-method comparison on PTB-XL fold 10. TSTR is the macro AUROC of an XResNet1d-50 trained for 30 epochs on each method's synthetic samples and tested on real fold 10; demographics are reported by external predictors trained on real PTB-XL; HR MAE is Pan–Tompkins on lead II against the intended HR (real-data floor 1.45 bpm). Cells with "—" are inapplicable: Pulse2Pulse is unconditional (no labels for TSTR; no specified HR), SSSD-ECG does not accept an HR input, and DiffuSETS does not report TSTR or marginal demographics (FID and P/R are taken from the original publication). **Bold** marks the best comparable cell per column.

Model	Params	TSTR (real 0.907)	Age σ (real 16.5)	Sex %F (real 46%)	FID	P / R	HR MAE (bpm)
Pulse2Pulse	10.5M	—	10.7	57%	**11.1**	0.98 / 0.84	—
SSSD-ECG	59.8M	0.840	12.2	74%	35.4	**0.99** / 0.81	—
DiffuSETS	51.3M	—	—	—	27.6	0.82 / 0.86	6.7
DCDM-ECG (ours)	**10.1M**	$\mathbf{0.885 \pm 0.012}$	**17.2**	34%	14.8	0.96 / **0.90**	**2.4**

Related work. Conditional 12-lead ECG generation has moved from GANs (Thambawita et al., 2021) to label-only diffusion models conditioned on the 71 SCP codes (Alcaraz & Strodthoff, 2023; Skorik & Avetisyan, 2024) and to text-conditioned diffusion that accepts a clinical-report prompt (Chung et al., 2023; Lai et al., 2025). DiffuSETS (Lai et al., 2025) routes age, sex and heart rate through a structured side-channel alongside the clinical text prompt, but none of the published baselines exposes *all five* demographic axes (age, sex, height, weight, heart rate) through such a channel. The closest evaluation work is Ibrahim et al. (2025), which probes synthetic medical time series at the subgroup level. We compare directly against the two baselines whose checkpoints are publicly redistributable (SSSD-ECG, DiffuSETS) and against the unconditional Pulse2Pulse (Thambawita et al., 2021).

2. Method

Dataset. PTB-XL (Wagner et al., 2020) is the largest freely-redistributable clinical 12-lead ECG dataset, with 21,799 ten-second recordings from 18,885 patients sampled at 100 and 500 Hz. Each record carries a multi-label annotation in the 71 SCP-ECG codes and the patient's demographic metadata: age, sex, height, weight and resting heart rate. We adopt the official 10-fold patient-disjoint split that has become the standard benchmark protocol for ECG classification (Strodthoff et al., 2021): folds 1 to 8 train the generator and all downstream classifiers, fold 9 is used for early stopping, and fold 10 is held out for every result reported here. Training-set coverage of the demographic fields is essentially 100% on age, sex and resting heart rate, but only 31.9% on height and 41.5% on weight; this asymmetry will surface in the per-sample fidelity result of Section 3.3.

Backbone. A frozen 4.71M-parameter 1-D VAE encodes a 12×1000 ECG into a 4×128 latent. A 5.36M-parameter U-Net denoiser operates in latent space, conditioned on a 76-dimensional vector that concatenates the 71-hot SCP label vector with five z-normalised numeric demographic axes (age, sex, height, weight, heart rate). Where a demographic field is missing in the source record, we substitute the per-axis training-set mean. Classifier-free guidance (Ho & Salimans, 2022) is applied at training (condition dropout 0.1) and inference (CFG scale 3.0); the diffusion process uses 1000 training steps and 100 DDIM (Song et al., 2021) sampling steps.

Training. 20,000 iterations on PTB-XL fold 1 to 8 ($n = 17{,}418$), batch 128, AdamW, learning rate 1×10^{-4}, weight decay 10^{-5}, on a single Apple Silicon GPU.

