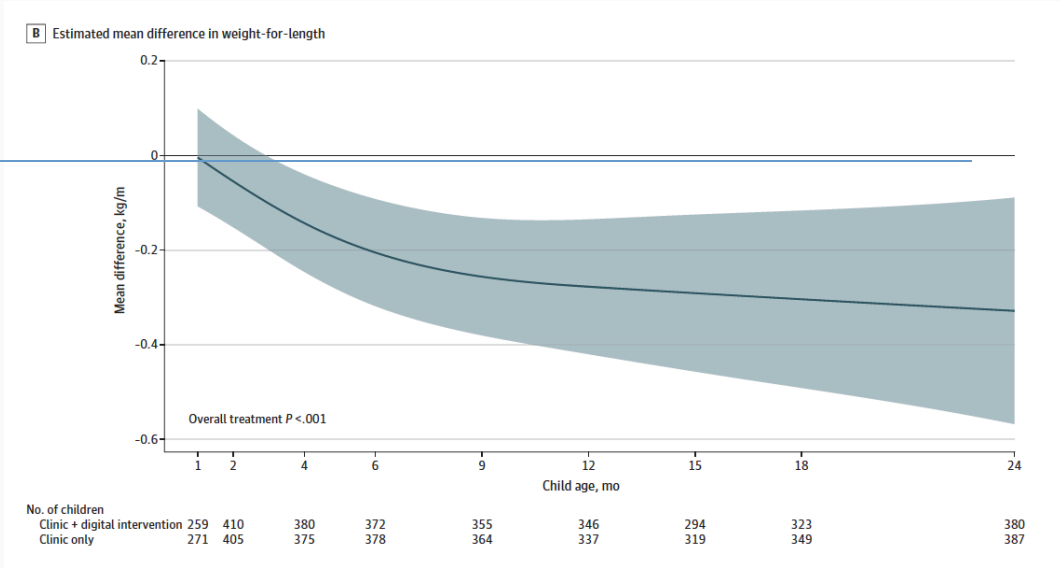
Mathematical Statistics II

STA2212H S LEC9101

Week 11

March 25 2025



JAMA | Original Investigation

A Digital Health Behavior Intervention to Prevent Childhood Obesity
The Greenlight Plus Randomized Clinical Trial

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IMPORTANCE Infant growth predicts long-term obesity and cardiovascular disease. Previous interventions designed to prevent obesity in the first 2 years of life have been largely unsuccessful. Obesity prevalence is high among traditional racial and ethnic minority groups.

OBJECTIVE To compare the effectiveness of adding a digital childhood obesity prevention intervention to health behavior counseling delivered by pediatric primary care clinicians.

DESIGN, SETTING, AND PARTICIPANTS Individually randomized, parallel-group trial conducted at 6 US medical centers and enrolling patients shortly after birth. To be eligible, parents spoke English or Spanish, and children were born after 34 weeks' gestational age. Study enrollment occurred between October 2019 and January 2022, with follow-up through January 2024.

INTERVENTIONS In the clinic-based health behavior counseling (clinic-only) group, pediatric clinicians used health literacy–informed booklets at well-child visits to promote healthy behaviors (n = 451). In the clinic + digital intervention group, families also received health literacy–informed, individually tailored, responsive text messages to support health behavior goals and a web-based dashboard (n = 449).

MAIN OUTCOMES AND MEASURES The primary outcome was child weight-for-length trajectory over 24 months. Secondary outcomes included weight-for-length *z* score, body mass index (BMI) *z* score, and the percentage of children with overweight or obesity.

RESULTS Of 900 randomized children, 86.3% had primary outcome data at the 24-month follow-up time point; 143 (15.9%) were Black, non-Hispanic; 405 (45.0%) were Hispanic; 185 (20.6%) were White, non-Hispanic; and 165 (18.3%) identified as other or multiple races and ethnicities. Children in the clinic + digital intervention group had a lower mean weight-for-length trajectory, with an estimated reduction of 0.33 kg/m (95% CI, 0.09 to 0.57) at 24 months. There was also an adjusted mean difference of −0.19 (95% CI, −0.37 to −0.02) for weight-for-length *z* score and −0.19 (95% CI, −0.36 to −0.01) for BMI *z* score. At

Visual Abstract

Multimedia

Supplemental content

Today

1. Recap Mar 18 intro to causality
2. HW 10 due April 2
3. Course evaluation window open — topics? review?
4. Theory and methods for missing data
5. Project schedule — randomized

Project Schedule April 1

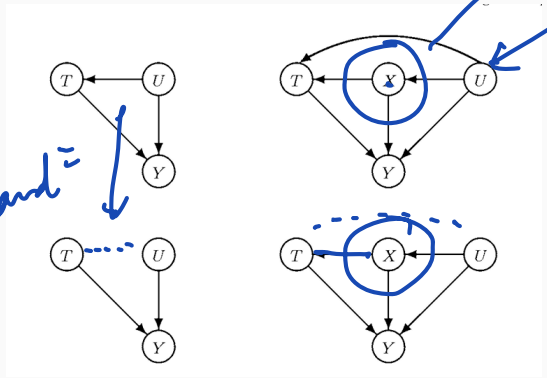
link Start at 10.00

Project Schedule April 1 2025 STA 2212S

Time	Team Members	Title
10.00	Hojung Kim & Markus Kangur	Asymptotics for Lasso type estimators.
10.10	Jingxin Wang & Connie Ens	Testing generalized linear models with high-dimensional nuisance parameters.
10.20	Phyllis Sun & Yufei Liu	Longitudinal data analysis using generalized linear models.
10.30	Abigail McGrory & Aoqi Xie	Models for exceedances over high thresholds.
10.40	Lillian Dong & Nevena Ciganovic	Regression models and life tables.
10.50	Joanna Lo & Adele Lauzon	Quantile regression for survival data.
11.00	Zifan Feng & Shiheng Huang	Quantile regression for longitudinal data.
11.10	Wenqi Shan & Yunqing Xu	A weakly informative default prior distribution for logistic and other regression models.
11.20	Break	
11.30	Alex Faassen & Thanh Huy Dang	Detecting and diagnosing prior and likelihood

Recap causality

- correlation or association is different than causality
- randomized assignment of treatment to units increases the strength of a causal claim



- observational studies can support a causal claim under some assumptions
  - consistency
  - no unmeasured confounding

not testable

from data in 1 study

... Recap causality

- Potential outcomes $Y(a), a = 0$ and 1
- Observed outcomes $Y \mid A = a; a = 0$ or 1
- Causal treatment effect $\mathrm{E}\{Y(1) - Y(0)\}$
- Estimable effect $\mathrm{E}(Y \mid A = 1) - \mathrm{E}(Y \mid A = 0)$
- blue = red if $(Y(0), Y(1)) \perp A$

