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# **HIV Vaccine Efficacy Trial Design: The Post-STEP Era**

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# Imperatives for efficacy trial designs in post-STEP era

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- **More focused (test fewer hypotheses)** (2 minutes)
- **More sensitive (to detect real vaccine effects)** (4 minutes)
- **More safe** (2 minutes)
- **As reliable/interpretable!** (2 minutes)

# Test fewer hypotheses (I)

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Previous TOC designs tested multiple primary and secondary hypotheses

- Trial populations and primary efficacy hypotheses
  - STEP (low Ad5 NAb, overall)
  - Phambili (men, women)
  - PAVE100 (3 human and viral populations)
- Secondary hypotheses
  - Correlates of protection
  - Sieve analyses

**Test efficacy hypotheses in a single, biologically and epidemiologically efficient population**

## Test fewer hypotheses (II)

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Previous TOC designs used co-primary endpoints

- **STEP, Phambili and PAVE100**
  - HIV VL Setpoint
  - HIV Infection

**Test efficacy hypothesis only for  
VL endpoint**

# STOC Design is a more focused TOC Design

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STOC requires smaller trial for VL test of same power

- TOC
  - Co-primary endpoints (Infection, VL)
  - Split Type I error between endpoints (eg .025 for each endpoint)
  - **40 evaluable VL endpoints required** for 80% power against  $\Delta VL$  of 1 log (assume  $VE_s = 0$ )
- STOC
  - Single primary endpoint (VL)
  - Type I error all spent on single test (eg .05)
  - **34 evaluable VL endpoints required** for 80% power against  $\Delta VL$  of 1 log (assume  $VE_s = 0$ )

**Use STOC-type trial designs**

# Additional strategies to improve sensitivity of STOC designs

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- **Definition of VL endpoint**
  - Focus
  - Reduce variability
- **Alternative Type I error rates**
  - Use Type I and Type II error rates appropriate for screening
- **Use of vaccine response rate in design exercise**
  - Predictive model of VL variation in vaccinees
  - Calibrate overall effect size

# Primary Endpoint: “VL Setpoint”

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- **Definition of evaluable VL endpoint**
  - Infection endpoint evaluable only if diagnosed subsequent to clinic visit of final immunization
    - Increase potential signal by considering infections after full immunization
  - VL endpoint the average of multiple pre-ART measurements within pre-specified window of quasi-stable VL
    - Reduce variation of endpoint measurement by restriction to epoch of quasi-stable VL
    - Reduce variation of endpoint by averaging multiple measurements
  - Consider ART-initiation guidelines in selection of study population
    - Rate of ART initiation within first year of Dx but without standard clinical/biomarker indicators

# Type I and Type II Error Rates

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- **For screening trials of candidates in sparse pipelines**
  - **Strict control of Type I error rates (false positives) to very low levels is less critical than in TOC and pivotal trials**
  - **Strict control of Type II error rates (false negatives) to very low levels is more critical than in TOC and pivotal trials**
- **Consider less stringent alternatives to standard Type I error rates (eg,  $\alpha = 0.10$  or higher)**
- **Consider more stringent alternatives to standard Type II error rates (eg,  $\beta = 0.10$  or lower)**

# Minimum Detectable Effect Size at 90% Power\* ( $\Delta_{90}$ )

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Assume 36 evaluable VL endpoints (18:18)

Type I Error (2-sided)	$\Delta_{90}$
.025	1.21
.05	1.12
.10	1.00
.15	0.92
.20	0.87

Standard  $\alpha$  split between co-primary endpoints

Standard  $\alpha$  with single primary endpoint

\*Wilcoxon rank sum test; 16,000 simulations

# Variability of VL Endpoint

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## Standard deviations of VL endpoints in studies of MSM

Study	Group	Number Infections	VL endpoint definition	sd
MACS	Natural history	269	Lyles et al. 2000	.75
Vax004	Ph 3	179 Vx + Plc	Avg of Week 8 and 16 PD	.80
STEP	Ph 2b	44 Vx + Plc	Avg of Week 8 and 12 PD	.88

