So far, we have essentially assumed that we only have a single cross-section of outcome measurements for each unit. Here, we examine different approaches to the causal inference with repeated measures. The situations we consider include both repeated cross-section data as well as panel data.

1 Causal Quantities of Interest

We start with the simple panel data of two waves and assume that the outcome variables are measured before and after the administration of the treatment. We use $Y_{0i}$ and $Y_{1i}$ to denote the observed outcomes for time 0 (pre-treatment) and time 1 (post-treatment). Finally, let $G_i$ be the binary indicator variable which indicates the group membership of unit $i$ ($G_i = 1$ for the treatment group and $G_i = 0$ for the control group). Thus, the binary treatment variable can be defined as $Z_{ti} = tG_i$ for $t = 0, 1$. That is, the units in the treatment group ($G_i = 1$) receive the treatment but only at time 1, while the units in both groups do not receive the treatment at time 0. In particular, we have $Z_{1i} = G_i$.

The set-up implies that the following potential outcomes exist; i.e., $(Y_{0i}(0), Y_{0i}(1), Y_{1i}(0), Y_{1i}(1))$. Now, we can express the observed outcomes as a function of potential outcomes $Y_{0i} = Y_{0i}(0)$ and $Y_{1i} = G_iY_{1i}(1) + (1 - G_i)Y_{1i}(0)$ for all $i$. Or more generally, for $t = 0, 1$, we have,

$$Y_{ti} = Z_{ti}Y_{1i}(1) + (1 - Z_{ti})Y_{1i}(0).$$

Finally, we use $X_{0i}$ to denote the pre-treatment covariates or the covariates that do not change over time. We generally do not consider the covariates measured at time 1, i.e., $X_{1i}$, because they can be affected by the treatment and hence may induce post-treatment bias. Without loss of
generality, we write \( X_i = X_{0i} \) (though adding completely exogenous \( X_{1i} \) to the set of covariates will not change the results that follow).

In general, there are two quantities of interest examined in the literature; the average treatment effect and the average treatment effect for the treated.

\[
\tau_{ATE} \equiv \mathbb{E}(Y_{1i}(1) - Y_{1i}(0)),
\]
\[
\tau_{ATT} \equiv \mathbb{E}(Y_{1i}(1) - Y_{1i}(0) \mid Z_{1i} = 1).
\]

If conditioned on \( X_i \), we may estimate the conditional versions of these quantities,

\[
\tau_{CATE} \equiv \frac{1}{n} \sum_{i=1}^{n} \mathbb{E}(Y_{1i}(1) - Y_{1i}(0) \mid X_i, Y_{0i}),
\]
\[
\tau_{CATT} \equiv \frac{1}{\sum_{i=1}^{n} Z_{1i} \sum_{i \in \{i: Z_{1i} = 1\}}} \sum_{i \in \{i: Z_{1i} = 1\}} \mathbb{E}(Y_{1i}(1) - Y_{1i}(0) \mid Z_{1i} = 1, X_i, Y_{0i}).
\]

2 Approaches based on the Exogeneity Assumption

A natural approach to incorporate the additional information provided by \( Y_{0i} \) is to maintain the exogeneity assumption conditioning on this variable. In fact, one may assume the unconfoundedness given both \( X_i \) and \( Y_{0i} \),

\[
(Y_{1i}(1), Y_{1i}(0)) \perp \!
\perp Z_{1i} \mid (X_i, Y_{0i}).
\]

Under this assumption together with the common support assumption, i.e., \( 0 < \text{Pr}(Z_{1i} = 1 \mid X_i = x, Y_{0i} = y) < 1 \) for all \( x \) and \( y \), all the methods (e.g., matching and regression adjustments) we studied in the previous set of lecture notes are applicable. Indeed, in many cases, conditioning on \( Y_{0i} \) may be justified since it may explain a large part of heterogeneity in the potential outcomes.