$Y(a)$ $a \in \mathbb{R}$ or continuous

↑ causal "regression curve"

ATE, ACE

tmt assignment independent of potential outcomes

- in observational studies we rely on adjusting for potential confounders X
- Causal treatment effect $\mathrm{E}_X \mathrm{E}\{Y(1) - Y(0) \mid X\}$
- Estimable effect $\int \mathrm{E}(Y \mid A = 1, X = x) f_X(x) dx - \mathrm{E}(Y \mid A = 0, X = x) f_X(x) dx$
- Estimate

e.g.: $\frac{1}{n} \sum \hat{r}(1, X_i) - \frac{1}{n} \sum \hat{r}(0, X_i)$

some fitted model

+ no unmeasured confounding

$$\frac{1}{n} \sum \hat{\mathrm{E}}(Y \mid A = 1, X_i) - \frac{1}{n} \sum \hat{\mathrm{E}}(Y \mid A = 0, X_i)$$

... Recap causality

- Estimate

same, diff not

$$\frac{1}{n}\sum_{i=1}^{n}\hat{r}(1, X_i) - \frac{1}{n}\sum_{i=1}^{n}\hat{r}(0, X_i)$$

some fitted model

$$\frac{1}{n}\sum \hat{\mathbb{E}}(Y \mid A = 1, X_i) - \frac{1}{n}\sum \hat{\mathbb{E}}(Y \mid A = 0, X_i)$$

- A different estimate

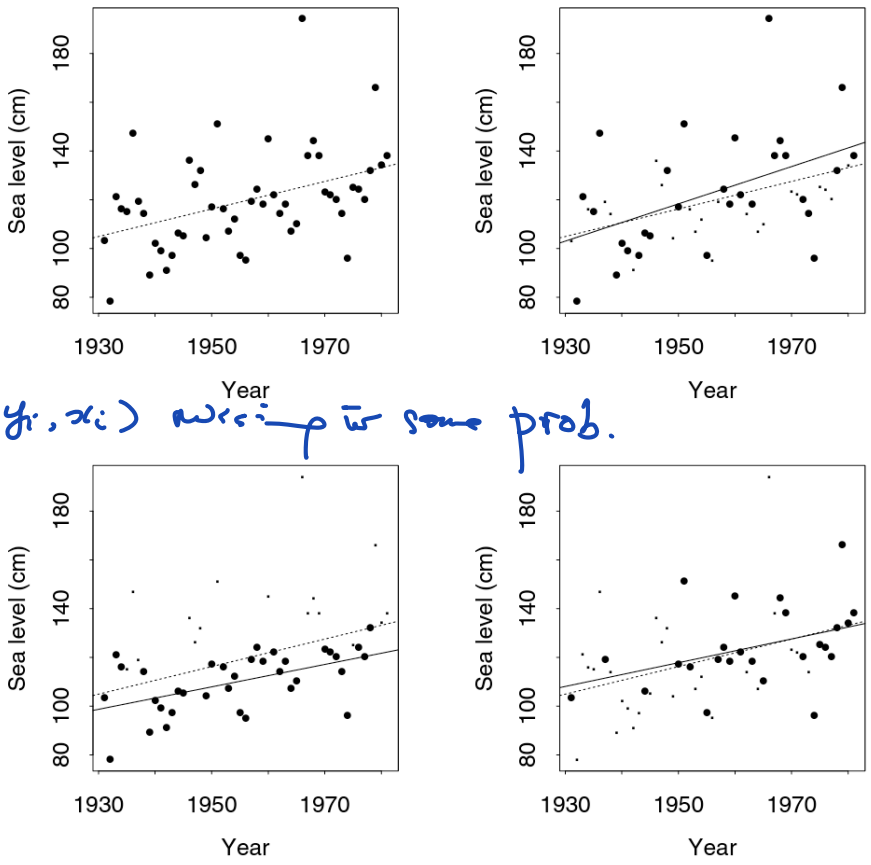
$$\frac{1}{n}\sum_{i=1}^{n}\frac{A_i Y_i}{\widehat{\mathrm{pr}}(A = 1 \mid X_i)} - \frac{1}{n}\sum_{i=1}^{n}\frac{(1 - A_i)Y_i}{\widehat{\mathrm{pr}}(A = 0 \mid X_i)} -$$

- combine these to get a so-called "doubly robust estimator"

Missing data

SM 5.5

Figure 5.12 Missing data in straight-line regression for Venice sea-level data. Clockwise from top left: original data, data with values missing completely at random, data with values missing at random — missingness depends on x but not on y, and data with non-ignorable non-response — missingness depends on both x and y. Missing values are represented by a small dot. The dotted line is the fit from the full data, the solid lines those from the non-missing data.



MCAR completely random

(y_i, x_i) missing w. some prob.

MAR at random

non-ignorable non-response

Models with missing data

- context: independent observations $(y_i, x_i), i = 1, \ldots, n$ — x_i could be a vector
- model $f(\boldsymbol{y} \mid \boldsymbol{x}; \theta)$ or sometimes $f(\boldsymbol{y}, \boldsymbol{x}; \theta)$ — linear regression; glm; etc
- some observations on $\boldsymbol{y}$ may be missing
- e.g. clinical trial, x_i covariate(s) measured at baseline, y_i response after treatment, or after some time has elapsed
- observation on subject i becomes (y_i, x_i, R_i), $R_i = 1$ for complete observation, $R_i = 0$ for incomplete observation

resp cov.

$(?, x_i)$ some i

or $(?, ?)$.

or $(y_i > c, x_i)$

Models with missing data

- context: independent observations $(y_i, x_i), i = 1, \ldots, n$ — x_i could be a vector
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- e.g. clinical trial, x_i covariate(s) measured at baseline, y_i response after treatment, or after some time has elapsed
- observation on subject i becomes (y_i, x_i, R_i), $R_i = 1$ for complete observation, $R_i = 0$ for incomplete observation
- contribution to likelihood function from complete observation

$$f(y_i, x_i, R_i; \theta) = \mathrm{pr}(R_i = 1 \mid x_i, y_i) f(y_i \mid x_i; \theta) f(x_i; \theta)$$

- contribution to likelihood function from incomplete observation — no θ

$$f(x_i, R_i; \theta) = \int \mathrm{pr}(R_i = 0 \mid x_i, y_i) f(y_i \mid x_i; \theta) f(x_i; \theta) dy_i$$

in usual regression settings, $f(x_i)$

Types of missing data

- contribution to likelihood function from incomplete observation $R_i = \text{o}$

$$f(x_i, R_i; \theta) = \int \text{pr}(R_i = \text{o} \mid x_i, y_i) f(y_i \mid x_i; \theta) f(x_i; \theta) dy_i$$

