A Different Diff-in-Diffs?
Fresh Takes on a Familiar Estimator

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Slides & Example Code `http://people.bu.edu/tsimcoe/data`

Diff-in-Diffs Setup

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- Three intuitive estimators
  1. Treatment vs. Control: $E[Y_A|T = 1] - E[Y_A|T = 0]$
  2. Before vs. After: $E[Y_A|T = 1] - E[Y_B|T = 1]$
  3. Diff-in-Diffs: $E[Y_A - Y_B|T = 1] - E[Y_A - Y_B|T = 0]$

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  2. Before vs. After: $E[Y_A | T = 1] - E[Y_B | T = 1]$
  3. Diff-in-Diffs: $E[Y_A - Y_B | T = 1] - E[Y_A - Y_B | T = 0]$
- DD + parallel trends assumption $\Rightarrow$ causal estimate

$$E[\underbrace{Y_A^1 - Y_B | T = 0}_{\text{Observed}}] = E[\underbrace{Y_A^0 - Y_B | T = 1}_{\text{Counterfactual}}]$$

John Snow (1854)

Cholera Cases per 10K Households

	1849	1854	After - Before
Lambeth (T=1)	85	19	-66
Southwark & Vauxhall	135	147	12
Treated - Control	-50	-128	-78

Card & Kruger (1994)

TABLE 5.2.1
Average employment in fast food restaurants before and after the New Jersey minimum wage increase

Variable	PA (i)	NJ (ii)	Difference, NJ – PA (iii)
1. FTE employment before, all available observations	23.33 (1.35)	20.44 (.51)	−2.89 (1.44)
2. FTE employment after, all available observations	21.17 (.94)	21.03 (.52)	−.14 (1.07)
3. Change in mean FTE employment	−2.16 (1.25)	.59 (.54)	2.76 (1.36)

Notes: Adapted from Card and Krueger (1994), table 3.

Popular Variations on Diff-in-Diffs

- DD as Linear Regression
  - $E[Y_{it}] = \alpha_0 + \alpha_1 Treated_i + \lambda Post_t + \beta Treated_i * Post_t$

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  - $E[Y_{it}] = \alpha_i + \lambda_t + \beta PostTreated_{it}$
- Event Study Specification
  - $E[Y_{it}] = \alpha_i + \lambda_t + \sum_k \beta_k 1[t - TreatmentYear_i = k]$
  - Plot dynamic treatment effects β_k (normalizing $\beta_{-1} = 0$)
  - "Pre trends" falsification test: $H_0 : \beta_k = 0$ for $k \leq -1$

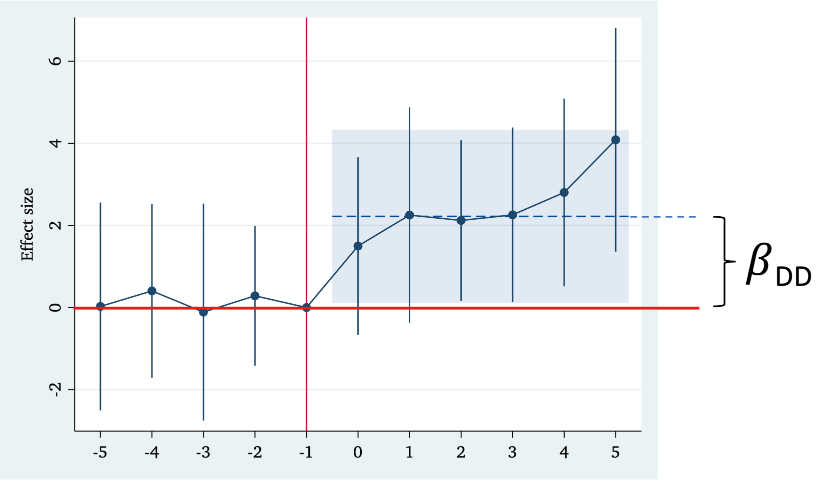
So What is the Problem?

Recent econometrics literature has emphasized two issues with DD:

1. Violations of parallel trends assumption
2. Identification under staggered adoption

Takeaway: If you are willing to assume parallel trends, and do not have staggered adoption, previous DD estimators work fine!