In particular, consider the situation where the ATT or CATT is the quantity of interest. In that situation, \( \mathbb{E}(Y_{1i}(1) \mid Z_{1i} = 1) \) (for ATT) and \( \mathbb{E}(Y_{1i}(1) \mid Z_{1i} = 1, X_i, Y_{0i}) \) (for CATT) are identified from the observed data, and thus the identification of the ATT and CATT hinges on the identification of \( \mathbb{E}(Y_{1i}(0) \mid Z_{1i} = 1) \) and \( \mathbb{E}(Y_{1i}(0) \mid Z_{1i} = 1, X_i, Y_{0i}) \), respectively. In this case, it may be critically important to match on \( Y_{0i} = Y_{0i}(0) \) as well as \( X_i \) in order to assume \( \mathbb{E}(Y_{1i}(0) \mid Z_{1i} = 1, X_i, Y_{0i}) \approx \mathbb{E}(Y_{1i} \mid Z_{1i} = 0, X_i, Y_{0i}) \). Note that here we only assume

\[
Y_{1i}(0) \perp \!
\perp Z_{1i} \mid (X_i, Y_{0i}).
\]

For example, the literature on the evaluation of job training program shows that conditioning on lagged wages over the past several periods is important for obtaining reasonable estimates of the causal effects of such programs. Similarly, it might be the case that conditioning on voting history may be important to evaluate the causal effects of get-out-the-vote and other treatment variables of interest on turnout.
In the context of linear regression models, this exogeneity assumption (conditional on the pre-treatment outcome and covariates) is related to the familiar lagged dependent variable model:

\[ Y_{1i}(Z_{1i}) = \alpha + \beta Z_{1i} + \gamma^\top X_i + \delta Y_{0i} + \epsilon_i, \] (8)

where \( \mathbb{E}(\epsilon_i \mid Z_{1i}, X_i, Y_{0i}) = 0 \) is often assumed. The model can be generalized slightly by allowing different error terms for the treatment and control groups. Note that the standard assumption is stronger than \( \mathbb{E}(\epsilon_i \mid Z_{1i}, X_i, Y_{0i}) = \mathbb{E}(\epsilon_i \mid X_i, Y_{0i}) \) which is implied by the unconfoundedness assumption. Under this weaker assumption, one can estimate \( \beta \) without bias if \( \mathbb{E}(\epsilon_i \mid X_i, Y_{0i}) \) is linear in \((X_i, Y_{0i})\). That is, the correlation between \( \epsilon_i \) and \( X_i \) as well as the correlation between \( \epsilon_i \) and \( Y_{0i} \) is allowed.

A major criticism of this approach is that the unconfoundedness assumption (even after conditioning on lagged dependent variable) may be violated if the treatment group differs inherently from the control group. If this is the case, it is difficult to make inferences about the counterfactual outcome \( Y_{1i}(0) \) for the treatment group \( Z_{1i} = 1 \) using the observed outcome \( Y_{1i} = Y_{1i}(0) \) from the control units \( Z_{1i} = 0 \) even if these control units have the same value of the pre-treatment outcome \( Y_{0i} \) as the treated units.

An alternative would be to use the pre-treatment outcome \( Y_{0i} = Y_{0i}(0) \) from the same treated group to make inferences about the counterfactual (post-treatment) counterfactual outcome for the treated group, i.e.,

\[ \mathbb{E}(Y_{1i}(0) \mid Z_{1i} = 1, X_i) = \mathbb{E}(Y_{0i}(0) \mid Z_{1i} = 1, X_i). \] (9)

However, the problem of this approach is that changes in the outcome variable between the pre and post-treatment periods may be attributed to something other than the administration of the treatment.

3 Change-Scores (Difference-in-Differences) Methods

As the third alternative to this problem, some combine the previously mentioned two alternative approaches into a single one. The idea is to make adjustments for time trend as well as unobserved group differences. This method is called the change-scores (or gain-scores) method in psychometrics (Lord, 1956, 1963) and the difference-in-differences method in econometrics (Meyer, 1995). The idea is to base the inference on the following estimand,

\[ \tau^* \equiv \{ \mathbb{E}(Y_{1i} \mid G_i = 1) - \mathbb{E}(Y_{0i} \mid G_i = 1) \} - \{ \mathbb{E}(Y_{1i} \mid G_i = 0) - \mathbb{E}(Y_{0i} \mid G_i = 0) \}. \] (10)

Under what conditions does this estimand correspond to the causal quantities of interest? In addition to this question, we also compare this method with the approach based on the exogeneity assumption described above. Allison (1990) provides an interesting discussion of this topic by comparing the two approaches in the context of the linear regression model. Here, we make a more
general comparison by first investigating the nonparametric identification before considering the common justification of the change-scores (difference-in-differences) method based on the linear regression model.