- missing completely at random: $\text{pr}(R_i = \text{o} \mid x_i, y_i) = \text{pr}(R_i = \text{o})$ MCAR
- missing at random: $\text{pr}(R_i = \text{o} \mid x_i, y_i) = \text{pr}(R_i = \text{o} \mid x_i)$ MAR
- non-ignorable non-response $\text{pr}(R_i = \text{o} \mid x_i, y_i)$ no simplification

Types of missing data

- contribution to likelihood function from incomplete observation $R_i = 0$

$$f(x_i, R_i; \theta) = \int \Pr(R_i = 0 \mid x_i, y_i) f(y_i \mid x_i; \theta) f(x_i; \theta) dy_i$$

- missing completely at random: $\Pr(R_i = 0 \mid x_i, y_i) = \Pr(R_i = 0)$ MCAR
- missing at random: $\Pr(R_i = 0 \mid x_i, y_i) = \Pr(R_i = 0 \mid x_i)$ MAR
- non-ignorable non-response $\Pr(R_i = 0 \mid x_i, y_i)$ no simplification

- likelihood function for sample $(y_i, x_i, R_i), i = 1, \ldots, n$

$$L(\theta; \boldsymbol{R}, \boldsymbol{x}, \boldsymbol{y}) = \prod_{i \in \mathcal{M}} \int \Pr(R_i = 0 \mid x_i, y_i) f(y_i \mid x_i; \theta) f(x_i; \theta) dy_i \times \prod_{i \notin \mathcal{M}} \Pr(R_i = 1 \mid x_i, y_i) f(y_i \mid x_i; \theta) f(x_i; \theta)$$

Likelihood function

- likelihood function for sample $(y_i, x_i, R_i), i = 1, \ldots, n$

$$L(\theta; \boldsymbol{R}, \boldsymbol{x}, \boldsymbol{y}) = \prod_{i \in \mathcal{M}} \int \mathrm{pr}(R_i = 0 \mid x_i, y_i) f(y_i \mid x_i; \theta) f(x_i; \theta) dy_i \times \prod_{i \notin \mathcal{M}} \mathrm{pr}(R_i = 1 \mid x_i, y_i) f(y_i \mid x_i; \theta) f(x_i; \theta)$$

- under MAR or MCAR ,

$$L(\theta; \boldsymbol{R}, \boldsymbol{x}, \boldsymbol{y}) \propto \prod_{i=1}^{n} \{f(y_i \mid x_i; \theta)\}^{R_i} f(x_i; \theta)$$

- and very often $f(x_i)$ free of θ, so $L(\theta) \propto \prod_{i=1}^{n_0} f(y_i \mid x_i; \theta)$ as usual
- expected information $I(\theta) = \mathrm{E}_\theta\{-\ell''(\theta)\}$ will depend on $\mathrm{pr}(R_i = 1)$
- use observed information $J(\hat{\theta}) = -\ell''(\hat{\theta})$ for estimating standard error of MLE

Annual maximum sea-level in Venice, 1931 – 1981

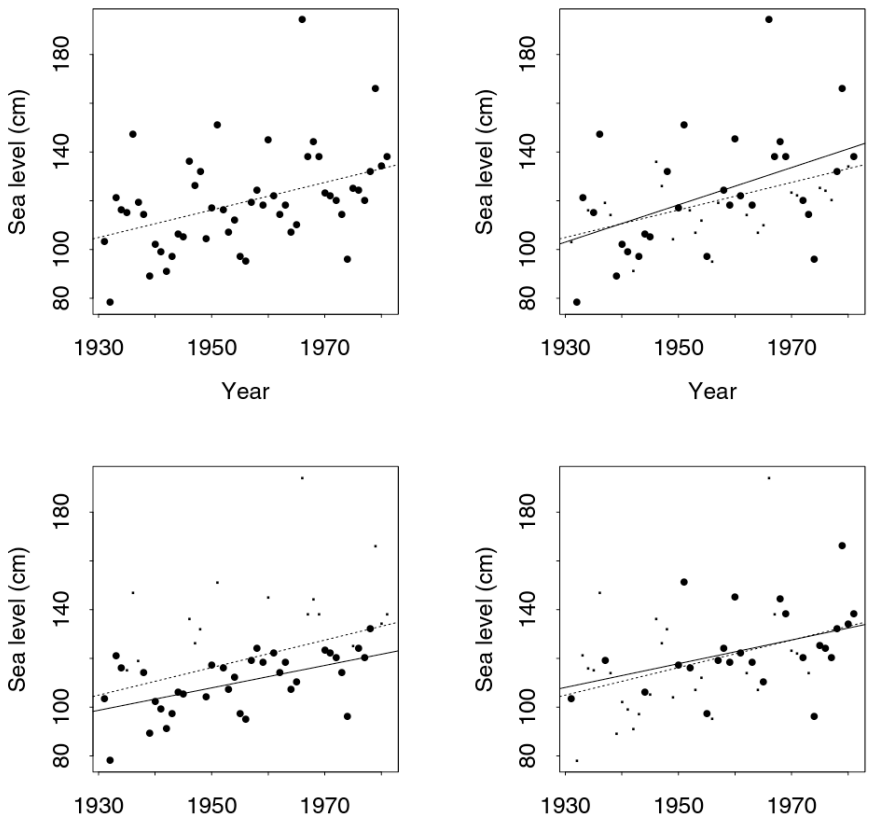

Figure 5.12 Missing data in straight-line regression for Venice sea-level data. Clockwise from top left: original data, data with values missing completely at random, data with values missing at random — missingness depends on x but not on y, and data with non-ignorable non-response — missingness depends on both x and y. Missing values are represented by a small dot. The dotted line is the fit from the full data, the solid lines those from the non-missing data.

```
> faraway::sumary(venice.lm)
                       Estimate Std. Error t value  Pr(>|t|)
(Intercept)          119.60784    2.60729 45.8744 < 2.2e-16
I(year - mean(year))   0.56697    0.17713  3.2009  0.002406

n = 51, p = 2, Residual SE = 18.61977, R-Squared = 0.17
```

simulate 1000 samples from linear model with $\beta_0 = 120$, $\beta_1 = 0.5$, $\sigma = 20$

generate missing data indicators as

$$\text{pr}(R = 1 \mid x, y) = \begin{cases} 0.5, \\ \Phi\{0.05(x - \bar{x})\}, \\ \Phi[0.05(x - \bar{x}) + \{y - \beta_0 - \beta_1(x - \bar{x})\}/\sigma] \end{cases}$$

$$\text{pr}(R = 1 \mid x, y) = \begin{cases} 0.5, \\ \Phi\{0.05(x - \bar{x})\}, \\ \Phi[0.05(x - \bar{x}) + \{y - \beta_0 - \beta_1(x - \bar{x})\}/\sigma] \end{cases}$$