Evaluation. For train-on-synthetic, test-on-real (TSTR) evaluation we draw $n = 8000$ conditioning tuples (label vector plus demographics) with replacement from the empirical training distribution, generate the corresponding 12-lead ECGs and train an XResNet1d-50 (Strodthoff et al., 2021) classifier for 30 epochs on the synthetic corpus. Macro AUROC over the 51 evaluable SCP codes is reported on real PTB-XL fold 10, averaged over 5 independent sampling and downstream-classifier seeds; we also report macro AUROC inside three prevalence buckets indexed off real training prevalence (common $\geq 5\%$, moderate 2 to 5%, rare $< 2\%$). The $n = 8000$ budget is the saturation point of our generator: a sanity run at $n = 17{,}418$ moves macro TSTR by 0.003. Demographic fidelity is measured by external predictors trained on real PTB-XL: a Ribeiro age regressor (Lima et al., 2021) and our own XResNet1d-50 sex, height and weight predictors. Heart rate is estimated by Pan–Tompkins on lead II. On real fold-10 ECGs these predictors carry error floors of 8.7 yr (age), 7.2 cm (height), 9.8 kg (weight) and AUROC 0.878 (sex); fidelity below should be read against these floors, not against zero error. FID and the precision / recall metrics of Kynkäänniemi et al. (2019) are reported in our shared sex-predictor penultimate feature space.

3. Results

3.1. Cross-method TSTR

Table 1 reports the cross-method comparison. Pulse2Pulse is unconditional and DiffuSETS does not report TSTR, so SSSD-ECG is the only label-conditional baseline with a

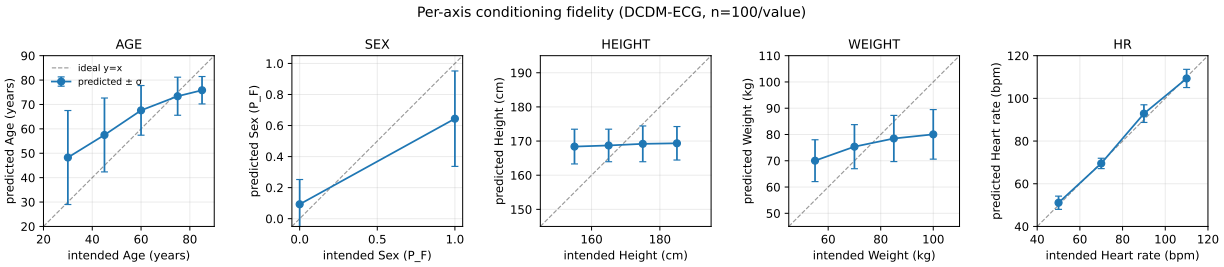

Figure 1. Per-axis conditioning fidelity. For each demographic axis we hold the label vector and the other demographics fixed, sweep that axis across controlled values and generate $n = 100$ samples per value. Each panel plots the intended value (x) against the corresponding external predictor's mean output ($y \pm \sigma$). The dashed line is $y = x$ (perfect recovery). Heart rate is recovered to within a few bpm of intended; age is monotonic but range-compressed; sex is directional with high residual variance; height and weight are essentially fixed at the real-cohort mean regardless of input. Per-axis training-set coverage (left to right) is 100%, 100%, 32%, 42%, 100%.

comparable number. DCDM-ECG reaches macro TSTR 0.885, 4.5 percentage points above SSSD-ECG's reported 0.840 (Alcaraz & Strodthoff, 2023); since that figure uses SSSD-ECG's own evaluation protocol, the gap is indicative rather than head-to-head, and the same-recipe ablation of Section 4 is the controlled comparison. Relative to our same-size real-data ceiling, the rare-class bucket is the tightest, at 0.887 against a 0.898 ceiling (a 0.011 gap; Section 3.4).