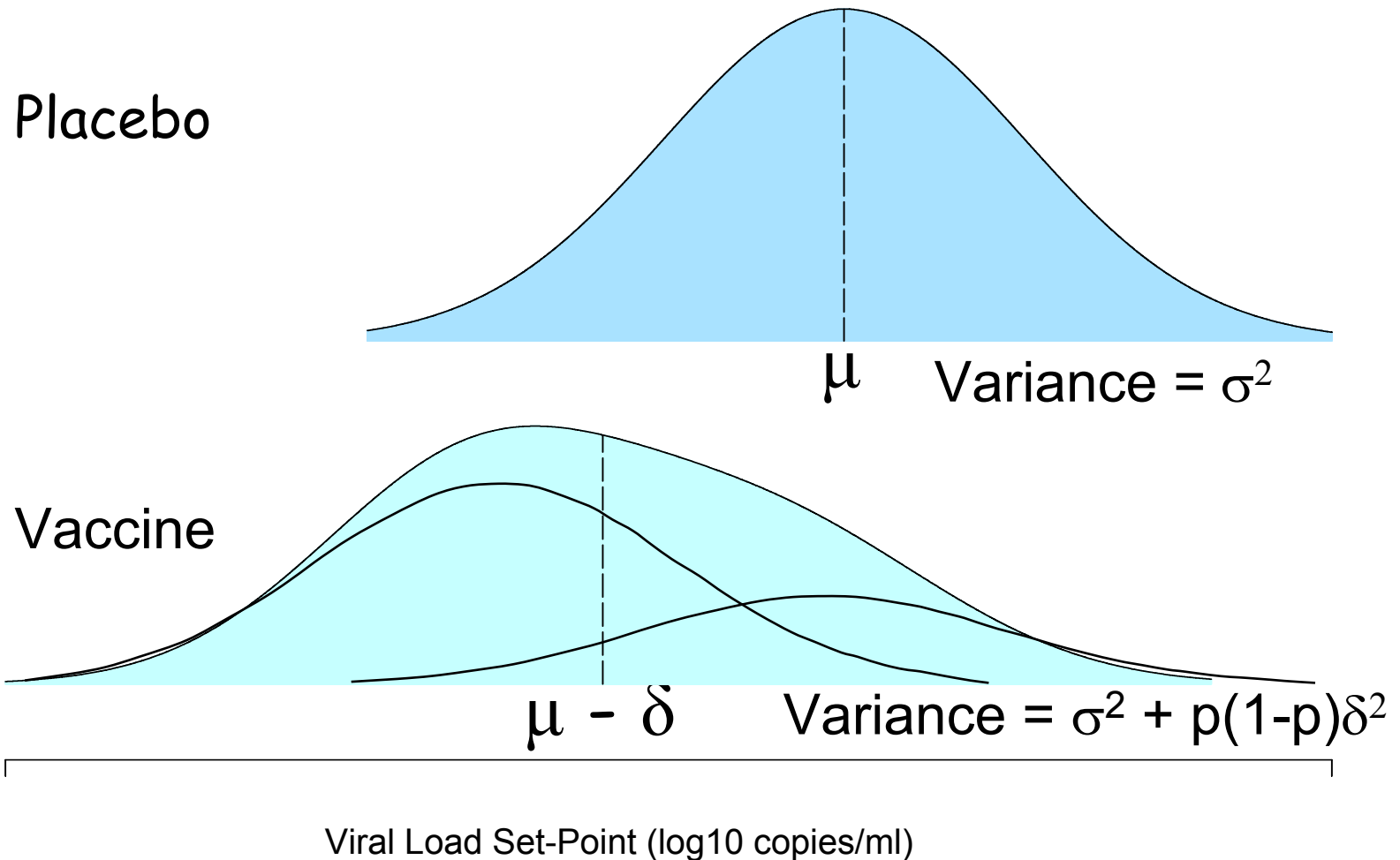
# Vaccine Response Rate (“Take”!?)