		Average estimate (average standard error)			
	Truth	Full	MCAR	MAR	NIN
β_0	120	120 (2.79)	120 (4.02)	120 (4.73)	132 (3.67)
β_1	0.50	0.49 (0.19)	0.48 (0.28)	0.50 (0.32)	0.20 (0.25)

Table 5.8 Average estimates and standard errors for missing value simulation based on Venice data, for full dataset, with data missing completely at random (MCAR), missing at random (MAR) and with non-ignorable non-response (NIN). 1000 samples were taken. Standard errors for the averages for $\widehat{\beta}_0$ and $\widehat{\beta}_1$ are at most 0.16 and 0.01; those for their standard errors are at most 0.03 and 0.002.

To assess the extent of this bias, we generated 1000 samples from a model with parameters $\beta_0 = 120$, $\beta_1 = 0.5$ and $\sigma = 20$, close to the estimates for the Venice data and with the same covariate x. We then computed maximum likelihood estimates for the full data and for those observations that remain after applying the non-response mechanisms.

Example: Publication bias

Trial	Magnesium r/m	Control r/m	n	$\widehat{\mu}$	$(v/n)^{1/2}$
1	1/25	3/23	48	1.18	1.05
2	1/40	2/36	76	0.80	0.83
3	2/48	2/46	94	0.04	0.75
4	1/50	9/53	103	2.14	0.72
5	4/56	14/56	112	1.25	0.69
6	3/66	6/66	132	0.69	0.63
7	2/92	7/93	185	1.24	0.53
8	27/135	43/135	270	0.47	0.44
9	10/160	8/156	316	−0.20	0.41
10	90/1159	118/1157	2316	0.27	0.15
Meta-analysis			3652	0.41	0.11
ISIS-4	2216/29011	2103/29039	58050	−0.05	0.03

Table 5.9 Data from 11 clinical trials to compare magnesium treatment for heart attacks with control, with n patients randomly allocated to treatment and control; there are r deaths out of m patients in each group (Copas, 1999). The estimated log treatment effect $\widehat{\mu}$ will be positive if treatment is effective; $(v/n)^{1/2}$ is its standard error. The huge ISIS-4 trial is not included in the meta-analysis.

A model for selection bias

- study with n individuals leads to estimate $\hat{\mu} \sim N(\mu, \sigma^2/n)$
- study is published ($R = 1$), if $Z > 0$
- Suppose $\hat{\mu}$ and Z are related according to the model

some measure of randomness in publication

$\text{cor}(U_1, U_2) = \rho$

$$\hat{\mu} = \mu + \sigma n^{-1/2} U_1, \quad Z = \gamma_0 + \gamma_1 n^{1/2} U_2$$

A model for selection bias

- study with n individuals leads to estimate $\hat{\mu} \dot{\sim} N(\mu, \sigma^2/n)$
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- $\mathrm{pr}(R = 1) = \mathrm{pr}(Z > 0) = \Phi(\gamma_0 + \gamma_1 n^{1/2})$
- $\mathrm{pr}(R = 1 \mid \hat{\mu}) = \mathrm{pr}(Z > 0 \mid \hat{\mu}) = \Phi\left\{\dfrac{\gamma_0 + \gamma_1 n^{1/2} + \rho n^{1/2}(\hat{\mu} - \mu)/\sigma}{(1 - \rho^2)^{1/2}}\right\}$

- non-ignorable non-response — unless $\rho = 0$

$$\begin{pmatrix} X_1 \\ X_2 \end{pmatrix} \sim N\left(\begin{pmatrix} \mu_1 \\ \mu_2 \end{pmatrix}, \Sigma \right) \qquad \Sigma = \begin{pmatrix} \Sigma_{11} & \Sigma_{12} \\ \Sigma_{21} & \Sigma_{22} \end{pmatrix}$$

$$(*) \quad X_2 \mid X_1 \sim N\left(\mu_2 + \Sigma_{21}\Sigma_{11}^{-1}(X_1 - \mu_1),\ \Sigma_{22} - \Sigma_{21}\Sigma_{11}^{-1}\Sigma_{12} \right)$$

$$\hat{\beta} = (X^T X)^{-1} X^T y \qquad \operatorname{Var}\hat{\beta} = (X^T X)^{-1}\sigma^2$$

$$y - X\hat{\beta} =$$

$$y = X\beta + \varepsilon$$

A model for selection bias

- study with n individuals leads to estimate $\hat{\mu} \dot{\sim} N(\mu, \sigma^2/n)$
- study is published ($R = 1$), if $Z > 0$ — some measure of randomness in publication
- Suppose $\hat{\mu}$ and Z are related according to the model — $\text{cor}(U_1, U_2) = \rho$

$$\hat{\mu} = \mu + \sigma n^{-1/2} U_1, \quad Z = \gamma_0 + \gamma_1 n^{1/2} U_2$$

- $\text{pr}(R = 1) = \text{pr}(Z > 0) = \Phi(\gamma_0 + \gamma_1 n^{1/2})$
- $\text{pr}(R = 1 \mid \hat{\mu}) = \text{pr}(Z > 0 \mid \hat{\mu}) = \Phi\left\{ \dfrac{\gamma_0 + \gamma_1 n^{1/2} + \rho n^{1/2}(\hat{\mu} - \mu)/\sigma)}{(1 - \rho^2)^{1/2}} \right\}$
- non-ignorable non-response — unless $\rho = 0$
- estimate of μ is biased: — small γ_1

$$E(\hat{\mu} \mid R = 1) = \mu + \rho\sigma n^{-1/2} \zeta(\gamma_0 + \gamma_1 n^{1/2})$$
$$\doteq \mu + \rho\sigma\gamma_1 \zeta'(\gamma_0) + \rho\sigma\zeta(\gamma_0) n^{-1/2}$$

