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FetMRQC: A robust quality control system for multi-centric fetal brain MRI

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ABSTRACT

Fetal brain MRI is becoming an increasingly relevant complement to neurosonography for perinatal diagnosis, allowing fundamental insights into fetal brain development throughout gestation. However, uncontrolled fetal motion and heterogeneity in acquisition protocols lead to data of variable quality, potentially biasing the outcome of subsequent studies. We present FetMRQC, an open-source machine-learning framework for automated image quality assessment and quality control that is robust to domain shifts induced by the heterogeneity of clinical data. FetMRQC extracts an ensemble of quality metrics from unprocessed anatomical MRI and combines them to predict experts' ratings using random forests. We validate our framework on a pioneeringly large and diverse dataset of more than 1600 manually rated fetal brain T2-weighted images from four clinical centers and 13 different scanners. Our study shows that FetMRQC's predictions generalize well to unseen data while being interpretable. FetMRQC is a step towards more robust fetal brain neuroimaging, which has the potential to shed new insights on the developing human brain.

1. Introduction

Establishing a protocol for objective image quality assessment and control for neuroimaging studies is critical to enforce reliability, generalization and replicability (Mortamet et al., 2009; Niso et al., 2022; Rosen et al., 2018). Quality assessment (QA) focuses on assessing and eventually improving the quality of a process to prevent issues from propagating, while quality control (QC) looks to find and discard problematic outputs of that process (Alfaro-Almagro et al., 2018). Both steps are fundamental in magnetic resonance imaging (MRI) studies, as insufficient MRI data quality has been shown to bias statistical analyses and neuroradiological interpretation (Power et al., 2012; Reuter et al., 2015; Alexander-Bloch et al., 2016).

Automated QA/QC tools designed to assist data exclusion decisions for adult brain neuroimaging studies (Esteban et al., 2017; Klapwijk et al., 2019; Vogelbacher et al., 2019; Ravi et al., 2023) are becoming increasingly available. However, these techniques are inapplicable to fetal MRI, as they rely on priors that are not valid *in utero*, such as e.g., assuming that the head is surrounded by air or the relative orientation of the brain with respect to the stereotaxic frame defined by the scanner. In addition, fetal brain MRI typically displays larger and uncontrolled motion of the head as fixation techniques (e.g., padding) and real-time feedback countermeasures are only available after birth (Fig. 1A). Moreover, fetal brain imaging greatly lacks standardization in acquisition protocols (Fig. 1B). While consensus has settled on 2-dimensional (2D) fast-spin echo interleaved T_2-weighted (T2w) MR schemes showcasing thick slices (Tortori-Donati et al., 2005; Gholipour et al., 2014), specific imaging parameters such as in-plane resolution, slice thickness, field of view, or vendor implementation of the imaging sequence greatly vary. As a result, the appearance and quality of

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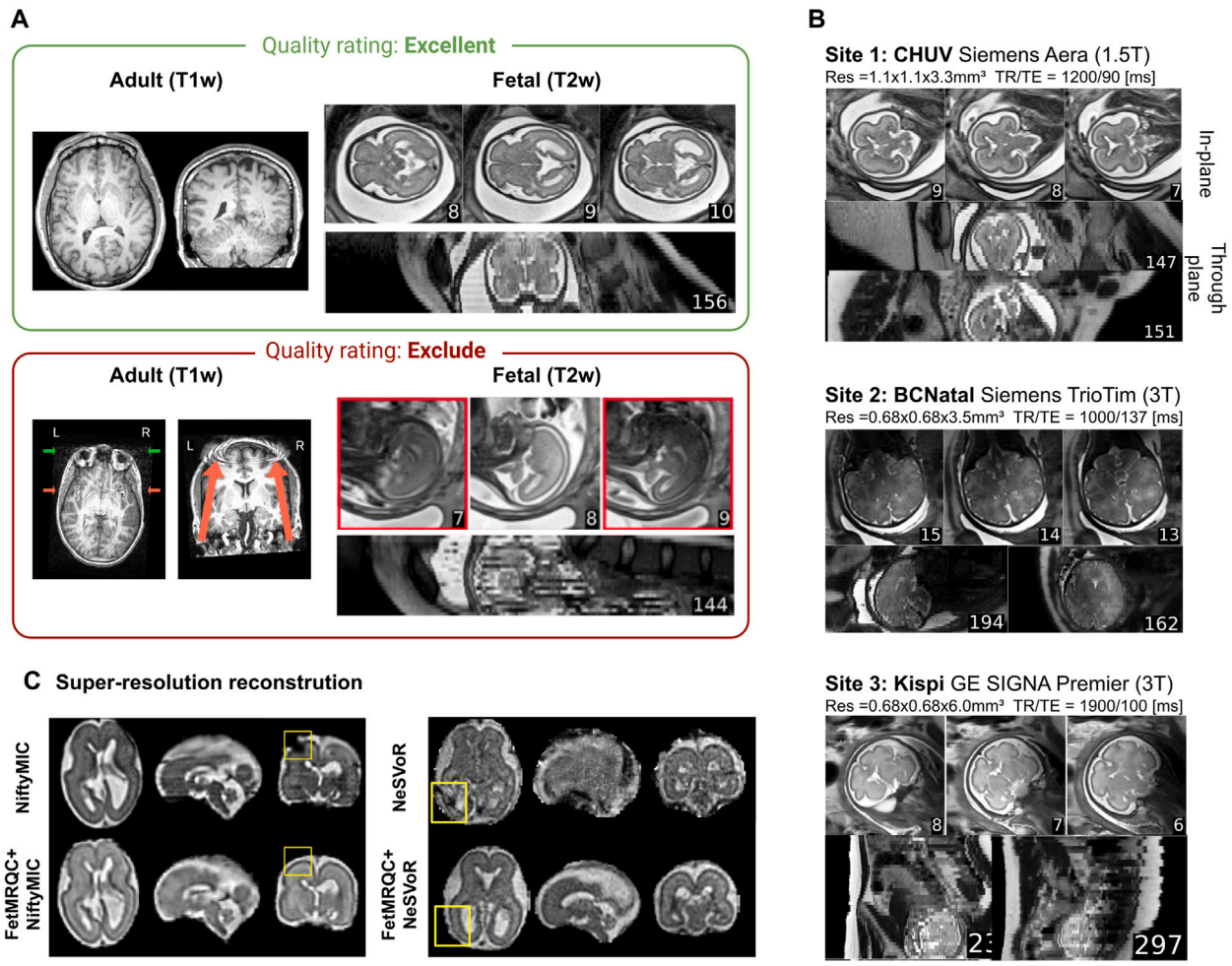

Fig. 1. Variations in data quality illustrated. A – Comparison of data across adult (T1w), from the ABIDE dataset (Di Martino et al., 2014) and from fetal acquisitions. In the excluded scans, the adult image on the left suffers from severe motion artifacts, while large coil artifacts corrupt the image on the right. The fetal data suffer from strong intensity changes between multiple slices and signal drop; in the through-plane view, strong inter-slice motion makes it difficult to discern the brain structures. B – Examples of data acquired on different scanners, with very different appearance. The in-plane and through-plane resolution, the field of view, the repetition time (TR), and the echo time (TE) can all substantially change between acquisition protocols. C – Importance of quality control for super-resolution reconstruction (SRR), illustrated using NiftyMIC (Ebner et al., 2020), and NeSVoR (Xu et al., 2023), two SRR methods with built-in outlier rejection. On the top row a subject is reconstructed using all stacks available (13 for NiftyMIC, 5 for NeSVoR), and each reconstruction shows large artifacts. On the bottom row, FetMRQC is plugged in and by removing low quality series (6 out of 13 for NiftyMIC, 2 out of 5 for NeSVoR), the reconstruction quality is improved.

fetal MR images in this wild-type data vary markedly across centers (Fig. 1B).

