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Political Regime, Reproductive Health Outcomes, and FemTech Privacy: An Exploratory Two-Strand Assessment

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Abstract. FemTech tools function within overlapping political and economic environments, which both structure population reproductive-health circumstances and give rise to distinct data-privacy hazards. This exploratory analysis works along two separate analytical tracks. Cross-country modelling draws on World Bank metrics paired with V-Dem political-regime classifications covering 167 nations. Once national income is incorporated into regression specifications, analytical samples shrink to 166 countries for maternal-mortality outcomes and 138 for contraceptive-prevalence estimates. Raw bivariate correlations reveal observable health gradients aligned with regime categories. For maternal mortality, income-adjusted estimates turn weak and lack statistical precision (beta = -0.053; HC3 95% CI -0.202 to 0.095; HC3 p = .481). By contrast, contraceptive prevalence retains a positive regime-linked association within the primary regression (4.83 percentage points per category; HC3 95% CI 0.91 to 8.75; HC3 p = .016). That statistical signal dissipates, however, for the subset limited strictly to 2015-2023 observations (beta = 3.27; HC3 95% CI -1.42 to 7.95; HC3 p = .170). A separate line of evidence compiles selected privacy evaluations together with public Federal Trade Commission enforcement records. Material assembled here confirms real-world app-level privacy vulnerabilities. Collectively the evidence argues for careful interpretive standards, strengthened measurement infrastructure, and precautionary privacy governance.

Keywords: FemTech, reproductive health, political regime, maternal mortality, privacy

1. Introduction

The FemTech sector has expanded from a niche market label into a large-scale consumer-health industry. One commercial market report estimated sector valuation at US$39.29 billion in 2024 and projected US$97.25 billion by 2030, corresponding to a compound annual growth rate of 16.3% [1]. As of June 2024, Flo reported nearly 70 million monthly active users [2], and corporate materials document more than 380 million total app downloads [3]. These metrics demonstrate substantial market reach. Because figures derive from company self-reporting and commercial forecasting, they offer limited insight into population-level public-health conditions.

Public-health considerations nonetheless remain relevant. Reproductive-health mobile applications can expand access to symptom tracking, menstrual-cycle information, pregnancy-related support, and general health education. Simultaneously, these platforms collect

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highly intimate personal health information that often never enters formal clinical health records. Both dimensions merit scholarly attention. Country-level health models cannot trace data flows from individual apps to advertising platforms. Similarly, single-app audits cannot capture population-wide health outcomes.

Two distinct lines of inquiry run through this work. One assesses cross-country relationships between political-regime classification and two reproductive-health outcomes. Estimates are generated with and without statistical adjustment for national income, for maternal mortality as well as contraceptive prevalence. A second body of work draws on independent reviews and regulatory case records, mapping out privacy hazards specific to FemTech and adjacent digital-health applications. Shared institutional contexts for digital reproductive-health research make it reasonable to bring these separate bodies of evidence together. Even so, no statistical test establishes country-level connections across these two bodies of evidence.

2. Background

Digital women's-health tools have gained traction amid persistent underinvestment in women's-health research. An editorial published in Nature Reviews Bioengineering notes that research and development targeting women's health accounts for approximately 5% of global health-research spending, with FemTech funding reaching about US$2.5 billion in 2021, or roughly 3% of digital-health funding [4]. The National Institutes of Health devoted about 10.8% of its budget to women's-health research in fiscal year 2020, or $4,466 million [5]. A separate editorial addresses this same resource gap from the perspective of medical-device engineering and study design practices [6]. World Health Organization estimates indicate endometriosis affects roughly 190 million women of reproductive age globally, while polycystic ovary syndrome impacts 10-13% of women of reproductive age [7, 8]. These epidemiological realities explain the considerable interest surrounding self-management and information-focused digital health tools.