- estimate of μ is biased:

$$E(\hat{\mu} \mid R = 1) = \mu + \rho\sigma n^{-1/2}\zeta(\gamma_0 + \gamma_1 n^{1/2})$$
$$\doteq \mu + \rho\sigma\gamma_1\zeta'(\gamma_0) + \rho\sigma\zeta(\gamma_0)n^{-1/2}$$

small γ_1

- Suppose now we have k published studies of the same treatment $\hat{\mu}_1, \ldots, \hat{\mu}_k$,
- assume $\hat{\mu}_j \sim N(\mu, \sigma^2/n_j)$ same mean, variance depends on study size
-

$$f(\hat{\mu}_j \mid R_j = 1; \mu, \sigma^2, \rho) = \frac{f(\hat{\mu}_j; \mu, \sigma^2)\mathrm{pr}(R_j = 1 \mid \hat{\mu}_j)}{\mathrm{pr}(R_j = 1)}$$

- log-likelihood function

$$\ell(\theta; \hat{\mu}) = -\sum_{j=1}^{k}\left\{\frac{1}{2}\log\sigma^2 + \frac{n_j}{2\sigma^2}(\hat{\mu}_j - \mu)^2 + \log\Phi(a_j) - \log\Phi(b_j)\right\}$$

$$a_j = \gamma_0 + \gamma^1 n_j^{1/2}, b_j = \{a_j + \rho n_j^{1/2}(\hat{\mu}_j - \mu)/\sigma\}(1-\rho^2)^{-1/2}$$

Publication bias and meta-analysis

- log-likelihood function

don't know γ_0 or γ_1 or ρ ; good est. of σ^2

$$\ell(\theta; \hat{\mu}) = -\sum_{j=1}^{k} \left\{ \frac{1}{2} \log \sigma^2 + \frac{n_j}{2\sigma^2} (\hat{\mu} - \mu)^2 + \log \Phi(a_j) - \log \Phi(b_j) \right\}$$

$\hat{\mu}_j \sim (\mu, \frac{\sigma^2}{n_j})$

- if we set $\rho = 0$, $\quad \hat{\mu} = \dfrac{\sum n_j \hat{\mu}_j}{\sum n_j} \dot{\sim} N\left(0, \dfrac{\sigma^2}{\Sigma n_j}\right)$ no publication bias

208 5 · Models

Trial	Magnesium r/m	Control r/m	n	$\hat{\mu}$	$(v/n)^{1/2}$
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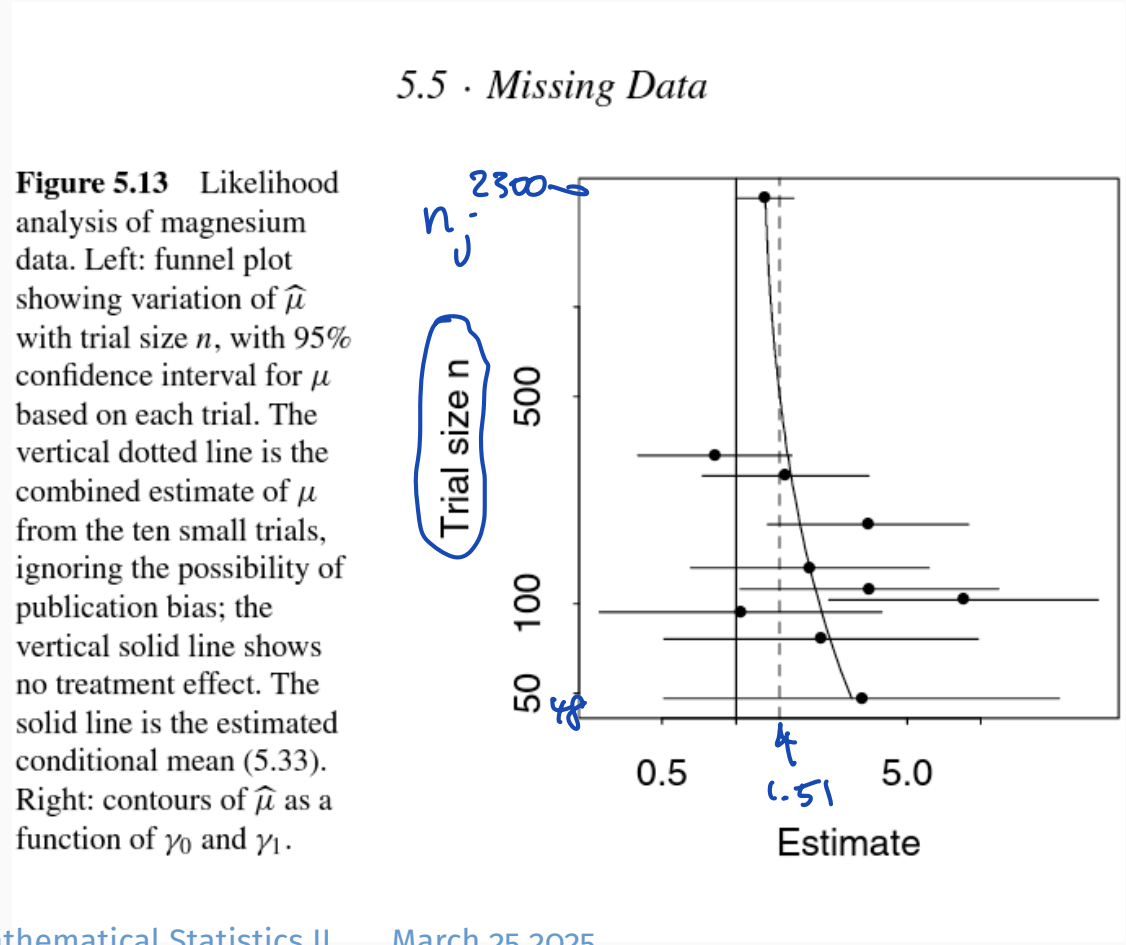
$\exp(\hat{\mu}) = 1.51,$
95% CI $(1.22, 1.86)$