Although fetal brain MRI can be severely affected by artifacts like inter-slice motion, signal drops or bias field (Gholipour et al., 2014), only few methods dedicated to QA/QC have been proposed. Initially, automated QA/QC has been integrated within the super-resolution reconstruction (SRR) process (Uus et al., 2022b; Kuklisova-Murgasova et al., 2012; Ebner et al., 2020; Tourbier et al., 2015; Xu et al., 2023). SRR is a ubiquitous early step of the fetal MRI processing workflow that builds a high-resolution, isotropic, 3D volume from several differently-oriented stacks of 2D slices with low-resolution (LR) along the through-plane axis (i.e., anisotropic resolution) (Uus et al., 2022a). Some of the proposed approaches incorporate an automated QC stage for outlier rejection that excludes sub-standard slices or pixels from the input low-resolution stacks, and measure the similarity between a reconstructed slice and an input slice using information-theoretic metrics (Ebner et al., 2020; Kuklisova-Murgasova et al., 2012; Kainz et al., 2015; Xu et al., 2023). However, as illustrated on Fig. 1c, sub-optimal quality stacks can remain detrimental to the final quality of the reconstruction, even when SRR pipelines include outlier rejection schemes. Additional QA/QC checkpoints are thus needed to filter out low-quality raw T2 stacks before using SRR, and several deep learning-based methods were recently proposed for this task (Lala et al., 2019; Xu et al., 2020; Liao et al., 2020). These solutions aim to automatically identify problematic slices for exclusion (QC), and, if streamlined with the acquisition, enable re-acquiring corrupted slices on the fly (Gagoski et al., 2022) (QA). However, these methods operate at the slice level, and not all artifacts can be seen by analyzing slices independently. For instance, inter-slice motion (visible on the right of Figure 1a), a strong bias field in the through-plane direction, or an incomplete field of view can be spotted only when considering the entire stack of slices. Stack-wise QA/QC methods are thus still needed.

Importantly, these methods face the challenge of deployment to unseen scanners or acquisition settings: how will they generalize to unseen domains? Due to the private and sensitive nature of medical data (Willemink et al., 2020), building large and diverse medical imaging datasets is difficult endeavor. As a consequence, proposed methods are often only evaluated on locally available data, and can fail to deal with the heterogeneity found across different centers (Sambasivan et al., 2021; Varoquaux and Cheplygina, 2022). In addition, while openly shared MRI databases have been released for adults (Mueller et al., 2005; Di Martino et al., 2014; Markiewicz et al., 2021; Van Essen et al., 2013), children and adolescents (Makropoulos et al., 2018; Casey et al., 2018), privacy protection regulations and ethical limitations to data-sharing are much stronger regarding fetuses, making it even more

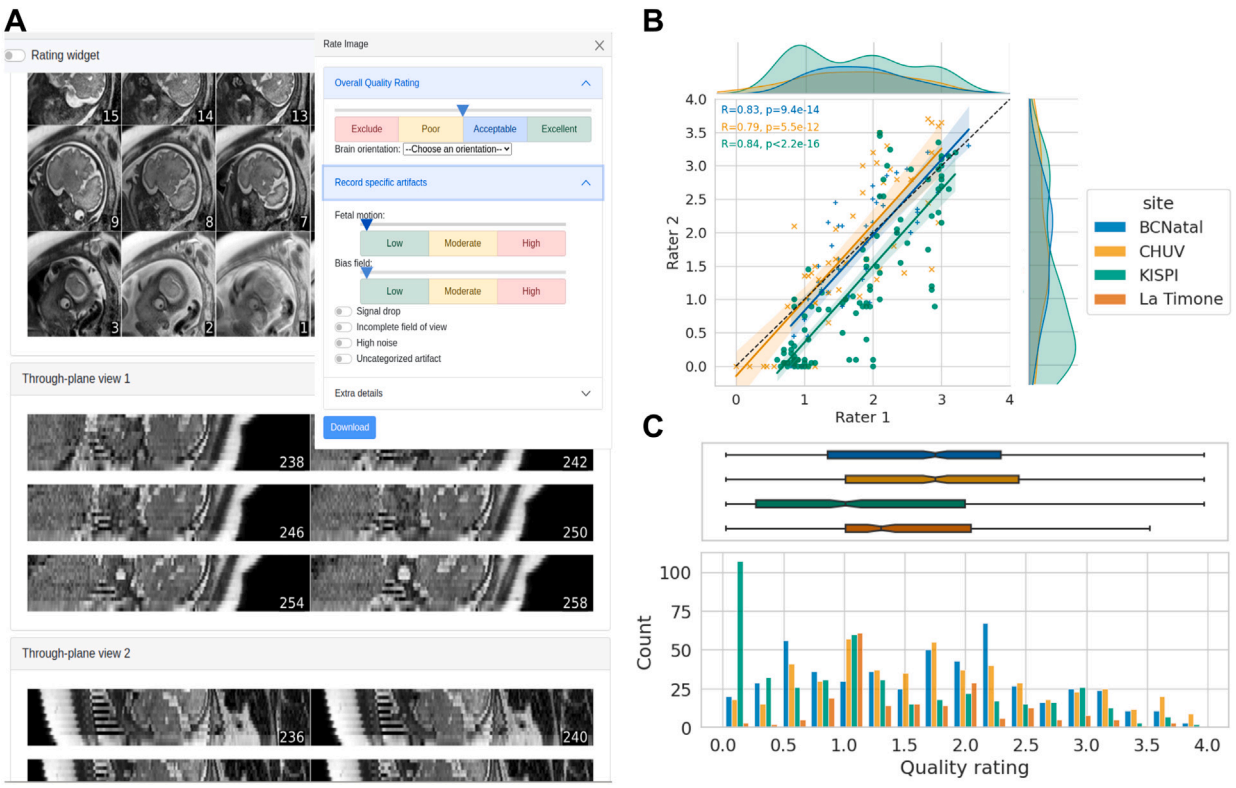

Fig. 2. A look into the dataset. A — Illustration of the quality rating interface developed in this work. B — Inter-rater agreement on the 211 stacks annotated by both raters. The global R value is 0.75. Note that stacks from La Timone were only annotated by Rater 2. C — Distribution of the quality ratings across the different sites considered, on all data. The median values are respectively 1.75 [0.84, 2.4] for BCN, 1.75 [1, 2.45] for CHUV and 1 [0.1, 2.05] for KISPI.

difficult to construct robust ML models trained on multicentric data. As today, the question of the robustness of state-of-the-art approaches to fetal brain quality control (Xu et al., 2020; Ebner et al., 2020; Uus et al., 2022b) to unseen domains remains open.

Beyond the need of supporting SRR, quality assessment also builds towards reproducible neuroimaging pipelines, allowing to fairly compare different processing steps (Payette et al., 2021). For instance, initiative of fetal brain tissue segmentation but lack of systematic/standardized objective evaluation of quality input data that would support the analysis of the comparison results (Payette et al., 2023).

The contribution of our paper is threefold. First, we introduce a framework specifically designed for QA/QC manual annotations of T2w fetal brain MRI. It generates a visual report for efficient stack screening and manual QA, facilitating the work of raters. Second, we present FetMRQC, a machine learning model based on manual ratings to automatically perform two tasks: (1) quality assessment, where a discrete score between 0 (bad quality) to 4 (excellent) quality is predicted, and (2) quality control, where the model predicts whether an image reaches a predefined quality threshold. QA — a regression task in our case – and QC — a binary classification problem — are performed automatically by a random forest that uses an ensemble of 332 image quality metrics (IQMs), extracted from raw T2w stacks, that reflect complementary quality features based on various statistics computed from image intensity, brain mask and segmentation (details on IQMs extraction is available in the Materials and Methods section). Third, by collecting and manually annotating a very large collection of 1649 low-resolution T2w images from 233 subjects, acquired in 13 different scanners in four different institutions across Europe, we can benchmark the generalization of automated QA/QC models to unseen domains, including existing baselines (Ebner et al., 2020) and pre-trained deep learning models (Legorreta et al., 2020; Xu et al., 2020). A pilot study of this work, including fewer IQMs and only two centers, was previously presented (Sanchez et al., 2023). The code and image quality metrics are available at https://github.com/Medical-Image-Analysis-Laboratory/fetmrqc.