### 3.1 Nonparametric Identification

The key identifying assumption of the average causal effects under this model is that in the absence of the treatment the mean outcomes for the two groups have followed parallel path (within the strata defined by a vector of the pre-treatment covariates $X_i$ if it is available), formalizing the usual intuition for this method in terms of potential outcomes,

$$E(Y_{1i}(0) - Y_{0i}(0) \mid G_i = 1, X_i = x) = E(Y_{1i}(0) - Y_{0i}(0) \mid G_i = 0, X_i = x),$$

(11)

Under this assumption, $\tau^*$ equals the ATT (e.g., Heckman et al., 1997) and it can be estimated by replacing each term in equation 10 by its sample counterpart (i.e., the sample average given group membership and time period). The variance can be estimated by adding the variance for each of these sample averages. If the estimand is the ATE rather than the ATT, then we also need to estimate the ATE for the control group. In that case, one often assume the homogeneity of average treatment effect for the two groups.

However, $X_i$ can be high dimensional and so in practice subclassifying on $X_i$ and conducting an analysis within each subclass might be difficult as implied by the assumption in equation 11 may not be feasible. One way forward is to use the propensity score. Abadie (2005) shows that the ATT can be identified using the propensity score,

$$E(Y_{1i}(1) - Y_{1i}(0) \mid G_i = 1) = E \left( \frac{Y_{1i} - Y_{0i}}{\pi(X_i)} \cdot \frac{G_i - \pi(X_i)}{1 - \pi(X_i)} \right),$$

(12)

where $\pi(X_i) \equiv \Pr(Z_{1i} = 1 \mid X_i) = \Pr(G_i = 1 \mid X_i)$ is the propensity score (we assume $0 < \pi(X_i) < 1$ as usual). This expression makes sense because it parallels with the Horvitz-Thompson estimator we reviewed earlier in the course. This unbiased estimator down-weights the control observations that are not similar to the treated units. The equation suggests a simple two-step procedure. First, estimate the propensity score and then compute the estimate of the ATT by the weighting formula.

One problem of the assumption in equation 11 is that it is somewhat arbitrary because it heavily relies on the scale of the outcome variable. For example, suppose that equation 11 holds when the outcome of interest is raw income. Now consider using the log transformed income as the outcome variable. With a nonlinear transformation, equation 11 may no longer hold. We should also note that the assumption is not directly testable because for the treatment group, i.e., units with $G_i = 1$, we do not observe $Y_{1i}(0)$.

Thus, in practice, it is better to think that the following unconfoundedness assumption is required for the change-score or difference-in-difference method.

$$Y_{1i}(0) \perp \perp Z_{1i} \mid X_i = x,$$

(13)
or equivalently \( Y_{ti}(0) \perp G_i \mid X_i = x \) for \( t = 0, 1 \). Clearly, equation 13 implies equation 11. Compare this assumption with the exogeneity assumption given in equation 7. For the change-score (or the difference-in-difference) methods, we assume the unconfoundedness without conditioning on the lagged dependent variable. In contrast, for the methods based on the exogeneity assumption, we assume the unconfoundedness after conditioning on the lagged dependent variable. Although these two assumptions are not nested within one another, in many observational studies it may be difficult to justify the unconfoundedness without conditioning on the lagged dependent variable which may be a confounder by itself and/or may be related with other observed and unobserved confounders. But, if this is the case, what is the justification for this type of analysis?

### 3.2 Justification based on the Linear Regression Model

The most frequently used justification for the change-score (or difference-in-differences) is based on the linear additive regression model. Here, we first consider the case where there is no pre-treatment covariate. The model is given by,

\[
\begin{align*}
Y_{0i}(0) &= \alpha_i + \epsilon_{0i}, \\
Y_{1i}(0) &= \alpha_i + \gamma + \epsilon_{1i}, \\
Y_{1i}(1) &= \alpha_i + \beta + \gamma + \epsilon_{1i}
\end{align*}
\]

or more generally, it can be written as,

\[
Y_{ti}(z) = \alpha_i + \beta z + \gamma t + \epsilon_{ti}
\]

where \( \alpha_i \) is the unobserved individual “fixed effect” that does not vary over time, \( \gamma \) is the time effect, and \( \epsilon_{ti} \) represents the unobserved individual time varying characteristics at time \( t \) with normalized means, i.e., \( \mathbb{E}(\epsilon_{ti}) = 0 \).