Funnel plot

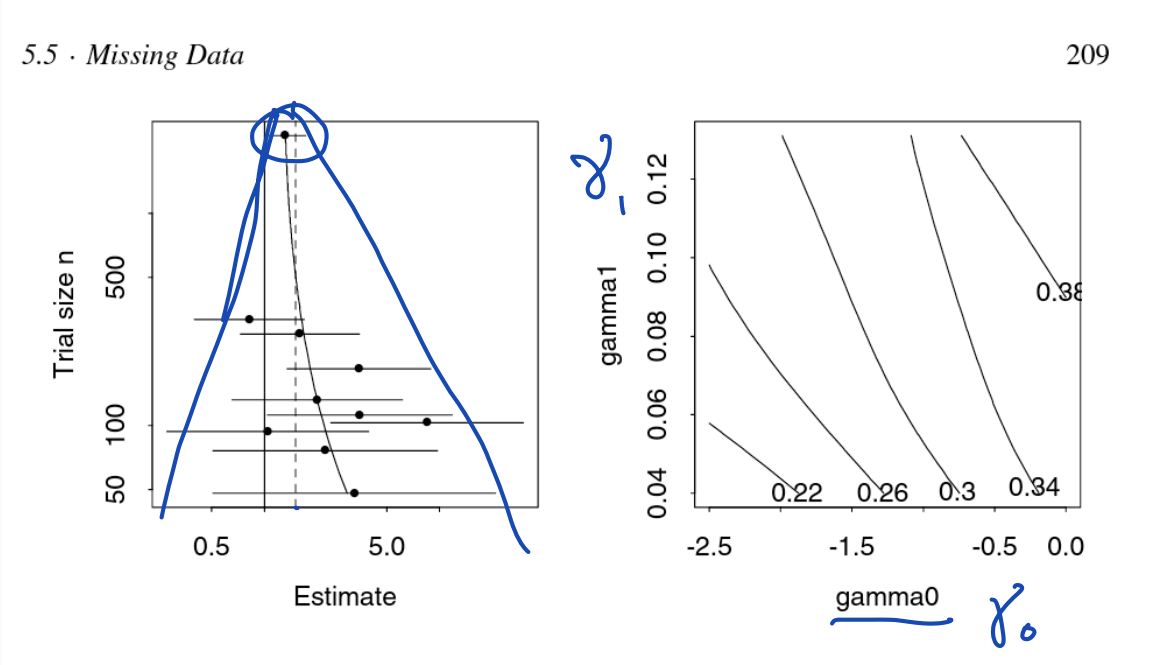


- smaller studies have wider confidence intervals
- seem to be missing small, negative, studies
- simple weighted average is positive (dashed line)
- estimate of average conditonal on publication favours smaller studies

$e^{\hat{\mu}} = e^{0.41} = 1.51$

95% CI $(1.22, 1.86)$

… Dependence on missingness parameters γ_0 and γ_1



back-of the envelope calculation suggests $\hat{\rho} = 0.5$ and $\hat{\mu} = 0.27 \pm 0.12$

$\exp(\hat{\mu}) = 1.31$, 95% CI $(1.03, 1.66)$

Large RCT (ISIS-4) found no benefit

preview

BMJ

BMJ 2011;342:d4002 doi: 10.1136/bmj.d4002

RESEARCH METHODS & REPORTING

Recommendations for examining and interpreting funnel plot asymmetry in meta-analyses of randomised controlled trials

Funnel plots, and tests for funnel plot asymmetry, have been widely used to examine bias in the results of meta-analyses. Funnel plot asymmetry should not be equated with publication bias, because it has a number of other possible causes. This article describes how to interpret funnel plot asymmetry, recommends appropriate tests, and explains the implications for choice of meta-analysis model

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More on funnel plots

Sterne et al. 2011

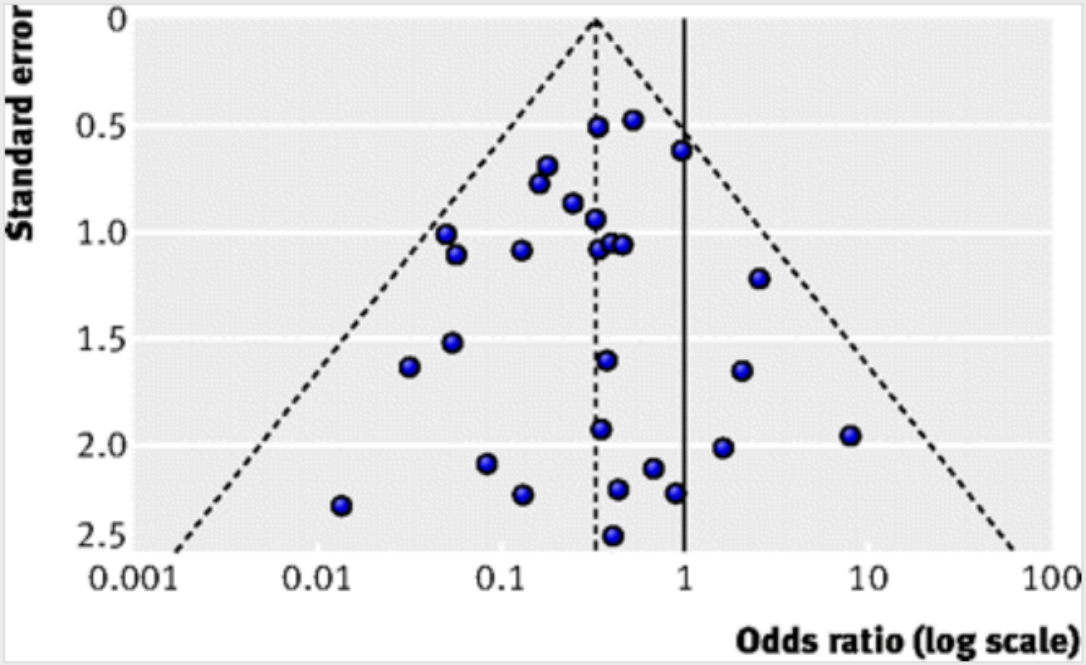
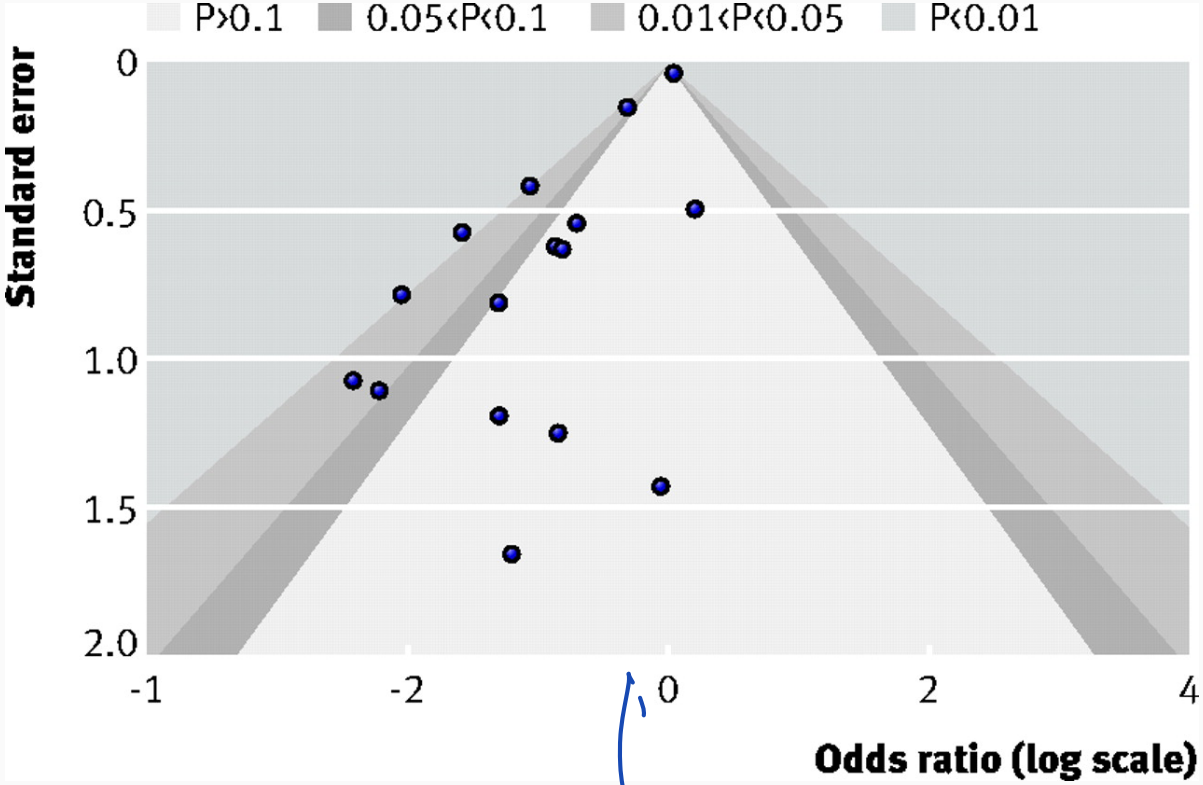