2. Methods

2.1. Data

For this study, we retrieved 1649 T2-weighted 2D stacks of slices from 233 subjects from existing databases at four different institutions, including both neurotypical and pathological cases. The corresponding local ethics committees independently approved the studies under which data were collected, and all participants gave written informed consent.

Lausanne University Hospital (CHUV), Switzerland, provided 61 subjects (498 scans), with an average of 7.9 ± 3.0 stacks per subject. BCNatal (Hospital Sant Joan de Déu, Barcelona, Spain) provided 85 subjects (508 scans), 5.8 ± 3.4 stacks per subject. University Children's Hospital Zürich (KISPI), Switzerland, provided 19 subjects (441 scans) with 23.2 ± 5.36 stacks per subject. La Timone University Hospital, Marseille, France, provided 68 subjects (203 scans) with 3 stacks per subject. The reason for having few scans per subject at La Timone is due to the acquisition duration being limited in clinical routine, while other centers have a more research-oriented acquisition. After the exclusion of scanners with insufficient data (CHUV - Siemens Avanto with 5 stacks), the aggregate sample size is N = 1644 stacks. The imaging parameters, magnetic field strength, repetition time (TR), echo time (TE), field of view (FoV), etc. greatly varied across centers and scanners, reflecting the heterogeneity found in clinical practice. The details are provided in Table 1.

The acquisition parameters show a very large variability across scanners and sites. For instance, the resolution of 1.5 T scanners changes from 1.1 × 1.1 mm^2 (e.g. CHUV - Aera) in-plane to 0.5 × 0.5 mm^2 (e.g. KISPI - Signa Artist), which leads to large differences

Table 1
Detailed description of the data used in the study. Field refers to the magnetic field of the scanner, TR is the repetition time and TE is the echo time, FoV is the field of view.

CHUV						
Model (Siemens)	Field [T]	$(n_{\text{subjects}}, n_{\text{LR}})$	TR [ms]	TE [ms]	Resolution [mm^3]	FoV [cm]
Aera	1.5	(34, 281)	1200	90	1.12 × 1.12 × 3.3	36
MAGNETOM sola	1.5	(17, 138)	1200	90	1.1 × 1.1 × 3.3	36
MAGNETOM vida	3	(2, 14)	1100	101	0.55 × 0.55 × 3	35
Skyra	3	(8, 77)	1100	90	0.55 × 0.55 × 3	35
BCNatal						
Model (Siemens)	Field [T]	$(n_{\text{subjects}}, n_{\text{LR}})$	TR [ms]	TE [ms]	Resolution [mm^3]	FoV [cm]
Aera	1.5	(**16**, **158**)				
		−(6, 80)	1500	82	0.55 × 0.55 × 2.5	28
		−(4, 34)	1000	137	0.59 × 0.59 × 3.5	23/30
		−(4, 33)	1000	81	0.55 × 0.55 × 3.15	28
		−(2, 11)	1200	94	1.72 × 1.72 × 4.2	36/44
MAGNETOM Vida	3	(11, 56)	1540	77	1.04 × 1.04 × 3	20
TrioTim	3	(**59**, **322**)				*// 4 outliers*
		−(24, 97)	1100	127	0.51 × 0.51 × 3.5	26
		−(15, 108)	990	137	0.68 × 0.68 × 3.5−6.0	26
		−(14, 71)	2009	137	0.51 × 0.51 × 3.5	26
		−(1, 14)	3640	137	0.51 × 0.51 × 3.5	26
Kispi						
Model (General Electric)	Field [T]	$(n_{\text{subjects}}, n_{\text{LR}})$	TR [ms]	TE [ms]	Resolution [mm^3]	FoV [cm]
SIGNA premier	3	(**3**, **58**)				*// 8 outliers*
		−(3, 24)	<2500	100/120	0.65 × 0.65 × 3/5	33
		−(3, 26)	3000	120	0.47/0.57 × 0.47/0.57 × 3	29/24
Discovery MR750	3	(**5**, **125**)				*// 5 outliers*
		−(5, 29)	<2500	120	0.65 × 0.65 × 3/5	33
		−(5, 81)	3000	120	0.55 × 0.55 × 3	28
		−(5, 10)	5000	120/500	0.53 × 0.53 × 3/5	28
SIGNA artist	1.5	(**11**, **258**)				*// 22 outliers*
		−(11, 108)	<2500	100/120	0.47/0.64 × 0.47/0.64 × 3/5	24−35
		−(11, 128)	3000	120	0.47/0.55 × 0.47/0.55 × 3	26
La Timone						
Model (Siemens)	Field [T]	$(n_{\text{subjects}}, n_{\text{LR}})$	TR [ms]	TE [ms]	Resolution [mm^3]	FoV [cm]
Skyra	3	(**34**, **101**)				
		−(31, 93)	3200	177	0.68 × 0.68 × 3	26
		−(3, 8)	3750	183	0.59 × 0.59 × 3	30
SymphonyTim	1.5	(34,102)	1680	137	0.74 × 0.74 × 3.5	38

in signal-to-noise ratio. In addition, different models using the similar parameters can also yield largely different images. Examples are shown on Fig. 1B. Such variable parameters are strong indicators of domain shifts that might challenge the generalization of machine learning models.

2.2. Manual QA of fetal MRI stacks

FetMRQC comprehends two major elements to implement QA/QC protocols of unprocessed (stacks of 2D slices) fetal brain MRI data. First, the tool builds upon MRIQC's framework and generates an individual QA report for each stack to assist and optimize screening and annotation by experts. Second, FetMRQC proposes to train machine learning models based on image quality metrics (IQMs).

Akin to MRIQC (Esteban et al., 2017), FetMRQC generates an HTML-based report adapted to the QA of fetal brains for each input stack of 2D slices (Fig. 2A) to help make the process of manual rating of quality standardized and efficient. The input dataset is required to comply with the Brain Imaging Data Structure (BIDS Gorgolewski et al., 2016), a format widely adopted in the neuroimaging community. The reports are generated using an image with a corresponding brain mask. This mask can be extracted automatically, and in this work, we used MONAIfbs (Ranzini et al., 2021). Each individual-stack report has a QA utility (the so-called rating widget), with which raters can fill in an overall quality score, the in-plane orientation, and the presence and grading of artifacts visible in the stack. We use an interval (as opposed to categorical) rating scale with four main quality ranges: [0,1): exclude – [1,2): poor – [2,3): acceptable – [3,4): excellent. Interval ratings simplify statistical modeling, set lower bounds to annotation noise, and enable the inference task where a continuous quality score is assigned to input images rather than broad categories. In addition, a navigation menu allows the rater to access all reports in a centralized location, and by being able to access the next image to be rated in a single click. Being HTML-based, the reports can be visualized on any web browser, and effectively remove any bias due to using different image visualization software.

2.3. IQMs extraction and prediction models

FetMRQC's QA/Qc prediction models work in two steps. An ensemble of image quality metrics are first extracted from the raw T2-weighted images and then are used as input to a classification or regression model that learns to predict the quality ratings from the IQMs.

2.3.1. IQMs tailored to fetal brain MRI

While tools designed for QA/QC for adult brain neuroimaging studies (Esteban et al., 2017; Klapwijk et al., 2019) are available, they are not readily applicable to fetal brain MRI, due to priors invalid in this context. However, some IQMs can be translated to fetal brain MRI and several works have proposed developed quantities that can be used as IQMs, and we include them as features in FetMRQC. The method of Kainz et al. (2015), `rank_error`, predicts the quality of a raw T2-weighted stack by estimating its compressibility using singular value decomposition. Ebner et al. (2020) used the volume of the brain mask, `mask_volume`, to exclude outlying stacks, and de Dumast et al. (2020) computed its centroid to estimate inter-slice motion. We also include recently proposed slice-wise and stack-wise deep learning-based IQMs,

`dl_slice` (Xu et al., 2020) and `dl_stack` (Legorreta et al., 2020). We use their pre-trained models, as we want to test the off-the-shelf value of these IQMs. Note that the method of Liao et al. Liao et al. (2020) was not included because their code is not publicly available and we could not get in contact with the authors. `dl_slice` (Xu et al., 2020) predicts simultaneously whether a slice contains some brain volume, and whether this slice is of good quality. We aggregate their slice-wise score into a global score by computing $\frac{1}{n_{\text{slices}}} \sum_{i=1}^{n_{\text{slices}}} p_{i,\text{pass}} - p_{i,\text{fail}}$, yielding a score between −1 and 1.