The Power of Parallel Trends



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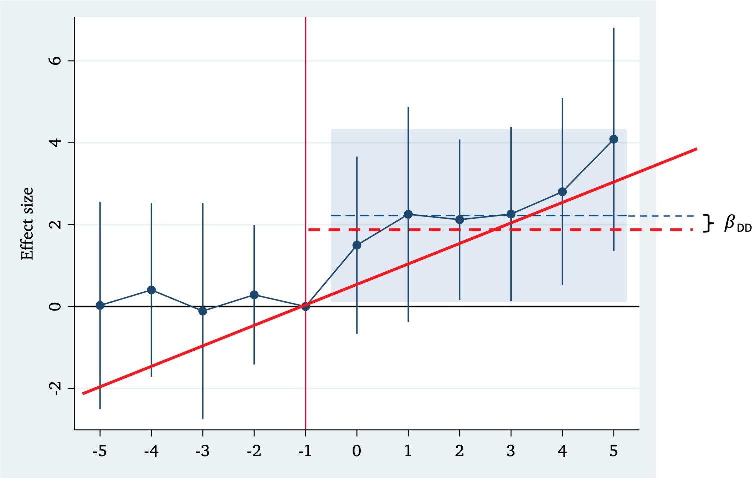

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- Enlarge the standard errors?
  - "An Honest Approach to $\parallel$ Trends" (Rambachan & Roth, WP)
  - User specified close-to-parallel trends $\Rightarrow$ set identification

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- Keep in mind, parallel trends is a maintained assumption
- Enlarge the standard errors?
  - "An Honest Approach to $\parallel$ Trends" (Rambachan & Roth, WP)
  - User specified close-to-parallel trends $\Rightarrow$ set identification
- Relax our rhetoric
  - Failing to reject pre-trends $\neq$ "Proving" parallel trends!!!
  - Authors: Don't over-sell your noisy falsification tests
  - Referees: Don't be Manichean about pre-trend testing

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  2. $E[Y_{it}] = \alpha_i + \lambda_t + \beta PostTreated_{it}$
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- but can estimate (2), even without a control group!!

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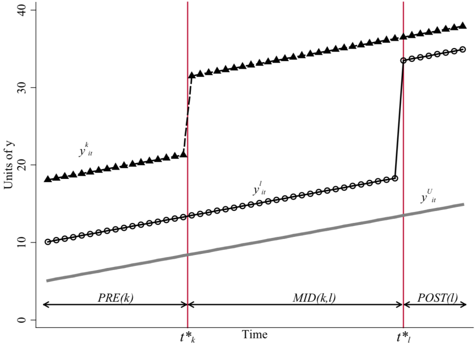
Key Point: Differences in treatment timing produce new comparisons, and therefore new sources of identification.

Goodman-Bacon (2019)

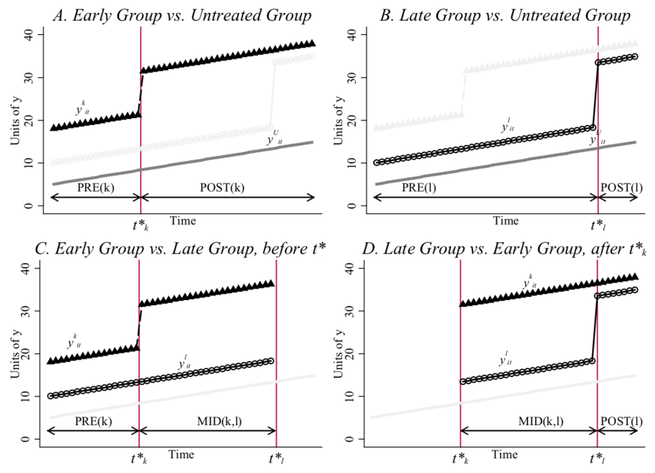
- TWFE β is a weighted average of 2 x 2 DD's
- Three Group Example: Early, Late & Never Treated

Figure 1. Difference-in-Differences with Variation in Treatment Timing: Three Groups



Components of the GB Decomposition

Figure 2. The Four Simple (2x2) Difference-in-Differences Estimates from the Three Group Case