Population-level reproductive-health outcomes follow steep economic and institutional gradients. WHO assessments document a global maternal-mortality ratio decline from 339 deaths per 100 000 live births in 2000 down to 223 in 2020, alongside persistent large-magnitude regional disparities [9]. Any cross-country comparative analysis must therefore account for national-income differences. Analytical frameworks also need to avoid treating broad political-regime metrics as direct proxies for granular health-data legal provisions. The Regimes of the World schema assigns each country-year observation to one of four ordered regime groups: closed autocracy (0), electoral autocracy (1), electoral democracy (2), and liberal democracy (3) [10]. This classification system does succeed at picking up central traits of national political arrangements. Even so, several practically relevant domains fall outside its analytical scope. No explicit scoring is built into this metric for privacy-protection statutes, health-system operational quality, gender-equality outcomes, or the real-world capacity of state institutions to enforce formal rules.

Mozilla's 2022 review applied its proprietary warning rubric to a set of 25 separate digital goods. Twenty of these were period or pregnancy-tracking applications, paired with five wearable hardware units; eighteen triggered the project's formal warning designation [11]. In separate peer-reviewed work, Grundy and co-authors inspected live network traffic originating from 24 Android medicine-focused applications. Their monitoring documented 19 instances where apps relayed user-derived data outside the boundaries of their own platform ecosystem [12]. Worth noting, this investigation's sample was not built around reproductive-health software specifically. Further case-oriented documentation of alleged data disclosures and resulting official regulatory responses can be found within FTC dossiers for Flo and Premom [13, 14]. When read side-by-side, these disparate materials

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surface tangible privacy risks tied to consumer health software. Still, heterogeneous sampling approaches, differing analytical pipelines, uneven geographic reach, and divergent outcome definitions rule out any attempt to collapse all evidence into a single aggregate risk percentage.

3. Methods

Individual countries constitute the unit of analysis for cross-country statistical work. The analytical sample includes 167 countries. Maternal mortality ratio corresponds to modelled maternal deaths per 100,000 live births. Contraceptive prevalence draws from World Bank metrics measuring uptake of any contraceptive method among married women aged 15-49; calculations do not restrict analysis exclusively to modern contraceptive technologies. Gross domestic product per capita uses purchasing-power-parity adjusted constant 2021 international-dollar values [15]. For all three indicators, the dataset draws the most recent available non-missing observation within 2010-2023 and stores the corresponding observation year. Gross domestic product per capita is log-transformed before inclusion within regression models.

Political-regime classifications originate from V-Dem's Regimes of the World four-category metric, distributed via Our World in Data [10]. This analysis relies upon the 5 August 2026 OWID archived dataset and uses 2023 regime classification values. Within the 167-country analytical sample, 27 countries fall under closed autocracy, 51 under electoral autocracy, 55 under electoral democracy, and 33 under liberal democracy. The archived dataset treats Palestine-Gaza and Palestine-West Bank as separate entries and does not provide a combined 2023 regime classification. Although the Palestine (PSE) country row remains within the raw dataset, it is excluded from all regression models requiring a unified regime score. This exclusion reduces the maternal-mortality analytical sample to 166 countries.

Ordinary least-squares regression supplies the core analytical framework for modelling maternal-mortality patterns. Log(MMR) constitutes the outcome variable, with political-regime grouping and log-transformed GDP entered as explanatory terms. Feeding regime scores 0 through 3 into regression as an ordered linear measure carries a built-in equal-spacing premise. Under that premise, each sequential step upward across regime tiers is presumed to generate uniform expected shifts in reproductive-health metrics. Two separate sensitivity exercises probe how model outputs shift once this linear-spacing premise is relaxed. One re-specification discards the ordered continuous trend entirely, replacing it with three binary dummy variables, and uses closed autocracy as the omitted reference group. A separate sensitivity iteration preserves the original ordered regime coding but swaps log-transformed MMR for raw unmodified mortality figures as the dependent measure. The primary contraceptive-prevalence regression mirrors this ordinal regime structure, again incorporating log-GDP adjustment. Among the full starting pool of 167 countries, 139 hold usable contraceptive-prevalence figures. Of those, 138 possess complete records for both prevalence metrics and a harmonized political-regime classification.