Fig 1 Example of symmetrical funnel plot. The outer dashed lines indicate the triangular region within which 95% of studies are expected to lie in the absence of both biases and heterogeneity (fixed effect summary log odds ratio±1.96×standard error of summary log odds ratio). The solid vertical line corresponds to no intervention effect

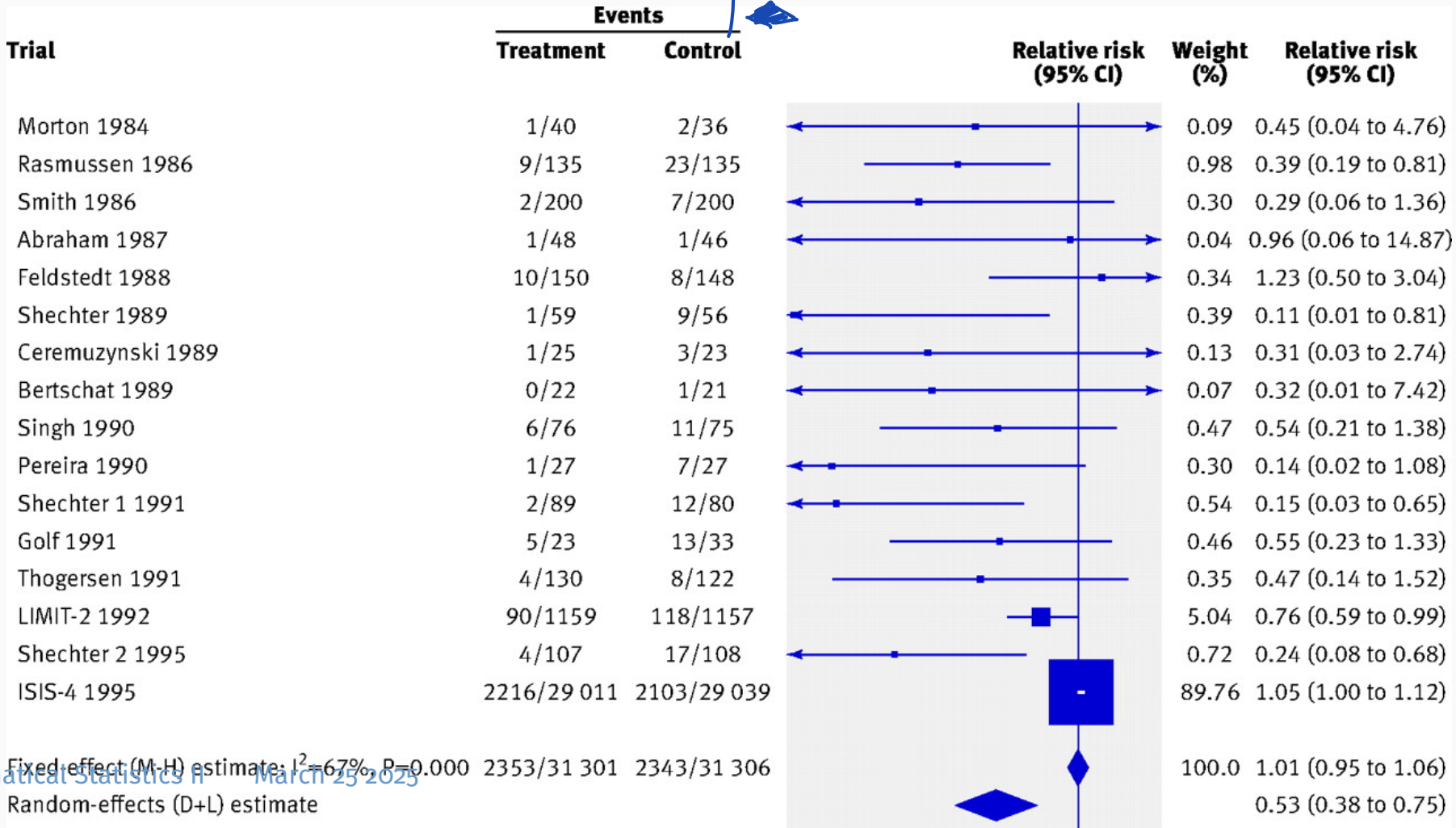
… More on funnel plots

Sterne et al. 2011



Forest plots

Sterne et al. 2011



Trial	Events: Treatment	Events: Control	Relative risk (95% CI)	Weight (%)	Relative risk (95% CI)
Morton 1984	1/40	2/36		0.09	0.45 (0.04 to 4.76)
Rasmussen 1986	9/135	23/135		0.98	0.39 (0.19 to 0.81)
Smith 1986	2/200	7/200		0.30	0.29 (0.06 to 1.36)
Abraham 1987	1/48	1/46		0.04	0.96 (0.06 to 14.87)
Feldstedt 1988	10/150	8/148		0.34	1.23 (0.50 to 3.04)
Shechter 1989	1/59	9/56		0.39	0.11 (0.01 to 0.81)
Ceremuzynski 1989	1/25	3/23		0.13	0.31 (0.03 to 2.74)
Bertschat 1989	0/22	1/21		0.07	0.32 (0.01 to 7.42)
Singh 1990	6/76	11/75		0.47	0.54 (0.21 to 1.38)
Pereira 1990	1/27	7/27		0.30	0.14 (0.02 to 1.08)
Shechter 1 1991	2/89	12/80		0.54	0.15 (0.03 to 0.65)
Golf 1991	5/23	13/33		0.46	0.55 (0.23 to 1.33)
Thogersen 1991	4/130	8/122		0.35	0.47 (0.14 to 1.52)
LIMIT-2 1992	90/1159	118/1157		5.04	0.76 (0.59 to 0.99)
Shechter 2 1995	4/107	17/108		0.72	0.24 (0.08 to 0.68)
ISIS-4 1995	2216/29 011	2103/29 039		89.76	1.05 (1.00 to 1.12)
Fixed-effect (M-H) estimate; I^2=67%, P=0.000	2353/31 301	2343/31 306		100.0	1.01 (0.95 to 1.06)
Random-effects (D+L) estimate					0.53 (0.38 to 0.75)

Inference with missing data

y_i missing x_i obs

or

y_i & x_i both missing

- if MAR or MCAR, can use usual likelihood-based inference with observed information to estimate variance
- if not, but the missing-ness pattern can be modelled, may be able to adjust estimates accordingly

pub bias

- adjustments will depend on the missing-ness model being correct
- there is a large literature on re-weighting standard estimators to accommodate missing-ness
- the potential outcomes model can be viewed as a type of missing data — we see either $Y(1)$ or $Y(0)$ but never both

- what about missing values of covariates?
- use only complete cases – may result in substantial reduction in sample size
- imputation of missing values is a popular choice
- based on prediction of missing covariate value, given observed values of other units

MICE Example

ID	Age_Original	Income_Original	Age_Imp1	Income_Imp1	Age_Imp2	Income_Imp2
1	25	50000	25	50000	25	50000
2		55000	25	55000	50	55000
3	35		35	65000	35	55000
4	40	70000	40	70000	40	70000
5		65000	25	65000	50	65000
6	50		50	75000	50	65000
7	45	80000	45	80000	45	80000
8		90000	35	90000	29	90000
9	38		38	75000	38	70000
10	29	75000	29	75000	29	75000

... Inference with missing data

Research

JAMA | Original Investigation

A Digital Health Behavior Intervention to Prevent Childhood Obesity
The Greenlight Plus Randomized Clinical Trial

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IMPORTANCE Infant growth predicts long-term obesity and cardiovascular disease. Previous interventions designed to prevent obesity in the first 2 years of life have been largely unsuccessful. Obesity prevalence is high among traditional racial and ethnic minority groups.