Along with these existing IQMs, we also propose additional IQMs for quality prediction that have not previously been used in the context of fetal brain MRI. They can be roughly categorized into three groups: intensity-based, mask-based, segmentation-based. In a nutshell, *intensity-based* IQMs directly rely on the voxel values of the image. These include summary statistics (Esteban et al., 2017) such as mean, median, and percentiles. We also repurpose metrics traditionally used for outlier rejection, such as PSNR or Normalized Cross Correlation (NCC) (Kuklisova-Murgasova et al., 2012; Kainz et al., 2015; Ebner et al., 2020) to quantify the intensity difference between slices in a volume. We compute entropy (Esteban et al., 2017), estimate the level of bias using N4 bias field correction (Tustison et al., 2010) and estimate the sharpness of the image with Laplace and Sobel filters. The second type of metrics are *mask-based* and operate directly on the automatically extracted brain mask. We propose to use a morphological closing in the through-plane direction to detect inter-slice motion, as well as edge detection, to estimate the variation at the surface of the brain mask, using Laplace and Sobel filters. The third type of IQMs is *segmentation-based*. While such metrics were originally proposed in the context of *MRIQC* (Esteban et al., 2017), they have never been adapted to fetal brain imaging. These are segmentation-based and include region-wise summary statistics, region-wise volume, region-wise signal-to-noise ratio (Dietrich et al., 2007), contrast-to-noise ratio between white matter (WM) and gray matter (GM) (Magnotta et al., 2006), coefficient of joint variation between gray matter and white matter (Ganzetti et al., 2016) and white matter to maximum intensity ratio (Esteban et al., 2017). In order to compute these segmentations from the raw T2-weighted stacks, we train a nnUNet-v2 (Isensee et al., 2021) 2D model on the FeTA dataset (Payette et al., 2021), a public dataset consisting of super-resolution (SR) reconstructed fetal brain images along with manual segmentations. The model is trained with the parameters automatically defined by nnUNet, which yield satisfactory results for SR volumes, and is then used to perform slice-wise inference on the low-resolution T2-weighted stacks. The segmentations are done over eight different classes, which we merge then into three groups: white matter (excluding corpus callosum), cerebrospinal fluid (CSF; intra-axial and extra-axial), and gray matter (cortical and deep). This is done to enable the use of the segmentation-based IQMs from MRIQC (Esteban et al., 2017), which rely on these three groups.

Variants of the metrics. All the IQMs operate by default on raw T2-weighted 2D images and/or masks, but they can be pre-processed in various manners. For example, Kainz et al. (2015) evaluated their metrics only on the third of the slices closest to the center of a given volume. We construct variants on our IQMs using various pre-processing methods. The variants include considering the third of the center-most slices instead of the whole ROI; masking the maternal tissue in the background; aggregating point estimates using mean, median, or other estimators; and computing information theoretic metrics on the union or intersection of masks. Finally, metrics used for outlier rejection can be either computed as a pairwise comparison between all slices (by default) or only on a window of neighboring slices. With all the different variations, we obtain a total of 166 different IQMs.

In addition to the previously described IQMs, we also include a Boolean variable that assesses whether a given IQM computation failed. If this occurs, the IQM will have a zero value and the corresponding Boolean variable will be set to true. This allows to keep all IQMs values to a real number. With the variants and the missing value flag, we reach a total of 332 IQMs. A more thorough description of each IQM used in FetMRQC is available in Table 4 in the supplementary material, along with a cross-correlation matrix on the entire training dataset of the 100 IQMs most frequently used.

2.3.2. QA/QC prediction

Given the extracted IQMs, a prediction model is then trained to predict the discrete ratings (QA; regression) or predict whether an image should be excluded (QC; classification), using various machine learning models from the Scikit Learn library (Pedregosa et al., 2011) and from the XGBoost python package. For the QA task, we consider linear regression, support vector machine (`SVR` class using an RBF kernel with a scaled kernel coefficient, regularization parameter C = 1.0), random forests (`RandomForestRegressor` class with 100 estimators, fitted using the Gini coefficient), and XGBoost's regression model (Chen and Guestrin, 2016) (`XGBRegressor` class using 100 estimators). For the QC task, we consider logistic regression, support vector classifier (`SVC` class using an RBF kernel with a scaled kernel coefficient, regularization parameter C = 1.0), random forest (`RandomForestClassifier` class with 100 estimators, fitted using the Gini coefficient), and XGBoost's classification model (Chen and Guestrin, 2016) (`XGBClassifier` function using 100 estimators).

Early experiments included also a multi-layer perceptron (`MLPRegressor` and `MLPClassifier` classes with multiple hidden layers with up to 1000 neurons per layer), but these models were not found to bring any added value compared to the non deep-learning based approaches, while very largely increasing the training time. They were not used in the following analyses. Note that this behavior is common in tabular data, where deep learning models are not necessarily performing best (Grinsztajn et al., 2022).

We performed model selection by ablating over the previously mentioned feature normalization and feature selection options, as well as various models.

Pre-processing. The QA/QC prediction started from the unprocessed clinical acquisitions, converted from the DICOM to the Nifti format. The same pre-processing steps were applied to the data from all the sites considered.

IQM normalization. Domain shifts, also known as batch effects (Leek et al., 2010; Esteban et al., 2017), can induce substantial biases in IQM computations. One approach to mitigate them is using group scaling (Esteban et al., 2017). This is why we experiment with various normalization techniques: standardization, robust (median-based) and quantile scaling, group-wise standardization, group-wise robust/quantile scaling (scaling by subject/scanner/site) and ComBat (Johnson et al., 2007). In addition to mitigating batch effects, feature standardization is important for models such as logistic or linear regression, but this is not the case for tree-based models.

Feature selection and dimensionality reduction. Correlated and irrelevant features can also be an obstacle for machine learning models. We experiment with dropping IQMs that are highly correlated with each other(with thresholds of 0.8 and 0.9), to remove constant features, and experiment with removing features that do not contribute more than noise using the Winnow algorithm (Littlestone, 1988) with extremely randomized trees (Esteban et al., 2017). Finally, we also explore using principal component analysis to construct orthogonal features.

Model selection. In our initial experiments, we used nested cross-validation to automatically perform model selection and evaluation without introducing optimistic biases (Varoquaux et al., 2017). We performed model selection by ablating over the previously mentioned feature normalization and feature selection options, as well as the different models. However, in the large majority of these experiments, the best-performing configuration used no standardization, no feature selection, and random forests for both classification and regression. Based on these ablations (available in the Supplementary Material 5.3),

Table 2
Summary of the methods compared in the paper.

`dl_slice`	Slice-wise deep learning (DL) quality control of Xu et al. (2020), aggregated into a single score. The decision threshold is learned by logistic regression.
`dl_stack`	Stack-wise DL QC of Legorreta et al. (2020). The decision threshold is learned by logistic regression.
`NiftyMIC-QC`	Subject-wise QC, excluding stacks with brain volume below 70% of the median brain volume calculated for the subject.
`Base`	Base version of FetMRQC using 6 IQMs: rank error (Kainz et al., 2015), mask centroid (de Dumast et al., 2020), mask volume (Ebner et al., 2020), normalized cross-correlation, mutual information (Kuklisova-Murgasova et al., 2012; Ebner et al., 2020), `dl_stack` (Legorreta et al., 2020), `dl_slice` (Xu et al., 2020)
`FetMRQC`	Full version of FetMRQC, using 332 IQMs
`FetMRQC-20`	Use the 20 best IQMs of FetMRQC – `rank_error`, `closing_mask_full`, `mask_volume`, `filter_mask_Laplace`, `filter_mask_sobel_full`, `nRMSE_window`, `filter_mask_Laplace_full`, `filter_mask_Laplace`, `closing_mask`, `rank_error_center`, `seg_sstats_BG_N`, `centroid`, `rank_error_center_relative`, `seg_sstats_CSF_N`, `seg_sstats_GM_N`, `im_size_z`, `NCC_intersection`, `NCC_window`, `PSNR_window`, `seg_SNR_WM`, `seg_volume_GM`.
`Subject-wise oracle`	For each subject, compute the average stack quality and return this value for all the stacks of the subject.

we decided to only use a random forest without standardization or feature selection. As no model selection needs to be carried out, nested cross-validation is not required and will not be used in the rest of the paper.