3.2. Marginal demographic fidelity

Figure 2 overlays each method's predicted age, sex, height and weight distributions against real PTB-XL. DCDM-ECG tracks the real distribution to within one standard deviation on age, height and weight, including the continuous axes where the label-only baselines collapse. Sex is the one exception: DCDM-ECG is biased male, though still markedly closer to real than SSSD-ECG. Pulse2Pulse is flatter still on age and sex, and its diagnostic distribution is dominated by NORM, an artefact of training on a normal-rhythm cohort. The reported DiffuSETS HR MAE (Lai et al., 2025) is computed on their own evaluator under their own conditioning protocol, so the HR comparison is indicative rather than head-to-head.

3.3. Per-sample conditioning fidelity

Marginal-distribution match does not by itself imply that the model *uses* demographic conditioning at the per-sample level: a generator that ignored the demographic input and just memorised the empirical PTB-XL marginal would also pass the marginal test. To probe per-sample fidelity we hold the label vector fixed at representative real PTB-XL training records and sweep one demographic axis at a time across controlled values; the sweep grid spans roughly the empirical PTB-XL inter-quartile range for each axis. We then run the corresponding external predictor on the generated cohorts and measure the recovery of the controlled value. The result, plotted in Figure 1, splits the four numeric axes into three tiers.

The three-tier split is consistent with the morphological content a 10-second 12-lead waveform actually carries. Age and sex have well-studied waveform correlates but are noisier per sample; the residual variance on sex matches the irreducible noise of the binary predictor itself (test AUROC 0.878 on real PTB-XL), so a fraction of intended-male samples are read female regardless of the generator. Height and weight have weak waveform correlates and large training-set imputation: our pipeline replaces missing values with cohort-mean defaults without exposing the presence mask to the model, so for any training batch the height channel sits at the default (165 cm) in roughly two out of three samples regardless of the actual patient, and the optimisation has little incentive to learn a height-dependent policy. The pattern is therefore most directly read as a coverage limitation of the source dataset rather than a hard limit of the modality. A natural follow-up is to restrict training to records with all five axes present, or to concatenate the per-axis presence mask to the conditioning vector so the model can distinguish real values from imputation defaults.

3.4. Per-class TSTR by prevalence bucket

Figure 3 reports macro AUROC within prevalence buckets, with the synthetic-real gap differing sharply by bucket. The rare-class bucket (0.887) sits within 0.011 of its same-size real-data ceiling (0.898); the moderate (0.875) and common (0.882) buckets sit roughly 0.05 below their ceilings (0.923 and 0.935). The pattern is the opposite of what a label-conditional generator typically produces: rare classes are the bright spot, not the failure mode. The mechanism is partly a ceiling effect (rare classes have fewer positive examples and thus a lower achievable AUROC), but the 0.011-from-ceiling rare-bucket result indicates the generator is not the bottleneck on the long tail. PTB-XL v1.0.3 has 71 SCP codes; 19 are zero-prevalence in the v2 stratified split and one (STACH) has no test positives, leaving 51 evaluable.

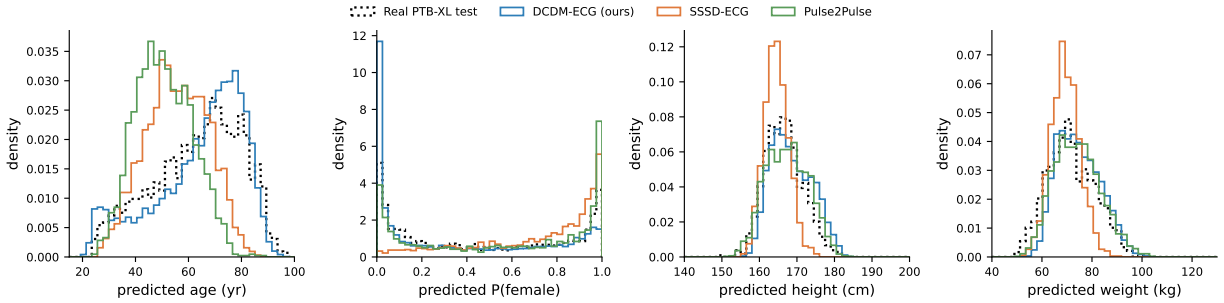

Figure 2. Marginal distributions of predicted age, sex (% female), height and weight on synthetic samples from each method, against real PTB-XL test. DCDM-ECG (blue) tracks real (black) to within one standard deviation on every axis. SSSD-ECG (orange) and Pulse2Pulse (green) show systematic shifts: younger and narrower age, biased sex, and collapsed continuous-axis standard deviation.