GB Decomposition Theorem

Theorem 1. Difference-in-Differences Decomposition Theorem
Assume that the data contain $k = 1,\dots,K$ groups of units ordered by the time when they receive a binary treatment, $t_k^* \in (1,T]$. There may be one group, U, that never receives treatment. The OLS estimate, $\widehat{\beta}^{DD}$, in a two-way fixed-effects regression (2) is a weighted average of all possible two-by-two DD estimators.

$$\widehat{\beta}^{DD} = \sum_{k \neq U} s_{kU}\, \widehat{\beta}_{kU}^{2x2} + \sum_{k \neq U} \sum_{\ell > k} \left[s_{k\ell}^{k}\, \widehat{\beta}_{k\ell}^{2x2,k} + s_{k\ell}^{\ell}\, \widehat{\beta}_{k\ell}^{2x2,\ell} \right]. \qquad (10a)$$

Where the 2x2 DD estimators are:

$$\widehat{\beta}_{kU}^{2x2} \equiv \left(\overline{y}_k^{POST(k)} - \overline{y}_k^{PRE(k)} \right) - \left(\overline{y}_U^{POST(j)} - \overline{y}_U^{PRE(j)} \right), \qquad (10b)$$

$$\widehat{\beta}_{k\ell}^{2x2,k} \equiv \left(\overline{y}_k^{MID(k,\ell)} - \overline{y}_k^{PRE(k)} \right) - \left(\overline{y}_\ell^{MID(k,\ell)} - \overline{y}_\ell^{PRE(k)} \right), \qquad (10c)$$

$$\widehat{\beta}_{k\ell}^{2x2,\ell} \equiv \left(\overline{y}_\ell^{POST(\ell)} - \overline{y}_\ell^{MID(k,\ell)} \right) - \left(\overline{y}_k^{POST(\ell)} - \overline{y}_k^{MID(k,\ell)} \right). \qquad (10d)$$

Weights, s_k, reflect sample size and treatment-variance for each "timing group" k

Goodman-Bacon Takeaways

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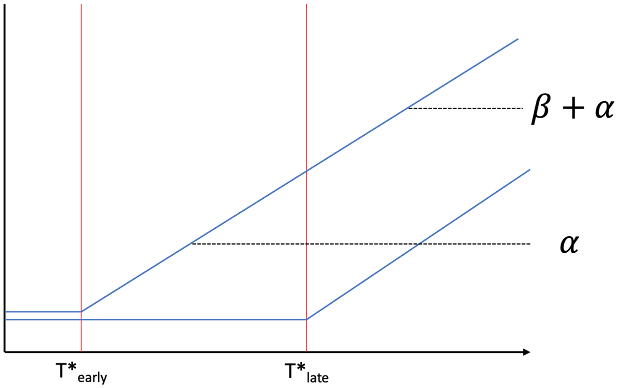
Goodman-Bacon Takeaways

1. GB is not a method or solution to biased TWFE estimates
    - The Stata/R “bacondecomp” module is a diagnostic tool
2. GB highlights key problem with Staggered DD
    - TWFE uses early-treated as control group for late-treated!
    - ...which is also why we can estimate β without controls
3. Points towards excluding “forbidden comparisons”
    - Callaway and Sant’Anna (aggregation)
    - Borusyak, Jaravel and Spiess (imputation)
    - Matching with pseudo-treatment

Good News! Staggered DD Estimates are (Probably) Conservative



$DD^{late} =$

$$(\alpha - 0) - (\beta + \alpha - \alpha) = \alpha - \beta < \alpha$$

Callaway and Sant'Anna (JOE 2021)

- Let G_i be treatment cohort of unit i ($G_i = \infty$ for Controls)
- C&S define group-time average treatment effects

$$DD(G,T) = E[Y_T - Y_{T_0} | T_0 < G_i < T] - E[Y_T - Y_{T_0} | T < G_i]$$

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- Construct $\widehat{\beta^{DD}}$ as weighted average of $DD(G,T)$'s
  - Basic idea: aggregation of "clean" 2 x 2's
  - Researcher chooses weights $\Rightarrow$ many possibilities
  - Loop over (G, T_0, T) $\Rightarrow$ slow on large panels
  - Paper discusses issues with inference (SEs)
- Wooldridge (2021) shows how to recover $DD(G,T)$'s from OLS with many interacted fixed effects