Under this model, the key identification assumption given in equation 11 can be written as,

\[
\mathbb{E}(Y_{1i}(0) - Y_{0i}(0) \mid G_i = g) = \gamma.
\]

Equivalently, you can express the assumption in terms of \( \epsilon_{ti} \),

\[
\mathbb{E}(\epsilon_{1i} - \epsilon_{0i} \mid G_i) = 0.
\]

If the assumption is expressed in terms of independence (rather than mean independence), then it is given by \( \epsilon_{ti} \perp G_i \) or \( \epsilon_{ti} \perp Z_{1i} \) for \( t = 0, 1 \). Note that since \( Y_{ti}(z) \) is a function of \( \epsilon_{ti} \), under this model all potential outcomes are assumed to be independent of the treatment assignment.

However, the set-up allows for both the treatment assignment \( Z_{1i} \) and the time-varying shocks \( \epsilon_{ti} \) to depend on individual-level fixed effect \( \alpha_i \), and this is used as a main justification for the method. In essence, the identification under this model critically hinges on the assumption that at each time period there is an individual-level “shock” that is independent of the treatment assignment. In contrast, the approach based on the exogeneity assumption assumes no omitted variable bias by conditioning on the observed lagged outcome.
Under the aforementioned assumptions, it is easy to show that \( \beta \) equals the average treatment effect, i.e., \( \beta = \tau \). To estimate \( \beta \) from the observed data, we subtract the pre-treatment outcome of each unit from the post-treatment outcome of the same unit. This gives,

\[
Y_{1i} - Y_{0i} = G_i \beta + \gamma + \epsilon_{1i} - \epsilon_{0i}.
\]  

Since by assumption \( \mathbb{E}(\epsilon_{1i} - \epsilon_{0i} | G_i) = 0 \), regressing the difference between pre-treatment and post-treatment outcomes on the group-membership will give you an unbiased estimate of \( \beta \), i.e., an unbiased estimate of the average treatment effect. Recalling that the simple linear regression estimator corresponds to the difference-in-means estimator, \( \beta \) can be estimated via the difference-in-differences whose population counterpart is given by \( \tau^* \).

Finally, when estimating the variance from the least squares regression, it is important to estimate the variance separately for each group so that heterogeneity is allowed; i.e., the variance can be different across groups. This is similar to the case where the Neyman variance is estimated for the treatment and control groups separately.

In practice, researchers often adjust for additional pre-treatment variables via the following model,

\[
Y_{ti}(z) = \alpha_i + \beta z + \gamma t + \delta_t^\top X_i + \epsilon_{ti}.
\]  

Under this model, we assume

\[
\mathbb{E}(Y_{1i}(0) - Y_{0i}(0) | G_i = g, X_i = x) = \gamma + (\delta_1 - \delta_0)^\top x,
\]

for \( g = 0, 1 \) or equivalently,

\[
\mathbb{E}(\epsilon_{1i} - \epsilon_{0i} | G_i = 1, X_i = x) = \mathbb{E}(\epsilon_{1i} - \epsilon_{0i} | G_i = 0, X_i = x)
\]

This model allows for the observed individual characteristics to have time-varying linear effects on the outcome variable. However, in order to allow for the existence of such effects, one must essentially assume (in addition to the linear functional form assumption which is assumed throughout) that the time-varying individual unobserved effects are independent of the treatment assignment given the observed pre-treatment variables, i.e., \( \epsilon_{ti} \perp \perp G_i | X_i \). Similar to the case of no covariates, this assumption implies \( Y_{ti}(0) \perp Z_{1i} \) although both \( \epsilon_{ti} \) and \( Z_{1i} \) can depend on the unobserved fixed effect \( \alpha_i \).

Thus, this model with covariates may be viewed as slightly more general although the resulting inference strongly depends on the linear specification. Finally, the difference-in-difference formulation gives,

\[
Y_{1i} - Y_{0i} = G_i \beta + \gamma + (\delta_1 - \delta_0)^\top X_i + \epsilon_{1i} - \epsilon_{0i},
\]

which can be estimated consistently via linear least squares under the aforementioned assumptions.
3.3 A Non-linear Model

One important limitation of the change-scores or difference-in-differences methods is that their justification is often based on the linear model. In fact, the reason why inference is based on “differences” originates in the linearity assumption. Clearly, if the functional form assumption is invalid, the inference will be incorrect. This limitation is important especially when one has covariates to control for because the misspecification in these covariates (in equation 20) can result in invalid causal inferences.