2.4. Experimental setting

We divide our dataset in two: 1246 stacks were used for training and validation of the models based on cross-validation experiments and 398 were used for assessing the generalization to unseen data, from La Timone and two randomly selected scanners. Data from La Timone were included in the study specifically to serve as external testing from an unseen site. Three increasingly challenging evaluation settings are considered: (i) Subject-wise 10-fold cross validation (CV) on the *training* stacks, which quantifies the expected performance of the method on new subjects acquired on already seen scanners; (ii) Leave-one-Scanner-out (LoSo) CV on the *training* stacks, where each fold leaves out all data from a single scanner for evaluation. This evaluates the expected performance of the method on different scanners; (iii) Pure testing on unseen scanners and an unseen site. This is the closest to a real-world deployment setting, as the pure testing data were not seen during the processes of design and training of the models.

Baselines. For classification, we consider the following baselines. We first include NiftyMIC-QC (Ebner et al., 2020), which computes the volume of the brain for each stack and, for each subject, excludes the stacks with a volume below 70% of the median volume. We also include the deep learning methods of Legorreta et al. (2020) (`dl_stack`) and Xu et al. (2020) (`dl_slice`). These IQMs are computed for each individual subject, we then standardize them and train a logistic regression model to adjust their prediction to the statistics of our dataset. This step adjusts the threshold for prediction and can only be beneficial to the prediction accuracy of these baselines.

For regression, as there is no baseline available to our knowledge, we consider a simple model predicting only subject-wise class statistics for regression, predicting the average rated quality of each subject as quality assessment (e.g. for a subject with three stacks rated as 3.5, 2, 3 respectively, the model assigns the value 2.83 to all stacks). This oracle is based on the assumption that the subject-wise averaged rating can be predictive of the quality rating, which is the case in our data, as the Pearson correlation of the two is $R = 0.59$. This method serves as a coarse point of comparison for the QA performance of FetMRQC.

In addition, for both QC and QA, we assessed the added value of our proposed IQMs as follows. First, we constructed a *Base* version of FetMRQC using the six state-of-the-art IQMs proposed in the context of fetal brain QA/QC. Then, we considered two variants of our model: FetMRQC used all estimated 332 IQMs and FetMRQC-20 used only 20 IQMs (selected based on their measured feature importance on the training data). Note that as this selection was based on the results in evaluation settings (i) and (ii), the performance of the model was likely be inflated due to double dipping (Kriegeskorte et al., 2009). It remains nonetheless informative on the expected performance of FetMRQC when only relying on a restricted set of IQMs. FetMRQC-20 is further discussed in our last experiment. All details regarding the baselines is provided in Table 2.

Evaluation metrics. Our classification results use a weighted F1-score, to handle imbalanced classes, and the area under the receiver operating characteristic curve (ROC AUC), as well as precision and recall. Our regression results are evaluated using Pearson's R^2 score, Spearman rank correlation, and mean absolute error (MAE).

Implementation. The experiments were implemented with Python 3.9.15 and Scikit-learn 1.1.3 (Pedregosa et al., 2011). All code is available on Github[1] and a Docker version[2] is also provided.

3. Results

3.1. Stack screening optimization with visual reports

Using FetMRQC's visual reports interface, Rater 1 annotated 657 stacks, and rater 2 annotated 1203 stacks. 211 of these stacks selected randomly across the training dataset were annotated by both raters to assess inter-rater reliability. Rater 1, YG, is a maternal-fetal physician with 5 years of experience, and Rater 2, MBC, is an engineer with 20 years of experience. The total rating time was 6 h 40 min for Rater 1 (median of 36 s per volume), and 14h20 for Rater 2 (median of 42 s per volume). A high inter-rater agreement was achieved in the manual quality annotations, with Pearson's correlation value of 0.75 overall ($R^2 = 0.56$; Fig. 2). The inter-rater agreement is consistently high within each site (2B). On CHUV data, 127 stacks were manually rated below the exclusion threshold (Quality < 1), and 371 were rated between poor and excellent. On BCNatal data, 155 stacks were excluded, and 353 rated above the threshold. On KISPI, 218 stacks were rated below 1, and 223 above. On La Timone, 42 stacks were rated below 1, and 161 stacks above. The average ratio of excluded stacks is 2.04. Regarding inclusion and exclusion of stacks (stacks with quality above 1 are included, other are rejected), the inter-rater agreement yielded a Cohen's coefficient of $\kappa = 0.58$ (moderate agreement according to the interpretation of Landis and Koch (1977)).

While the raters were trained to rate the overall quality of the images, they also were instructed, but not trained, to rate specific artifacts. They were asked to rate the degree of fetal motion (visible as discontinuities through-plane and signal drops in-plane) and bias field, visible as a low-frequency varying field. However, as their main goal was to give a global rating, the raters often skipped the assessment of the artifacts when the image was either clearly good or clearly bad, leading to inconsistent ratings for motion rating, their Pearson's correlation drops to $R^2 = 0.15$, and for bias rating, $R^2 = 0.02$. We believe that such a low reliability could be avoided by designing the rating differently, and asking the raters to assess artifacts before giving a global score. In the sequel, we will only use the overall quality rating of the images.

[1] https://github.com/medical-image-analysis-laboratory/fetmrqc.
[2] https://hub.docker.com/u/thsanchez.

Table 3
Quality control and assessment results. QC (classification, left) and QA (regression, right) results were averaged over five repetitions of the experiment. Results are the median cross-validation performance. The number in parentheses is the average worst-performing cross-validation fold. Three evaluation settings were considered: 10-fold subject-wise cross-validation (CV), LoSo CV and pure testing. Pure testing evaluation was grouped by scanners in the testing set.