Records for contraceptive prevalence display notable heterogeneity in survey timing, while a subset of nations furnish no valid measurements at all. Every country draws on its most recent usable survey reading falling inside the 2010-2023 window. The median observation year lands on 2018, and interquartile spread spans six calendar years. This scattered time distribution introduces analytical trade-offs, addressed through two distinct sensitivity evaluations. One evaluation narrows the dataset exclusively to readings collected from 2015 through 2023. The other integrates observation year—centred on 2018—as an additional regression covariate. Once merged against regime classification tables, this temporally restricted dataset yields 109 unique national observations. Complete-case procedures cannot reconstruct unmeasured data points, nor can the

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available material diagnose the underlying processes driving data absence. For transparency, missing-data tallies are laid out both by regime subgroup and for each individual country.

Each regression computation produces a suite of diagnostic statistics. Outputs span regression coefficients, uncorrected standard errors, two-sided t-distribution derived p-values, HC3-robustified standard errors and corresponding confidence intervals, plus R-squared and adjusted R-squared goodness-of-fit metrics. Cross-national datasets commonly exhibit heteroskedastic error behaviour. This practical concern means HC3-adjusted standard errors form the foundation for all core interpretive judgements. Unadjusted conventional statistics nonetheless remain visible within tables so readers can make side-by-side comparisons. Statistical significance computed for single model runs and robustness patterns observed across alternative specifications are evaluated independently. No attempt is made to merge these two dimensions into a single composite summary marker. Estimates whose HC3 p-value falls in the 0.05-to-0.10 range are treated as borderline evidence. Any estimate returning HC3 p-values of 0.10 or higher falls under the imprecisely-estimated category.

Instead of executing formal meta-analysis, the privacy-related segment of this work organises available material inside a structured evidence matrix. Every source record catalogues what items were investigated, relevant operating platforms, geographic scope, sample dimensions, interpretable numerator-denominator pairs, measured outcome endpoints, analytical workflows, evidence category, limits on cross-source comparison, source citations, and any publicly documented follow-up activities. No computational transformation merges Mozilla warning designations, directly captured network-transfer logs, and FTC regulatory allegations into a single aggregate percentage. Public corporate disclosures confirm Flo completed the independent privacy audit mandated within its FTC settlement during March 2022 [16].

4. Results

Maternal-mortality values display strong right skew. Across the full 167-country dataset, median maternal mortality equals 41 deaths per 100 000 live births; the mean registers 116.9, and values range from 1 up to 993. For the 139 countries with valid contraceptive-prevalence records, median prevalence reaches 54.6%, with observed values spanning 6.9%-85.5%. Bivariate correlation analysis shows moderate negative association between political-regime category and log-transformed maternal mortality ($r = -0.441$, $n = 166$), alongside moderate positive correlation with contraceptive prevalence ($r = 0.446$, $n = 138$). Log-transformed GDP per capita displays stronger negative correlation with log maternal mortality ($r = -0.850$, $n = 167$) and positive correlation with contraceptive prevalence ($r = 0.575$, $n = 139$). Political-regime scores also correlate moderately with log GDP ($r = 0.489$, $n = 166$). This pattern of shared correlation implies national income may function as a potential confounder.

Figure 1 visualizes bivariate maternal-mortality gradients across the four regime categories. Median log(MMR) declines as regime-category values increase, yet individual country datapoints show substantial overlap across every category stratum. Substantial outcome dispersion exists for countries classified within identical regime groupings. Political-regime type neither determines nor delivers close predictive power for national maternal-mortality burden.