Borusyak, Jaravel and Spiess (2021, WP)

1. Estimate TWFE model using untreated observations
   - $Y_{it}^0 = \alpha_i + \lambda_t + X_{it}\theta$

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   - $DD_{it} = Y_{it} - \widehat{Y_{it}^0}$

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   - $DD_{it} = Y_{it} - \widehat{Y_{it}^0}$
3. Construct $\widehat{\beta^{DD}}$ as weighted average of DD_{it}'s
   - Weighting choices $\Rightarrow$ researcher DOF
   - Faster than C&S, but need to store α_i

Stacked Difference-in-Differences

- Deshpande & Li (2019), Cengiz et al (2019)

1. Choose a time-window (t_{pre}, t_{post})
2. For each treatment cohort G create a new dataset containing
   - Periods $G - t_{pre}$ to $G + t_{post}$ for treated cohort
   - All units not treated over same time period
3. Stack datasets (indexed by c) into one large panel
4. Estimate $E[Y_{cgpit}] = \alpha_{cg} + \lambda_{cp} + \beta PostTreated_{it}$
   - where α_{cg}, λ_{cp} are dataset-by-group and -period effects

DD as Matching

1. For each treated i, pick a similar control $\Rightarrow$ 1:1 match
   - Coarsened matching (CEM) or propensity score
2. Estimate DD or TWFE model. Done.

- Exact matching yields $N_{Treated}$ "clean" DD's

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- Should you match on Y_0 and/or pre-trends?
- It depends. Chabe-Ferret (JOE 2015) $\Rightarrow$ FE and matching are not complementary

I have a DD Paper. What Should I do?

- Simultaneous Adoption $\Rightarrow$ "old" DD or TWFE
  - Can add matching / weighting to address selection
- Sequential Adoption
  - Large N_i / many controls $\Rightarrow$ exact matching
  - Large N_i / mostly treated $\Rightarrow$ BJS
  - Small N_i / mostly treated $\Rightarrow$ C&S or Stacked DD

Leading use case for new estimators: Impact of policy adopted by most states at different times (e.g. smoking bans)

Rysman & Simcoe (MS 2008)

What is the impact of technology standardization on "patent value"?

- Sample of patents "declared essential" ($T_i = 1$) to SSOs
- Controls ($T_i = 0$) matched on vintage and tech-class
- Panel Data: $i =$ Patent, $t =$ Year
  - Outcome $Y_{it} =$ Citation count
  - Disclosure years $\Rightarrow$ staggered treatment
- Data & Code: `http://people.bu.edu/tsimcoe/data`

Baseline 2 x 2 DD

. reg cites PostTreat TreatGroup PostPeriod, cluster(pat)

```
Linear regression                               Number of obs     =     67,367
                                                F(3, 6139)        =      67.96
                                                Prob > F          =     0.0000
                                                R-squared         =     0.0222
                                                Root MSE          =     5.4855

                             (Std. Err. adjusted for 6,140 clusters in pat_id)
```

cites	Coef.	Robust Std. Err.	t	P>|t|	[95% Conf.	Interval]
PostTreat	.0938327	.1670108	0.56	0.574	-.2335671	.4212324
TreatGroup	1.578988	.1493449	10.57	0.000	1.286219	1.871756
PostPeriod	.2001718	.0985406	2.03	0.042	.0069977	.393346
_cons	1.717133	.0753659	22.78	0.000	1.569389	1.864876

Baseline DD with Year Effects

. reg cites PostTreat TreatGroup i.year, cluster(pat)

```
Linear regression                               Number of obs     =     67,367
                                                F(32, 6139)       =      29.68
                                                Prob > F          =     0.0000
                                                R-squared         =     0.0400
                                                Root MSE          =     5.4365

                            (Std. Err. adjusted for 6,140 clusters in pat_id)
------------------------------------------------------------------------------
             |               Robust
       cites |      Coef.   Std. Err.      t    P>|t|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
   PostTreat |   .5879134   .1546432     3.80   0.000     .2847585    .8910683
  TreatGroup |   1.302752   .1480573     8.80   0.000     1.012508    1.592997
             |
        year |
       1981  |  -.1254601   .1565342    -0.80   0.423    -.4323219    .1814017
       1982  |  -.1384076   .1424279    -0.97   0.331    -.4176162     .140801
```