Quality control (classification)					Quality assessment (regression)			
	Weighted F1 (↑)	ROC AUC (↑)	Precision (↑)	Recall (↑)		R^2 (↑)	Spearman (↑)	MAE (↓)
10-fold subject-wise cross-validation					10-fold subject-wise cross-validation			
`dl_slice` (Xu et al., 2020)	0.64 (0.65)	0.72 (0.61)	0.71 (0.73)	0.98 (0.86)				
`dl_stack` (Legorreta et al., 2020)	0.71 (0.72)	0.77 (0.73)	0.78 (0.80)	0.85 (0.81)				
NiftyMIC-QC (Ebner et al., 2020)	0.76 (0.75)	–	0.76 (0.77)	0.96 (0.96)	Subject-wise oracle	0.33 (0.39)	0.53 (0.68)	0.65 (0.61)
Base	0.82 (0.78)	0.88 (0.79)	0.85 (0.83)	0.92 (0.84)	Base	0.40 (0.38)	0.69 (0.68)	0.59 (0.61)
FetMRQC	0.86 (0.79)	0.91 (0.87)	0.86 (0.85)	0.94 (0.86)	FetMRQC	0.60 (0.49)	0.80 (0.75)	0.50 (0.56)
FetMRQC-20	0.86 (0.77)	0.92 (0.87)	0.86 (0.85)	0.93 (0.81)	FetMRQC-20	0.60 (0.53)	0.79 (0.78)	0.50 (0.53)
Leave-one-Scanner-out cross-validation					Leave-one-Scanner-out cross-validation			
`dl_slice` (Xu et al., 2020)	0.61 (0.47)	0.75 (0.60)	0.70 (0.62)	0.96 (0.93)				
`dl_stack` (Legorreta et al., 2020)	0.64 (0.53)	0.75 (0.62)	0.69 (0.47)	0.90 (0.87)				
NiftyMIC-QC (Ebner et al., 2020)	0.75 (0.66)	–	0.76 (0.71)	0.95 (0.86)	Subject-wise oracle	0.29 (0.40)	0.48 (0.58)	0.64 (0.64)
Base	0.78 (0.63)	0.80 (0.76)	0.80 (0.69)	0.84 (0.67)	Base	0.29 (0.25)	0.59 (0.48)	0.64 (0.66)
FetMRQC	0.80 (0.64)	0.89 (0.74)	0.85 (0.71)	0.86 (0.73)	FetMRQC	0.45 (0.39)	0.74 (0.72)	0.56 (0.60)
FetMRQC-20	0.82 (0.72)	0.90 (0.83)	0.85 (0.76)	0.88 (0.83)	FetMRQC-20	0.52 (0.36)	0.77 (0.71)	0.55 (0.62)
Pure testing (KISPI + CHUV + La Timone – by scanner)					Pure testing (KISPI + CHUV + La Timone – by scanner)			
`dl_slice` (Xu et al., 2020)	0.73 (0.76)	0.79 (0.79)	0.77 (0.77)	0.97 (0.92)				
`dl_stack` (Legorreta et al., 2020)	0.62 (0.60)	0.72 (0.51)	0.68 (0.67)	0.97 (0.86)				
NiftyMIC-QC (Ebner et al., 2020)	0.74 (0.52)	–	0.70 (0.65)	0.98 (1.00)	Subject-wise oracle	0.41 (0.41)	0.60 (0.60)	0.45 (0.45)
Base	0.77 (0.54)	0.77 (0.62)	0.80 (0.65)	0.97 (1.00)	Base	0.26 (0.36)	0.45 (0.47)	0.65 (0.37)
FetMRQC	0.82 (0.67)	0.77 (0.76)	0.83 (0.70)	0.91 (0.91)	FetMRQC	0.35 (−0.74)	0.59 (0.39)	0.51 (0.65)
FetMRQC-20	0.79 (0.56)	0.74 (0.64)	0.78 (0.65)	0.93 (0.94)	FetMRQC-20	0.30 (−0.94)	0.54 (0.31)	0.53 (0.68)

3.2. Performance and robustness of FetMRQC

Based on the ratings from FetMRQC, we considered two tasks: a quality control (QC) task, where we aimed at predicting whether a scan should be excluded (rating below 1), and a quality assessment (QA) task, where we predicted the interval rating (between 0 and 4). Results from the experiment are summarized in Table 3. A more detailed outlook at the variations in performance across scanners in the LoSo cross-validation and pure testing performance is available in Fig. 3. As expected, the three increasingly challenging evaluation settings (10-fold CV, LoSo CV, pure testing) led to a decrease of performance. This decrease is less notable for QC than QA.

Quality control. Overall, FetMRQC and FetMRQC-20 consistently performed best with a performance (weighted F1) of 0.86, 0.80 and 0.82 in median for the cross-validation, leave-one-out scanner and pure testing scenarios respectively. This performance is consistent across the evaluation metrics considered (3). Precision is of great interest in our case, as including bad quality in further analysis can be greatly detrimental to further processing. FetMRQC shows a consistently high precision in all settings considered, with median performance of 0.86, 0.85 and 0.83 in CV, LoSo CV and pure testing respectively.

Focusing on the scanner-wise breakdown of performance (Fig. 3A and B), FetMRQC and FetMRQC-20's performance is very consistent across almost all scanners considered, and does not change on new scanners from sites used in training (Siemens' MAGNETOM Vida at CHUV and BCNatal - GE's Discovery MR750 at Kispi). On the other hand, DL-based methods (Legorreta et al., 2020; Xu et al., 2020), trained on homogeneous data from a single site, fail to perform and exhibit very large variations in performance across sites, making them generally unreliable. We note also that a few scanners were consistently challenging for the models. On panel A, we see that all methods except NiftyMIC-QC and FetMRQC-20 struggled on the CHUV - Skyra scanner. On panel B, we see that FetMRQC managed to generalize well to unseen scanners from known sites (BCN, KISPI and CHUV). However, all models, except `dl_slice`, poorly generalized to data from La Timone.

Quality assessment. In the case of quality assessment, we observed that FetMRQC's new IQMs were instrumental in achieving a performance above the subject-wise oracle. On Table 3B, we see that while the IQMs used in the base model (R^2= 0.49) were sufficient to outperform the subject-wise oracle (R^2 = 0.33) in the subject-wise CV, using FetMRQC with either all IQMs (R^2 = 0.44) or the selected 20 (R^2 = 0.49) was necessary to achieve a performance over the subject-wise oracle (R^2 = 0.29) in the LoSo setting. This was nonetheless not sufficient to achieve a satisfying performance in the pure testing setting, where FetMRQC's prediction, despite outperforming consistently over the base model, do not outperform the subject-wise oracle. It also fails on one scanner (CHUV - MAGNETOM Vida scanner, Fig. 3D), but we hypothesize that such drop is likely due to the small amount of data available from this scanner.

3.3. Generalization as a function of scanner diversity and number of training examples

Data annotation is known to be a time-consuming process that requires highly specialized raters (Rädsch et al., 2023). Given a limited budget (in time and expertise), the question of which data to annotate then raises naturally. In this experiment, we investigated how the number of scanners n_{scanner} and the number of data n_{training} available during training impacted the generalization performance of FetMRQC in the context of LoSo CV. We had in total 8 different scanners and 1251 data points. For a given configuration (n_{scanner}, n_{training}), we performed a LoSo CV where the data used in training were subsampled: between 1 and 7 scanners were sampled randomly from the available data and between 100 and 900 data points were then randomly sampled from the available scanners. For each (n_{scanner}, n_{training}), the experiment was repeated 20 times.

Fig. 4 contains the results of the experiment, showing the minimum, maximum and median performance with the deviation from the median, across 20 repetitions. In each case entry, the reported measure was computed as the average across the 20 repetitions. Looking at the median performance, it is clear that increasing the size of the training

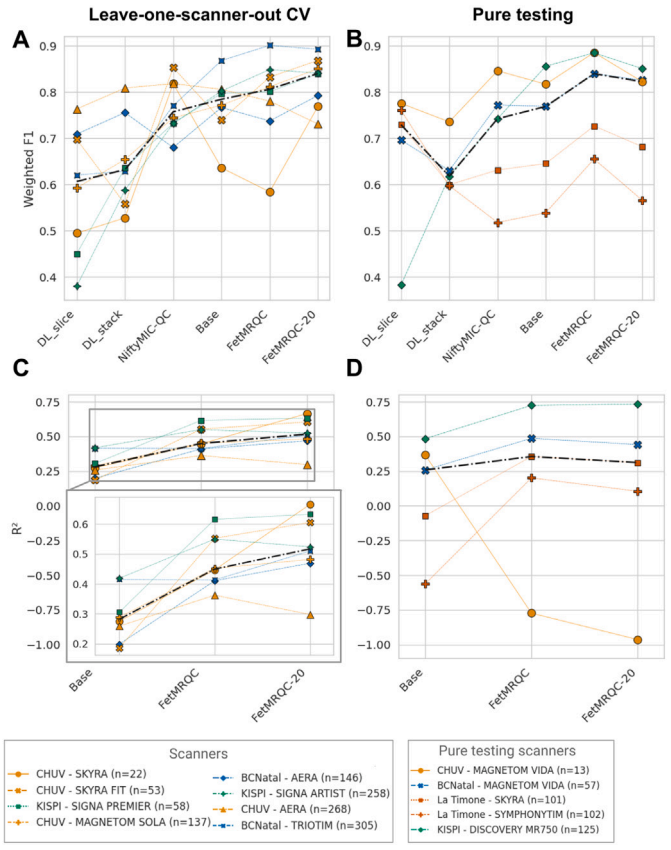

Fig. 3. Scanner-wise results for QA/QC. A – Weighted F1 score for the QC task for each scanner used in LoSo cross-validation (sorted from the one with the least subjects to the most subjects). B – Weighted F1 score for the QC task for each scanner used in the pure testing set. C – R^2 for the QA task for each scanner used in LoSo cross-validation. D – R^2 for the QA task for each scanner used in the pure testing set. Distribution of scores is aggregated by scanner, and the median performance for each method is shown as the black dashed line. The red line in the prediction task at 0 shows the baselines for a constant predictor. These results detail the ones presented in Table 3.