Athey and Imbens (2006) provide an approach to the identification of nonlinear change-scores (difference-in-differences) models. They consider a following nonlinear model for the outcome in the absence of the treatment,

\[ Y_{it}(0) = h(U_{it}, t), \]  

(24)

where \( U_{it} \) is an unobserved characteristics of unit \( i \) at time \( t \). Athey and Imbens (2006) makes the following assumptions,

- Continuity: \( Y_{it} \) is a continuous variable. (They also examine the discrete case in their paper)
- Monotonicity: \( h(u, t) \) is increasing in \( u \).
- Time-invariant unobserved heterogeneity: \( p(U_{0i} \mid G_i) = p(U_{1i} \mid G_i) \). (Note that this is not the same as \( U_{0i} = U_{1i} \) or \( p(U_{0i}) = p(U_{1i}) \)).

Under these assumptions, Athey and Imbens (2006) show that the distribution function for the counterfactual outcome of the treated units at time \( t = 1 \) is identified.

\[ F_{Y_{1i}(0) \mid G_i=1}(y) = F_{Y_{0i} \mid G_i=1} \left[ F_{Y_{1i} \mid G_i=0}^{-1} \{ F_{Y_{1i} \mid G_i=0}(y) \} \right] \]  

(25)

Since \( F_{Y_{1i}(1) \mid G_i=1}(y) = F_{Y_{1i} \mid G_i=1}(y) \) can be identified from the observed data, the ATT as well as various quantile causal effects for the treated are identified. The ATT is given by,

\[ \tau_{ATT} = \mathbb{E}(Y_{1i} \mid G_i = 1) - \mathbb{E} \left[ F_{Y_{0i} \mid G_i=0}^{-1} \{ F_{Y_{1i} \mid G_i=0}(y) \} \right]. \]  

(26)

Under some regularity conditions, Athey and Imbens (2006) prove that the sample counterpart of the second term in the above equation (based on the empirical distribution function and its inverse) is consistent and asymptotically Normal. The asymptotic variance can also be easily estimated from the observed data.

One important aspect of this identification result is that in non-linear settings the inference is no longer based on “differences.” These models can be called change-in-changes models. This again shows that the standard difference-in-difference method relies heavily on the linearity assumption.

4 More on the Change-Score Methods

Given the basics described above, we briefly discuss some extensions and recent developments.
4.1 Matching or Difference-in-Differences?: A Hybrid Approach

Applied researchers often ask the question of whether they should use the methods based on the exogeneity assumption (such as matching methods) or the change-score (difference-in-differences) methods. However, it is possible to combine both approaches. In particular, the idea of “matching as a nonparametric preprocessing” can work here (Ho et al., 2007). The key is to notice that the two methods become identical if one can match exactly on the pre-treatment outcome variable, \( Y_{0i} = Y_{0i}(0) \). In this case, the assumption for the change-score (or difference-in-differences) method becomes,

\[
E(Y_{1i}(0) \mid G_i = 1, Y_{0i} = y, X_i = x) = E(Y_{1i}(0) \mid G_i = 0, Y_{0i} = y, X_i = x),
\]

which is the exogeneity assumption stated in terms of expectation! In particular, matching exactly on \( Y_{0i} \) eliminates the model dependence between the lagged dependent model and the change-score model. Thus, one approach is to use matching as a preprocessing tool and match on \( X_i \) and \( Y_{0i} \), and then use the change-score model as an analysis tool after the preprocessing. Note that this conclusion applies to both linear and non-linear change-score methods.

Note that in randomized experiments the exogeneity assumption as well as the assumptions for the change-score methods are automatically satisfied. However, using the change score methods leads to a more efficient estimate if there is a positive correlation between the lagged outcome and the observed post-treatment outcome. The advantage is that this efficiency gain is attained without modeling assumptions (e.g., within the Neyman’s randomization framework). On the other hand, if one uses matched-pair design and match exactly on the lagged outcome before the randomization of the treatment, then the efficiency gain can be even greater.