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- Is it unrealistic to expect vaccine effect on VL among vaccinees who show no evidence of meaningful response to vaccine?
- If not, use vaccine response rate in two ways
  - **Prediction of variability** of VL measurements in vaccinees to be used in design calculations
  - **Calibration** of overall effect (vaccinees vs controls)
- **Example:**
  - **Assume**
    - vaccine response rate is  $p$
    - variance of VL measurement is same among placebos and among vaccinees who did not response to vaccine ( $\sigma^2$ )
    - $\log_{10}$  VL is lower among responders by  $\delta$
  - **Then**
    - Overall vaccine effect on VL is  $\Delta = p\delta$
    - Distribution of VL endpoint among vaccinees is mixture of 2 distributions and is more variable than that for controls

# Rate of Vaccine Response (p) and Variability of VL among Vaccinees

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Assume VL distribution for vaccinees is mixture of vaccine “responders” (proportion  $p$ ) and “non-responders”; is more variable than for non-vaccinees

# Effect of Vaccine Response Rate on Power and Calibration of overall Effect Size

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Power to detect an overall mean  $\log_{10}$  VL difference ( $\Delta$ )  
 (mean VL difference in “takers”  $\delta$ )\*

Vaccine proportion who “take” p	$\Delta = 0.6$	$\Delta = 0.8$	$\Delta = 1.0$	$\Delta = 1.4$
.5	.55 (1.2)	.69 (1.6)	.77 (2.0)	.82 (2.8)
.6	.58 (1.0)	.76 (1.33)	.85 (1.67)	.92 (2.33)
.7	.60 (.86)	.80 (1.14)	<b>.90 (1.43)</b>	.97 (2.0)
.8	.62 (.75)	.82 (1.0)	.92 (1.25)	.99 (1.75)
.9	.63 (.67)	.84 (.89)	.94 (1.11)	1.0 (1.56)
1.0	.64 (.60)	.85 (.80)	.96 (1.0)	1.0 (1.4)

\*2-sided  $\alpha = 0.10$ ; power = 90%; 36 evaluable endpoints (18 in each group)  
 Wilcoxon rank sum test; 10,000 simulations