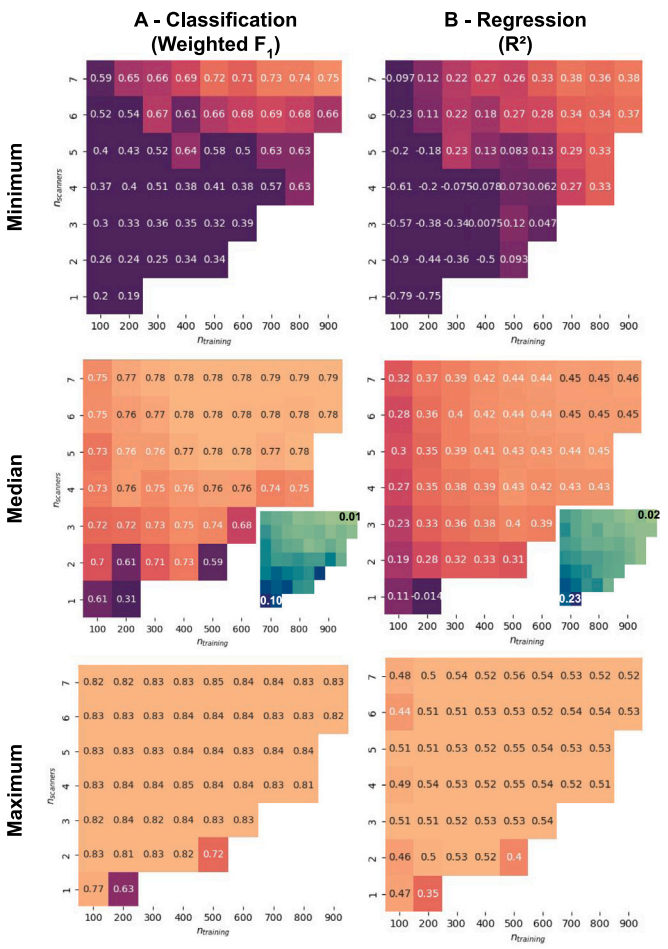

Fig. 4. Performance as a function of the number of scanners and training points. This is obtained by performing leave-one-scanner-out cross-validation 20 times, using different random subsets of data. (Top row.) Minimum (worst-case) performance across folds (Middle row.) Median performance across folds. The smaller plots show the corresponding median average deviation. (Bottom row.) Maximum (best-case) performance across folds.

set (x axis) or the number of scanners (y axis) both improve the generalization. Starting with best-case generalization (maximum performance, lower row in Fig. 4), we see that in every case, there is a subset of data that enables reaching the best performance with only 100 data points. While this is not surprising, this is also difficult to exploit: one cannot readily find ahead of time a subset of data that will generalize well to the testing data. The worst-case generalization is more interesting: using 100 training data points from seven scanners reaches a similar performance as using 700 data points from four scanners in the case of classification. In the case of regression however, we see that both the number of training samples and scanners is important: the worst-case generalization with 100 training data and 7 scanners is close to zero, and the performance steadily increases with more data.

Overall, using multiple scanners is key to achieving the highest performance regimes, but using more data is also greatly valuable. However, if constrained to a limited annotation budget, we anticipate that annotating more diverse data from various scanners will be more helpful for generalization than gathering a large corpus from a single scanner.

In addition, we also observe, on the median performance, that the classification task is generally more straightforward than the regression task: fewer data allow to reach the highest performance, while performance keeps increasing for regression when adding more sites and more data. Thus, we hypothesize that regression performance would further be increased by increasing the size of training data. In contrast, the median classification performance might stagnate, although its worst case performance might still improve, thus making the model more robust to new scanners by further enhancing the training dataset.

3.4. Model performance on a restricted set of IQMs

FetMRQC relies 332 different IQMs that are not fully independent from each other, as shown in Figure 6. In this final experiment, we explore the IQMs that are most important for FetMRQC QA and QC models.

We computed the feature importance of the random forest model used in each fold of the LoSo CV and average them across folds. We grouped together the IQMs with a correlation coefficient above 0.95 (as shown in Figure 6) to prevent several IQMs contributing very similar information but selected by different models in the LoSo CV for QA and QC. We then randomly selected a single IQM from each correlated group, and arrived at the ranking shown in the top row of Fig. 5. First, we see that in the QC task (A), IQMs are generally spread out (the top four IQMs sum up to 0.20). In the regression task (B) however, a few IQMs capture a large part of the feature importance (the top four IQMs sum up to 0.53). Nonetheless, three IQMs are consistently among the top predictors: rank-based error (Kainz et al., 2015), the volume of the brain mask and the morphological closing of the brain mask. The first estimates the consistency of the intensities across slices by computing how well a low-rank approximation can represent the volume, the

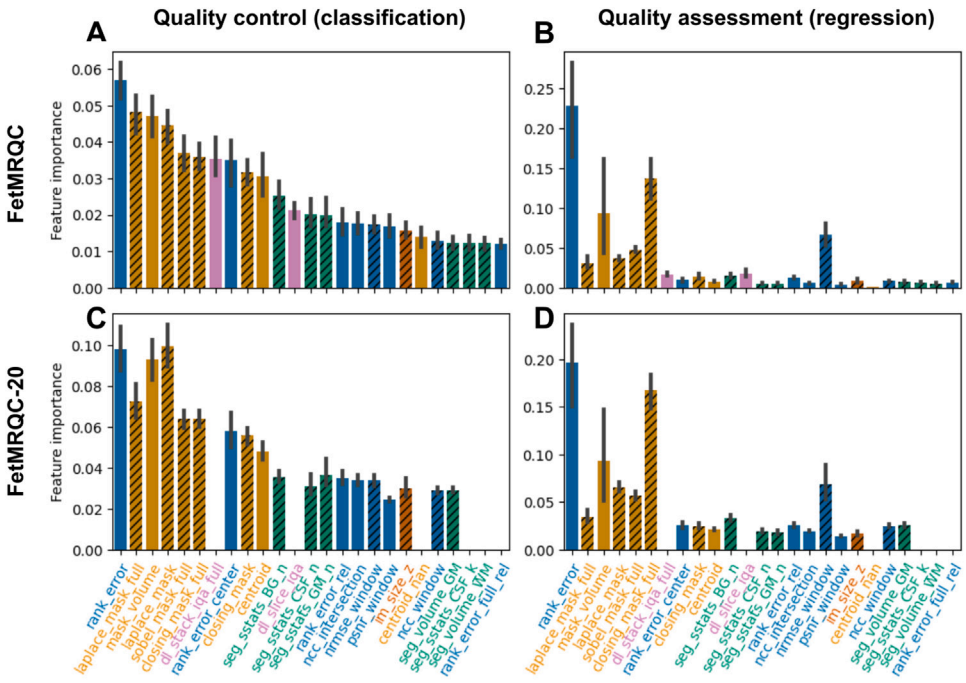

Fig. 5. Most important IQMs for QA/QC. Feature importance for quality control (classification) on the left, and for quality assessment (regression) on the right. The top row shows the top-25 IQMs from FetMRQC and the bottom row shows the 20 selected IQMs that form FetMRQC-20. Blue IQMs are intensity-based, orange are mask- (or shape) based, green are segmentation based, pink are deep-learning based and brown are metadata based. Hatched features denote the new ones proposed in this work. The error bars are the standard deviation over the different cross-validation folds, performed over different scanners. Note that the scales are very different between the plots: the highest feature importance for classification is around 0.055, whereas it is around 0.23 for regression.

second estimates the volume of the brain and the third estimates the degree of motion across stacks by computing a morphological closing of the brain mask in the through-plane direction and then subtracting the original brain mask. The first two IQMs are the ones that have been used in NiftyMIC-QC (Ebner et al., 2020) and complement each other well. Secondly, we see that although the ranking of the most important IQMs can vary, overall 19 out of the 25 IQMs of Fig. 5A and B appear in common in both tasks as the most important IQMs. Thirdly, let us note that the best IQMs cover different representative families of features: intensity-based, mask (or shape)-based, and segmentation-based IQMs. Finally, note that features proposed within FetMRQC rank highly in terms of feature importance: 14 out of the 25 IQMs shown in Fig. 5A and B were proposed in this work.