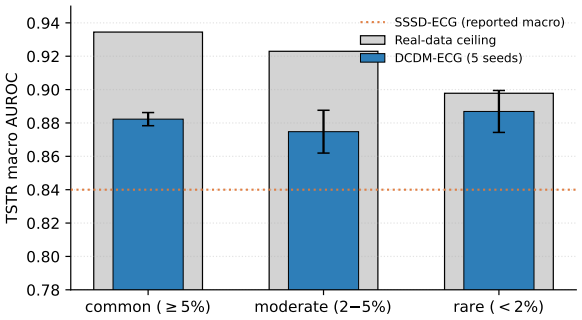

Figure 3. TSTR macro AUROC by prevalence bucket (common $\geq 5\%$, moderate $2-5\%$, rare $< 2\%$). Real-data ceiling is an XResNet1d-50 on a same-size real subset (dashed). SSSD-ECG (paper-reported macro 0.840) shown as a horizontal reference (dotted). DCDM-ECG sits within 0.011 of the rare-class ceiling.

4. Discussion

What the per-axis sweep tells us. Each axis enters through a learned column of the first projection matrix and the shared FiLM stack, so the model is free to use or ignore an axis depending on whether it can be *recovered* from the generated waveform during training. The per-axis pattern in Figure 1 is consistent with this: heart rate, age, and sex have well-studied morphological signatures in 10-second 12-lead ECG (Lima et al., 2021; Attia et al., 2019) and are followed per sample; height and weight, largely missing or imputed on PTB-XL (Section 2, 32% and 42% coverage), are ignored despite being part of the input. The channel thus surfaces an axis at sample time only when the training loss can ground it in the waveform.

Same-recipe ablation. To isolate the contribution of the demographic channel we retrain the same backbone with the demographic axes removed from the conditioning vector (cond_dim $= 71$, SCP labels only) under the same training recipe and evaluate both arms under the headline TSTR protocol ($n = 8000$, training-distribution conditioning, 5 seeds; Section 2). The labels-only model reaches macro TSTR 0.561 ± 0.021 against the full-cond model's 0.885 ± 0.012, a like-for-like gap of 0.324. Validation loss converges to a similar level in both settings, so the gap is on the class-conditional generation that TSTR measures, not on the denoising objective. The full-cond model sampled without CFG still reaches macro TSTR 0.878, ruling out CFG-amplification as the explanation. Which of the five input axes drives the gap we leave to future work; the per-sample fidelity in Figure 1 suggests three of the five carry most of the contribution.

Other limitations. We evaluate on a single dataset (PTB-XL); MIMIC-IV-ECG is a natural next step. The one diagnostic class on which DCDM-ECG underperforms SSSD-ECG is IVCD, consistent with our smaller backbone losing low-frequency QRS detail that the S4 stack preserves. We assess realism only distributionally, via FID and the precision / recall metrics in the predictor feature space; expert review of generated morphology and clinical plausibility is left to future work. The TSTR protocol bootstraps training-record conditioning tuples with replacement, which would advantage a model that memorised records; the demographic standard deviation matching the real distribution (Figure 2) and the rare-class TSTR at the real-data ceiling are inconsistent with record-level memorisation.

5. Conclusion

The take-away is that a small architectural change, routing demographics through a structured numeric channel rather than only through diagnostic labels, recovers the demographic information that label-only and text-conditioned ECG generators lose, while lifting TSTR 4.5 points over the strongest label-only baseline at $\sim 6\times$ fewer parameters; a same-recipe ablation removing the demographic vector drops macro TSTR by 0.33. Natural next steps are a per-axis presence mask to address the height/weight coverage failure and extension to MIMIC-IV-ECG.

Acknowledgements

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