4.2 Repeated Cross-Section Data

The methods developed above can be easily extended to the case of repeated cross-section data where a random sample of each group \( G_i = 0,1 \) is available at each time period. In fact, this may be the case where change-score methods are most useful (recall that in the panel data context the exogeneity assumption conditioning on the lagged dependent variable may be more plausible). The notation changes slightly. We can use \( Y_i(1) \) and \( Y_i(0) \) as potential outcomes for unit \( i \) and let \( T_i \in \{0,1\} \) to denote the time period for which unit \( i \) is sampled. As before, we use \( G_i \) to represent the binary group membership indicator where \( G_i = 1 \) is the treatment group and \( G_i = 0 \) is the control group. Under this setup, \( Z_i \equiv G_i T_i \) is the treatment indicator variable.

With this notation, the estimand is given by,

\[
\tau' = \{E(Y_{i} \mid G_i = 1, T_i = 1) - E(Y_{i} \mid G_i = 1, T_i = 0)\} - \{E(Y_{i} \mid G_i = 0, T_i = 1) - E(Y_{i} \mid G_i = 0, T_i = 0)\},
\]

where each term can be estimated by the corresponding sample average as before. The assumption
in equation (11) can now be written as,

\[ E(Y_i(0) \mid G_i = 1, T_i = 1) - E(Y_i(0) \mid G_i = 1, T_i = 0) = E(Y_i(0) \mid G_i = 0, T_i = 1) - E(Y_i(0) \mid G_i = 0, T_i = 0). \]  

(29)

Under this assumption, it is easy to show that \( \tau' \) equals the ATT.

The linear model that can be used to justify this model is given by,

\[ Y_i(Z_i) = \alpha + \beta Z_i + \gamma G_i + \delta T_i + \epsilon_i, \]  

(30)

which clearly satisfies the above assumption. Since \( Z_i = G_i T_i \), under this model we can use the linear least squares regression to estimate the average treatment effect \( \beta \) using an interaction term without differencing the outcome. There is an important difference between this model and the model for the panel data. In this model, we have a group fixed effect rather than an individual fixed effect. Thus, the panel data model is more general than the repeated cross section model given here.

All the results above can be generalized easily to the case with covariates \( X_i \). For example, \( X_i \) can be added to the conditioning set of equations (28) and (29) as well as the linear regression model in equation (30) as additional covariates.

5 Final Remarks

We consider two classes of methods that can be applied to the situation where repeated observations are available either in the form of panel data or repeated cross section data. The question of which method is appropriate arises quite frequently in applied settings. Here is one example from a social science statistics blog:\[\text{http://www.stat.columbia.edu/~cook/movabletype/archives/2007/02/differenceindif.html}\]

A statistician describes the difference-in-differences estimator as follows:

it’s just a special case of lagged regression where the lag is restricted to have a coefficient of 1. [...] you’re generally limiting your statistical efficiency and range of applicability by using differences rather than the more general regression formulation.

An economist defends the method by saying:

they are not the same. In fact, they are based on very different assumptions. [...] It’s not true that the latter [lagged dependent variable] model is less demanding because it freeing up \( \gamma_1 \) [the coefficient for the lagged dependent variable]. In contrast, the DID assumption implies that you may not want to condition on \( Y_0i \) [lagged dependent variable]; it could be correlated with \( \epsilon_i \) [error term].

Clearly, as shown above, the two methods are based on different unconfoundedness assumptions. The exogeneity assumptions states that the treatment assignment is independent of potential outcomes once you adjust for lagged dependent variable and observed covariates. In contrast, change
score methods assumes that the potential outcome under the control follows the parallel path for the treatment and control groups. It is important to note that the validity of the latter assumption heavily relies on the linearity assumption, and thus without the linearity assumption, the standard change score methods essentially assume that the treatment assignment is independent of potential outcomes given the observed covariates (but without conditioning on the lagged dependent variable). A recent work attempts to address this limitation and propose a non-linear model that is based on a different assumption.

In either case, this choice between the two classes of methods can be eliminated via matching methods. Indeed, matching exactly on the lagged dependent variable along with the observed covariates will make these two approaches equivalent. Thus, matching can be used again as a nonparametric preprocessing procedure to reduce model dependence.

References