FetMRQC-20 is built on the feature importance obtained for FetMRQC (Fig. 5A and B). The IQMs were selected by averaging the feature importance from QC and QA, and then by selecting the top-20 features. In order to keep the reduced model as interpretable as possible, we excluded the deep learning (DL)-based IQMs from FetMRQC-20 and replaced them with the two features that came next in line. Results in Table 3 show that does not yield a decrease in performance. The feature importance using only FetMRQC-20's IQMs is shown on Fig. 5C and D and is generally consistent with FetMRQC's results. As fewer IQMs are available, their relative importance is generally higher, and the same IQMs end up carrying the largest weight in decision.

4. Discussion

In this work, we proposed FetMRQC, a novel open-source machine learning framework for the automated quality control and quality assessment of fetal brain MRI. While most existing works focus on a single-center, single-scanner setting (Legorreta et al., 2020; Xu et al., 2020; Gagoski et al., 2022), the evaluation in this work was carried out on a large, multi-scanner, multi-centric dataset. These diverse data allowed us to measure the impact of domain shift on generalization, and assess the variability in performance across scanners. Being trained with multi-centric data FetMRQC achieves a reliable performance in quality control over most scanners considered, which is not the case for baseline DL-methods, trained on homogeneous data, which exhibit a very large variability in performance. These observations were made possible by following good practices regarding evaluation and reporting of dataset with domain shifts (Roberts et al., 2021; Varoquaux and Cheplygina, 2022; Zech et al., 2018). Indeed, cross-validation at the group level (subject or scanner in our case) (Varoquaux et al., 2017), computing the performance metrics at the group level and reporting the worst-performing site were essential in unfolding the large variability in performance, which is obfuscated when averaging across the entire testing set (Dockès et al., 2021; Zhou et al., 2023). Designing a pure testing (Varoquaux and Cheplygina, 2022; Kapoor and Narayanan, 2022) set comporting both unseen scanners and unseen sites allowed to observe another trend: methods performing well in the LoSo CV setting performed well on unseen scanners from known sites, but struggled on the unseen site. Indeed, data from La Timone were very different from the ones acquired at other institutions: only three stacks were acquired per subject due to strong constraints on the duration of the scanning session, and the acquisition was done at a high in-plane resolution, leading to higher level of noise in the images compared to the rest of the data.

Beyond measuring the impact of domain shifts, several methods to correct and compensate them on tabular data have been proposed, including group-wise normalization of data (Esteban et al., 2017), or empirical Bayes approaches, like ComBat (Johnson et al., 2007). As shown in our supplementary experiments, we did not find these approaches to be beneficial in our case, which is most likely due to the quality of data being related to the scanner on which data were acquired: removing the scanner information at the IQM level might not be helpful because it might remove meaningful information (Dockès et al., 2021). This might be mitigated by attempting to directly harmonize the input T2w images (Zhou et al., 2023; Wang et al., 2022) rather than the IQMs, as the IQMs were directly extracted from images acquired with widely different imaging parameters that could induce some confounding factors in the derived metrics.

A question that can be raised is whether a deep models (like convolutional neural networks (CNN) or transformers (Vaswani et al., 2017)) could serve as an alternative to FetMRQC. FetMRQC operates in a highly heterogeneous setting, with relatively few, high dimensional data points when compared to deep learning standards — where datasets commonly feature more than $10^5 - 10^6$ data points (Deng et al., 2009; Varoquaux and Cheplygina, 2022). Using our data, we were unable to train a CNN or a transformer model that would outperform FetMRQC. In addition, the trained models exhibited unstable generalization performances. We hypothesize that the diversity of IQMs of FetMRQC, leveraging image intensity, brain masks and finer segmentations were able to provide a more stable ground for generalization than the one learned by a deep learning model on our data. Our choice of privileging random forests over deep networks in FetMRQC then hinged on practical considerations, rather than the theoretical representation power of deep networks. Nevertheless, deep learning has still been successful for quality control (Legorreta et al., 2020; Xu et al., 2020; Liao et al., 2020) and it is likely that having more data or leveraging semi-supervised (Xu et al., 2020) or self-supervised (Liu et al., 2021; He et al., 2022) learning methods could help build some robust deep models.

Note however that FetMRQC suffers from two main limitations. As any other supervised learning method, the first limitation comes from an often underestimated component of machine learning pipelines, namely the quality of annotations. As QA/QC has an inherently subjective dimension, narrowing the task at hand for rating is key to maximize inter-rater reliability (Esteban et al., 2018; Rädsch et al., 2023). The quality rating interface is an essential tool for displaying the raw T2w fetal brain data uniformly, and when providing the raters with a training session, can successfully lead to high inter-rater reliability. However, our fetal motion and bias field rating results suggest that a finer protocol is needed. The protocol should, in particular, encourage raters to proceed in artifact-based quality ratings: first assessing the presence and degree of various artifacts and then deciding on a score to give rather than the opposite. Improving the inter-rater agreement might further improve the quality of FetMRQC, in particular on the quality assessment task, where the subject-wise CV regression performance comes close to the level of agreement between the raters: $R^2 = 0.58$ for the subject-wise CV and the inter-rater agreement has $R^2 = 0.56$. A second limitation comes from the simplicity of the model: while FetMRQC's predictions are easily interpretable and generally depend on a small number of IQMs, its learning capabilities are limited by its shallow nature. A deep learning model trained directly on 3D clinical acquisitions is likely to improve QA/QC predictions, if enough training data is available, as it can make better use of large amounts of training data.

Beyond addressing these limitations, future work will investigate how preprocessing the raw T2w data might impact FetMRQC's performance. Future work will also include a more thorough evaluation of the impact of FetMRQC on downstream tasks such as super-resolution reconstruction quality. FetMRQC is only a first step towards robust tools for quantitative analysis of fetal neuroimaging. While QA/QC starts at the raw images, it is greatly needed at every stage of the fetal brain MRI pipeline, from acquisition to reconstruction to surface extraction. Such checkpoints, along with community efforts in collecting large, reality-centric datasets are key to developing robust and reliable learning-based approaches for fetal neuroimaging and beyond.

CRediT authorship contribution statement

Thomas Sanchez: Writing – review & editing, Writing – original draft, Visualization, Validation, Software, Resources, Methodology, Investigation, Formal analysis, Data curation, Conceptualization. **Oscar Esteban:** Writing – review & editing, Writing – original draft, Conceptualization. **Yvan Gomez:** Writing – review & editing, Data curation. **Alexandre Pron:** Writing – review & editing, Validation. **Mériam Koob:** Writing – review & editing, Data curation. **Vincent Dunet:** Writing – review & editing, Data curation. **Nadine Girard:** Writing – review & editing, Data curation. **Andras Jakab:** Writing – review & editing, Data curation. **Elisenda Eixarch:** Writing – review & editing, Data curation. **Guillaume Auzias:** Writing – original draft, Data curation. **Meritxell Bach Cuadra:** Writing – review & editing, Writing – original draft, Supervision, Methodology, Investigation, Funding acquisition, Data curation, Conceptualization.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

Raw fetal brain MRI cannot readily be shared because of patient privacy. Derived measures, such as extracted IQMs will be shared along with the codes on Zenodo (under CC BY 4.0 license) and GitHub (under an Apache 2.0 license) respectively, for the results to be reproduced.

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Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.media.2024.103282.

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