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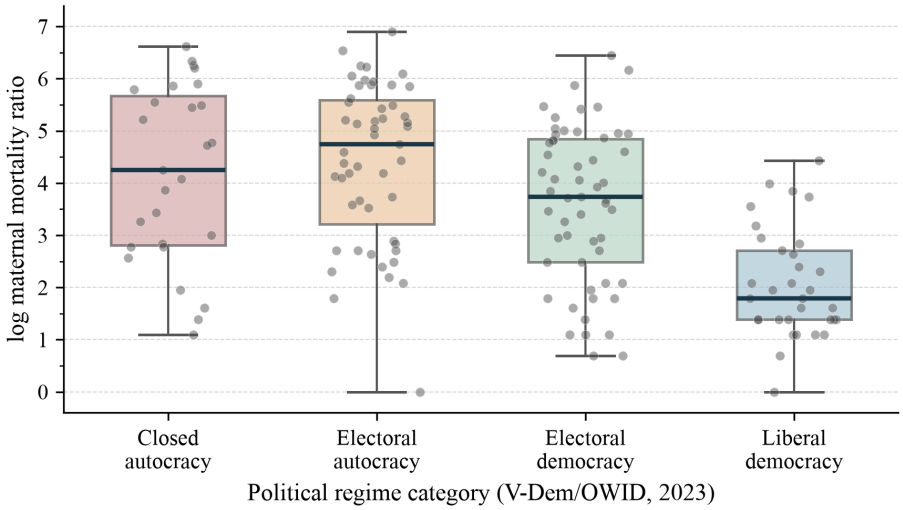


Figure 1. Distribution of log maternal mortality ratio across four political-regime categories. Boxes represent interquartile ranges with medians; points represent individual countries (n = 166)

Within the primary income-adjusted regression model, a one-unit category increase in regime score corresponds to a decrease of 0.053 log points in maternal mortality (conventional SE 0.079, p = .504; HC3 SE 0.075, p = .481; HC3 95% CI -0.202 to 0.095). R-squared equals .731 while adjusted R-squared equals .728. Log-GDP produces a large negative coefficient of -1.203, with HC3 p-value below .001. Outcomes from the two sensitivity specifications align with this core finding. Models using untransformed raw MMR generate coefficient -3.49 deaths per 100 000 live births for each regime-category step (HC3 p = .707). When regime status is recoded into three separate binary indicator variables, none of the contrasts against closed-autocracy reference group achieve statistical significance. Once income differences are controlled for, this mortality association weakens substantially and no longer reaches meaningful statistical precision.

Findings differ for contraceptive-prevalence outcomes. After adjusting for log-GDP per capita, each single-category advance toward liberal democracy is associated with a 4.83-percentage-point rise in contraceptive prevalence (conventional SE 1.68, p = .0047; HC3 SE 1.98, p = .016; HC3 95% CI 0.91 to 8.75). R-squared equals .373 and adjusted R-squared .363. Adding centred observation-year as an extra predictor barely shifts coefficient magnitude, yielding estimate 4.80 percentage points (HC3 95% CI 0.79-8.82; HC3 p = .019). Primary-model estimates maintain statistical significance under HC3 standard-error correction.

Table 1. Income-adjusted political-regime estimates across primary and sensitivity models

Outcome / specification	n	beta	Conventional SE	Conventional p	HC3 SE	HC3 p	HC3 95% CI	R2 / adj. R2
log(MMR), ordinal trend (primary)	166	-0.053	0.079	.504	0.075	.481	-0.202, 0.095	.731 / .728
MMR raw scale, ordinal trend	166	-3.49	10.62	.743	9.25	.707	-21.74, 14.77	.520 / .514
CPR 2010-2023 latest (primary)	138	4.83	1.68	.0047	1.98	.016	0.91, 8.75	.373 / .363
CPR plus observation year	138	4.80	1.69	.0051	2.03	.019	0.79, 8.82	.373 / .359

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Table 1. (continued)

CPR restricted to 2015-2023	109	3.27	2.02	.108	2.36	.170	-1.42, 7.95	.342 / .330

Notes: beta is the coefficient for a one-category increase in political regime (0-3), adjusted for log GDP per capita. HC3 standard errors are used for the main statistical inference; conventional results are reported for comparison. All p values use a two-sided t distribution with residual degrees of freedom.