Two-Way Fixed Effects

. xtreg cites PostTreat i.year, fe i(pat) robust

```
Fixed-effects (within) regression               Number of obs     =     67,367
Group variable: pat_id                          Number of groups  =      6,140

R-sq:                                           Obs per group:
     within  = 0.0270                                         min =          1
     between = 0.0222                                         avg =       11.0
     overall = 0.0228                                         max =         26

                                                F(31,6139)        =      19.49
corr(u_i, Xb)  = 0.0233                         Prob > F          =     0.0000

                             (Std. Err. adjusted for 6,140 clusters in pat_id)
```

cites	Coef.	Robust Std. Err.	t	P>|t|	[95% Conf.	Interval]
PostTreat	.9803645	.1001289	9.79	0.000	.7840767	1.176652
year						
1981	.1006381	.3203953	0.31	0.753	-.5274491	.7287252
1982	-.0227574	.4543496	-0.05	0.960	-.9134419	.8679272

Matching + DD

. xtreg cites PostTreat PostPeriod i.year, fe i(pat) robust

```
Fixed-effects (within) regression               Number of obs     =     67,367
Group variable: pat_id                          Number of groups  =      6,140

R-sq:                                           Obs per group:
     within  = 0.0275                                         min =          1
     between = 0.0258                                         avg =       11.0
     overall = 0.0220                                         max =         26

                                                F(32,6139)        =      19.82
corr(u_i, Xb)  = 0.0250                         Prob > F          =     0.0000

                                (Std. Err. adjusted for 6,140 clusters in pat_id)
```

cites	Coef.	Robust Std. Err.	t	P>|t|	[95% Conf.	Interval]
PostTreat	.7555577	.1122421	6.73	0.000	.5355238	.9755917
PostPeriod	.3430708	.0834113	4.11	0.000	.1795553	.5065863
year						
1981	.0982575	.3209077	0.31	0.759	-.530834	.727349
1982	-.0426922	.4568111	-0.09	0.926	-.9382021	.8528178

Borusyak, Jaravel & Spiess (Imputation)

. net install did_imputation . replace TreatYr = . if (TreatGroup==0)
. did_imputation cites pat_id year TreatYr, autosample

```
Warning: part of the sample was dropped for the following coefficients because

                                              Number of obs     =     64,757
```

cites	Coef.	Std. Err.	z	P>|z|	[95% Conf.	Interval]
tau	.7901201	.1123972	7.03	0.000	.5698256	1.010415

Callaway & Sant'Anna (GT Effects)

. net install csdid

. replace TreatYr = 0 if (TreatGroup==0)

. csdid cites, ivar(pat_id) time(year) gvar(TreatYr)

. estat simple

```
Units always treated found. These will be excluded
Panel is not balanced
Will use observations with Pair balanced (observed at t0 and t1)
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```

References & Extra Resources

References

- Borusyak, K., X. Jaravel & J. Spiess (2021) "Revisiting Event Study Designs: Robust and Efficient Estimation" *Working Paper*.
- Callaway, B. & P. Sant'Anna (2021) "Difference-in-Differences with multiple time periods" *Journal of Econometrics*, 225(2): 200–230.
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- Deshpande, M. & Y. Li (2019) "Who is screened out? Application costs and the targeting of disability programs." *AEJ: Economic Policy*, 11(4): 213–248.
- Goodman-Bacon, A. (2021) "Difference-in-differences with variation in treatment timing" *Journal of Econometrics*, 225(2): 254–277.
- Rambachan A. & J. Roth (2021) "An Honest Approach to Parallel Trends" *Working Paper*.
- Wooldridge, J. (2021) "Two-Way Effects, the Two-Way Mundlak Regression, and Difference-in-Differences Estimators" *Working Paper*.

Resources

- Stata/R Packages: bacondecomp, csdid, did_imputation
- `https://taylorjwright.github.io/did-reading-group/`