OBJECTIVE To compare the effectiveness of adding a digital childhood obesity prevention intervention to health behavior counseling delivered by pediatric primary care clinicians.

DESIGN, SETTING, AND PARTICIPANTS Individually randomized, parallel-group trial conducted at 6 US medical centers and enrolling patients shortly after birth. To be eligible, parents spoke English or Spanish, and children were born after 34 weeks' gestational age. Study enrollment occurred between October 2019 and January 2022, with follow-up through January 2024.

INTERVENTIONS In the clinic-based health behavior counseling (clinic-only) group, pediatric clinicians used health literacy–informed booklets at well-child visits to promote healthy behaviors (n = 451). In the clinic + digital intervention group, families also received health literacy–informed, individually tailored, responsive text messages to support health behavior goals and a web-based dashboard (n = 449).

MAIN OUTCOMES AND MEASURES The primary outcome was child weight-for-length trajectory over 24 months. Secondary outcomes included weight-for-length *z* score, body mass index (BMI) *z* score, and the percentage of children with overweight or obesity.

RESULTS Of 900 randomized children, 86.3% had primary outcome data at the 24-month follow-up time point; 143 (15.9%) were Black, non-Hispanic; 405 (45.0%) were Hispanic; 185

Visual Abstract
Multimedia
Supplemental content

"Missing baseline variables were imputed 1000 times with chained equations" (p.4)

A curious example

AoS Ex. 11.9

- data $(X_1, R_1, Y_1), \ldots, (X_n, R_n, Y_n)$ i.i.d.
  1. $X_i \sim$ Uniform from $\{1, \ldots, B\}$
  2. $R_i \sim \text{Bernoulli}(\xi_{X_i})$
  3. If $R_i = 1$, $Y_i \sim \text{Bernoulli}(\theta_{X_i})$
- $\theta = (\theta_1, \ldots, \theta_B)$ unknown, $0 \le \theta_j \le 1$
- $\xi = (\xi_1, \ldots, \xi_B)$ known, $0 < \delta \le \xi_j \le 1 - \delta < 1$
- parameter of interest $\psi = \text{pr}(Y_i = 1) = \sum_{j=1}^{B} \text{pr}(Y_i = 1 \mid X_i = j)\text{pr}(X_i = j) = \frac{1}{B}\sum_j \theta_j$
- An unbiased estimator of ψ:

$$\hat{\psi} = \frac{1}{n}\sum_{i=1}^{n} \frac{R_i Y_i}{\xi_{X_i}}$$

- observed values are averaged, but weighted by probability of being observed
- Horvitz-Thompson estimator

- data $(X_1, R_1, Y_1), \ldots, (X_n, R_n, Y_n)$ i.i.d.
  1. $X_i \sim$ Uniform from $\{1, \ldots, B\}$
  2. $R_i \sim$ Bernoulli(ξ_{X_i})
  3. If $R_i = 1$, $Y_i \sim$ Bernoulli(θ_{X_i})
- one term in likelihood function:
  $f(X_i)f(R_i \mid X_i)f(Y_i \mid X_i)^{R_i} = \frac{1}{B}\xi_{X_i}^{R_i}(1-\xi_{X_i})^{1-R_i}\theta_{X_i}^{Y_i R_i}(1-\theta_{X_i})^{(1-Y_i)R_i}$
- likelihood function: $L(\theta) \propto \prod_{i=1}^n \theta_{X_i}^{Y_i R_i}(1-\theta_{X_i})^{(1-Y_i)R_i} = \prod_{j=1}^B \theta_j^{n_j}(1-\theta_j)^{m_j}$
- $n_j = \#\{i : Y_i = 1, R_i = 1, X_i = j\}$, $\quad m_j = \#\{i : Y_i = 1, R_i = 0, X_i = j\}$
- most $n_j, m_j = 0$ (B very large) $\implies$ mle of θ_j doesn't exist for many j
  $\implies \pi(\theta \mid \text{data}) \propto \pi(\theta)$