Temporal sensitivity testing qualifies this primary result. Within the 109-country subset limited to 2015-2023 contraceptive observations, coefficient magnitude falls to 3.27 percentage points, alongside loss of statistical precision (HC3 95% CI -1.42-7.95, p = .170). Categorical model specifications generate comparable patterns. The electoral-democracy contrast produces a 10.61-point difference (HC3 p = .040). Liberal-democracy comparison yields a 12.48-point difference with borderline statistical significance (HC3 p = .053). Estimates for electoral-autocracy contrast remain imprecisely measured. Effect direction stays consistently positive across alternative specifications, yet coefficient magnitude shifts depending upon time-frame selection and variable coding choices.

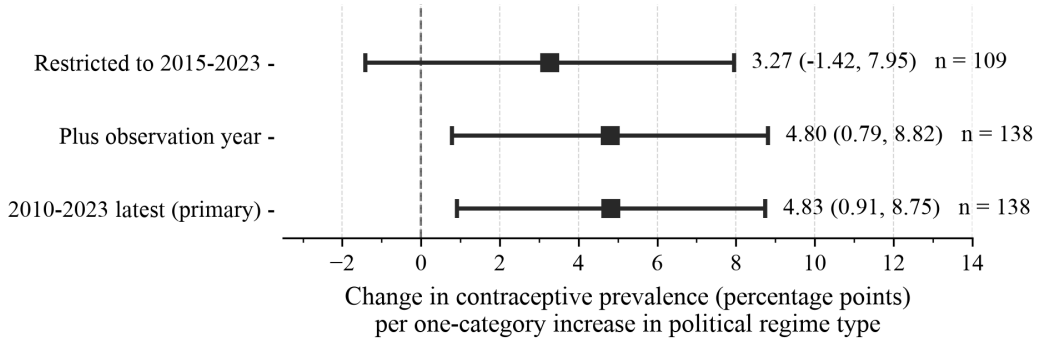


Figure 2. CPR coefficient estimates with HC3 95% confidence intervals across three specifications

Twenty-eight of the original 167 countries lack valid contraceptive-prevalence measurements. Missing observations are unevenly distributed across regime strata: 3 out of 27 closed autocracies (11.1%), 4 out of 51 electoral autocracies (7.8%), 10 out of 55 electoral democracies (18.2%), and 11 out of 33 liberal democracies (33.3%). Missing observations concentrate heavily inside the liberal-democracy subgroup. This makes the missing-completely-at-random assumption hard to defend. HC3 standard-error adjustments correct for heteroskedastic error structures but do not address selection bias.

The privacy-evidence matrix supports restrained interpretation. Mozilla's 18/25 warning count reflects its internal criteria, while the BMJ study observed data sharing in 19/24 medicine-related Android apps [11, 12]. FTC records concern two individual enforcement cases [13, 14], while Flo reports completion of its required independent audit in March 2022 [16]. These sources document credible privacy risks but cannot estimate a global FemTech failure rate.

Table 2. Privacy evidence matrix: distinct endpoints and limits

Source	Object / endpoint	Evidence	Comparability limit or follow-up
Mozilla [11]	20 apps + 5 devices; 18/25 received a Mozilla warning	Independent civil-society review	Warning criteria are not observed transmission; devices included

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Table 2. (continued)

Grundy et al. [12]	24 medicines-related Android apps; 19 shared data outside app ecosystem	Peer-reviewed traffic/network study	Not a reproductive-health sample; endpoint differs from Mozilla
FTC: Flo [13]	Individual fertility-app enforcement case	Regulatory order	Not a prevalence estimate; Flo reports March 2022 independent review [16]
FTC: Premom [14]	Individual ovulation-app enforcement case	Regulatory action	Not a prevalence estimate; later compliance not independently assessed here

5. Discussion

Raw bivariate cross-country patterns link political-regime classification to both reproductive-health outcomes. After national income is controlled for, the maternal-mortality association weakens, yielding small, imprecise point estimates. Contraceptive-prevalence results differ. A positive association remains statistically significant in the primary regression. This result is nonetheless sensitive to model choices. HC3 correction and adjustment for survey-observation year preserve significance. Significance disappears, however, when analysis uses only the smaller, more recent observation subset. Estimate magnitudes also change if ordinal regime trends are replaced with categorical indicators. Overall, the primary model detects a positive association, yet its stability depends heavily on the chosen observation time window.