# Safety Monitoring

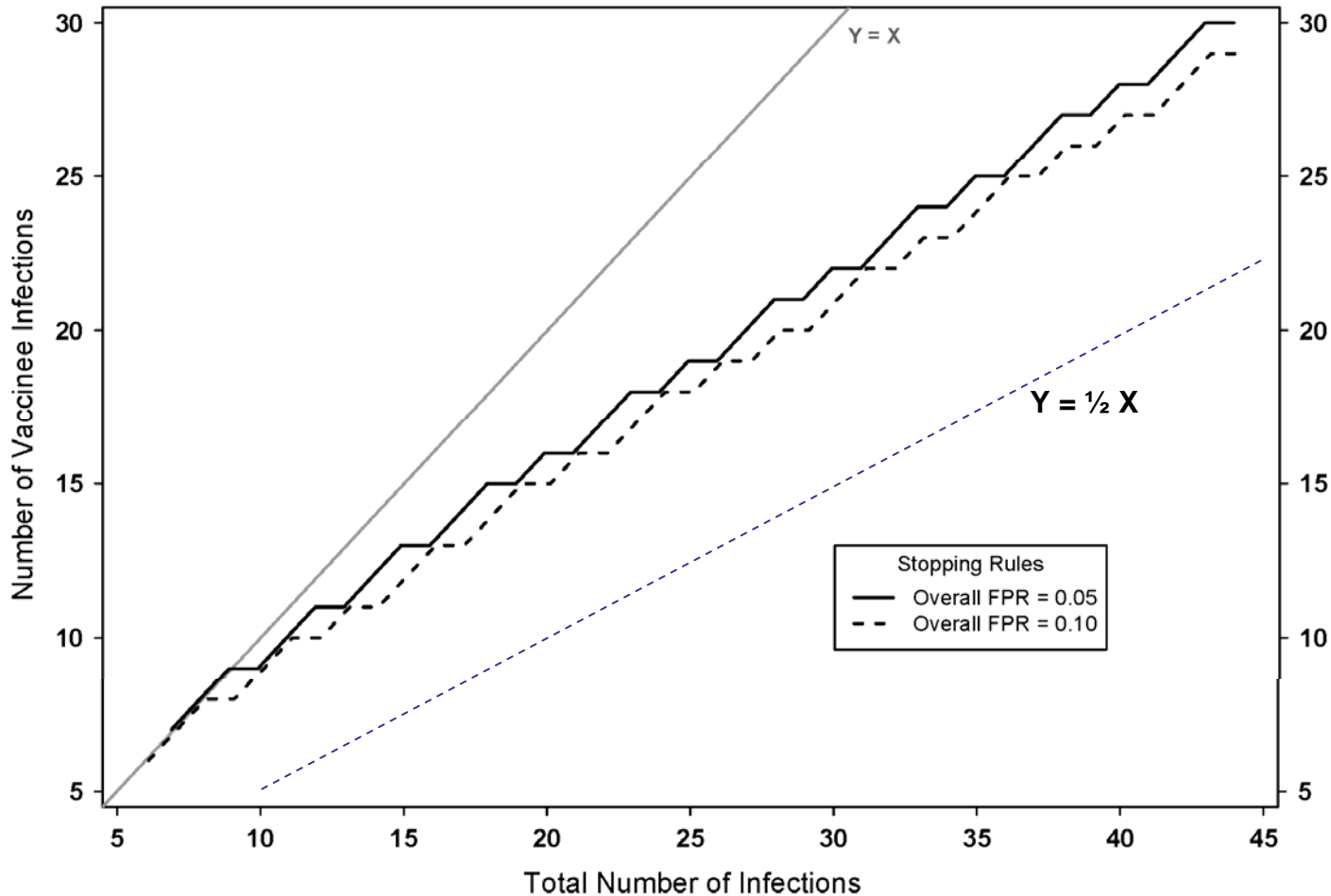
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- Monitor for higher HIV infection rate in vaccine group
  - Study population defined to ensure equipoise
  - Maximally vigilant “continuous” monitoring (after each confirmed WITT infection event)
  - Safety monitoring strategy developed for (Heyse et al, 2008) and used effectively in rotovirus vaccine efficacy trial
- Formally test for elevated HIV infection relative risk RR (vaccine group relative to placebo) after each observed infection
  - Harm indicated if  $H_0: RR \leq 1$  is rejected in favor of  $H_1: RR > 1$
  - Control overall Type I error of tests for harm to no more than 0.05 (or 0.10) 1-sided
  - Use same statistical criterion for each test performed; do not use very conservative criteria for early tests as in sequential testing for efficacy
    - Overall “0.05” rule: Stop if individual 1-sided test\* gives  $p \leq 0.015$
    - Overall “0.10” rule: Stop if individual 1-sided test\* gives  $p \leq 0.031$

\* Exact binomial test with unadjusted/nominal p-value

# Stopping Boundaries for the Overall “0.05” and “0.10” Rules

Figure 1: Stopping Bounds for Two False Positive Rates



Examples of Vx:Plc Infection splits that just hit stopping boundary

0.05 rule:  
7:0, 9:1, 16:4, 22:8

0.10 rule:  
6:0, 9:1, 15:5, 21:9

# “Power” of the “0.05” and “0.10” Rules for Correctly Stopping Early

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Probability of stopping early (before the 45th WITT infection) for a range of possible true relative risks (RRs) of HIV infection (vaccine/placebo)

Stopping Rule (overall FP rate)	RR=1	RR=1.5	RR=2.0	RR=2.5	RR=3.0
0.05 rule	.049	.29	.61	.82	.93
0.10 rule	.093	.42	.73	.90	.96

# Reliability/Interpretability

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- **STOC design “unusual” for a randomized controlled trial as no aspect of primary analysis is based on entire randomized population**
  - Comparison of highly-selected small subset of randomized participants who become infected can be subject to bias
  - Even if bias is due to measured confounders, small numbers of subjects with evaluable endpoints may thwart attempts for post-hoc statistical adjustment
- **Sensitivity analyses can help assess extent to which observed effect may be due to selection bias**
  - Depends on range of plausible values for RR
  - Estimate of RR from STOC trial highly variable
- **Significant effects on VL in STOC trial may not be reliably attributed to direct biological effect on VL**

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