Maternal-mortality regression outputs cannot definitively rule out the existence of some underlying association. Adjusted point estimates are small relative to their associated uncertainty. Neither outcome-scale transformation nor categorical regime coding restores a clear, robust statistical signal. Observed effect attenuation aligns with income-driven confounding mechanisms. Limitations inherent to this coarse political-regime proxy and model specification constraints may also contribute toward this result. Political-regime classification and national income show moderate pairwise correlation ($r = 0.489$). Large-magnitude shrinkage of the adjusted mortality coefficient relative to bivariate correlation suggests most raw bivariate covariation overlaps with cross-national income disparities. Importantly, this regime metric does not capture many critical contextual factors: health-system performance, armed conflict, gender-based inequities, healthcare service accessibility, formal legal safeguards and real-world policy enforcement [10].

Several contextual pathways may explain the positive association between more democratic regime categories and higher contraceptive prevalence. Prior work links women's political representation to reproductive-health service access, contraceptive uptake and female educational attainment [17]. Education, local health-system capacity and broader economic development are also plausible contributing factors. Uneven survey-measurement dates and known non-random missing outcome data can distort model results. The contraceptive-prevalence metric has built-in limits. It measures uptake of any contraceptive method only among married women aged 15-49 [15]. Reproductive-age people outside this demographic group are not captured. The dataset also cannot distinguish freely informed contraceptive choices from coerced uptake. Causal and normative conclusions go beyond what these observational cross-country data can support.

The privacy strand operates at a different analytical level compared to country-level regression analysis. Product-level audits and regulatory case records show that sensitive reproductive and pharmaceutical-related health data may flow past boundaries end-users reasonably anticipate [11-14]. These findings support stronger oversight for consent workflows, data-minimization rules, data-deletion practices, third-party SDK integrations, and independent post-implementation compliance checks. At the same time, large differences across study designs argue against simple aggregated

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summaries. Mozilla warning labels, BMJ network-traffic records and FTC formal allegations represent three distinct endpoints produced by different analytical methods. Researchers cannot empirically combine these heterogeneous sources into one single percentage estimate.

Policy recommendations should be grounded in available empirical evidence. Cross-country regression coefficients cannot prove causal effects of privacy-law provisions, a consideration relevant for regulators. Audit results and regulatory case records, by contrast, support precautionary regulatory rules [13, 14]. Cross-country findings point to a separate priority: investment in better legal metrics and population-outcome measurement.

This work is exploratory and ecological by analytical design. Analysis draws on a single cross-sectional snapshot, and the 167-country sample does not achieve full global coverage. Contraceptive-prevalence observations come from heterogeneous survey years, and estimation follows complete-case procedures. Political-regime metrics rely on expert-coded four-category classifications [10]. Palestine receives no unified 2023 regime classification inside the archived dataset. Substantial methodological heterogeneity also characterizes the set of privacy-evidence sources brought into review. Given its cross-sectional structure, this analysis cannot sustain causal-inference claims.

6. Conclusion

Bivariate cross-country comparisons show political-regime gradients for maternal mortality as well as contraceptive prevalence, yet patterns diverge once national income is accounted for. The adjusted maternal-mortality estimate shrinks toward zero with low statistical precision. For contraceptive prevalence, the primary model yields a positive statistically significant coefficient, yet the estimate is no longer statistically significant when limiting data to observations collected 2015-2023. These should be interpreted only as exploratory ecological associations; no causal inference about privacy-policy effects can be drawn.

Independent privacy-audit outputs together with FTC regulatory cases separately document tangible concerns around sensitive health-data operational practices. These records establish grounds for continued further investigation alongside precautionary regulatory frameworks. Subsequent research should bring validated legal metrics, enforcement indicators, application adoption statistics, privacy-exposure measurements, and reproductive-health outcomes together at consistent analytical levels. Until such integrated datasets become available, these two separate strands of inquiry remain analytically distinct.

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