CHAPTER 6: THE EVALUATION OF CONTROL SYSTEMS.

6.1. INTRODUCTION.

Process control engineers are concerned with improving the performance of their plants, both functionally and economically. This often involves performing tests on the existing plant, to determine the benefits of new control strategies, or a different method of operating the plant. The problem with this approach is determining whether a real plant change (often with a small magnitude) has occurred, in the context of background noise and normal plant variations.

A wide range of statistical procedures is available to evaluate plant performance in the presence of disturbances. The use of these techniques in control applications however still seems to be the exception rather than the rule. As a consequence, plant improvement trials are often inappropriately designed, take too long, and lead to either no conclusion where a useful conclusion could have been reached, or the wrong conclusion [30].

In this chapter, some of the available evaluation techniques will be discussed and some other experimental considerations mentioned. An evaluation strategy will be suggested that will be followed in Chapter 7.

6.2. EXPERIMENTAL TECHNIQUES FOR COMPARATIVE EXPERIMENTS.

Comparative experimental techniques can roughly be divided into 3 categories: Replication, blocking and randomisation [31]. Most comparative experimental techniques form part of one of these categories or a combination of two or three.

Replication involves replicating each experiment performed immediately after the initial experiment. If two controllers, A and B, were to be compared using replication, the performance of controller A would be measured once, and the experiment repeated at least once immediately after the initial experiment. Controller B would then be switched on and

the performance measured. The experiment involving controller B would then be repeated immediately afterwards. By using replication, the variations due to experimental error can be determined (from the replicated data) as well as the variations due to the different controllers. As the two sources of variations are separated, a much more accurate representation of the actual controller performance can be obtained.

Blocking is usually performed if it is expected that some disturbance exists which would cause data collected at different times or places to differ significantly. Experimental data collected within a block is compared, and not the data between blocks. For controller evaluation it can for example be expected that two operators would have different capabilities in controlling some plant variable effectively. A shift will then typically be selected as a block, to eliminate the variations due to different operator capabilities. The advantage of blocking is that the variations due to known parameters (e.g. operators, temperature differences, and different procedures on weekdays and weekend days) can be blocked out to improve the accuracy of the final estimate.

Blocking eliminates variances due to known disturbances and replication quantifies the experimental error more accurately. Random disturbances, disturbances with unknown frequencies and long term time trends are however not accounted for in any of the above-mentioned techniques. Randomisation is considered the most important basic principle in good experimentation [31], and ensures that successive errors are random and uncorrelated, thus eliminating the influence of any long term trends and systematic changes. A random testing sequence is easily created, by generating a series of random numbers, and assigning an experiment to a certain range of random numbers. If controllers A and B are to be compared, a random sequence consisting of zeros and ones can be generated, with all zeros indicating the use of controller A, and all ones indicating the use of controller B.

Replication, blocking and randomisation are seldom used in isolation. Two experimental combinations are described by Napier-Munn [30]: A replicated block experiment, and a randomised block experiment that was extended to a replicated randomised block experiment. For the third case described by Napier-Munn [30], a repeated on-off strategy, or blocking, was used, but a randomised block experiment is suggested, as time trends exist.

A method commonly employed by control engineers is a simple comparison of plant data before automation and after automation (e.g. single month-on, month-off trials). Although this may at first sight appear to use the blocking experimental procedure consisting of one block, the fundamental requirement of blocking is not satisfied, as operating conditions within the block are likely to change. Bergh *et al.* [32] quantified the benefits brought about by a supervisory control system (SCS), by comparing two months' data before implementation of the SCS to two months' data collected more than one year before, whilst a DCS was controlling the plant. Unless the improvement is large and the advantages hence obvious, this technique in general produces statistically invalid data. Changes in operating conditions will generally overwhelm the effects the experiment was designed to test for, invalidating any conclusions drawn.

Similar to this scenario, historical data is also used extensively to justify advanced control upgrades [33]. The "before and after" type experiments (improvement estimates relative to a base case) used by Marlin and co-workers [34] provides a useful framework in identifying control upgrade projects. These techniques however have severe shortcomings in producing statistically significant data required for controller evaluation [35]. Historical data might in many cases be the only method of control project justification. The infrastructure required for a new control strategy, or to perform comparative experiments, often contributes a major cost component to the proposed control upgrade project, favouring historical data analyses. Results based on historical data should however be examined with caution and a thorough statistical verification conducted to ensure that important trends are not overlooked. It is also suggested to perform a thorough audit after implementation of the new control strategy (using randomisation, blocking and/or replication) to verify the accuracy of recommendations based on historical data.

A number of further variations on the experimental procedures can be used to account for specific plant operating conditions. Craig and Henning [35] compared two controllers on a flotation circuit using a repeated on-off switching strategy, switching once per day. It is mentioned that a trial schedule was drawn up to alternate between the two controllers, and some form of randomisation was thus included. This experimental procedure can be classified as a random blocking experiment with a block size of 2 days. It might be argued that numerous changes might occur within each block, as there are for example four shifts per day. The dynamics of the system should however also be considered to ensure that the controllers are compared under typical operating conditions, and not just during the transient period. Craig and Henning [35] stated that the residence time of the circuit, as well as the frequency with which grades are analysed was used to determine the time between switching. Although knowledge of shifts could intuitively lead to a selection of a smaller block size, knowledge of plant dynamics should always carry an appropriate priority, and no experimental procedure attempted without sufficient process knowledge.

Instead of selecting the block size based on known disturbances, Craig and Henning [35] suggested switching as frequently as possible, but with each on-period significantly longer than the longest time constants of the process. These suggestions were further investigated by Oosthuizen, Craig and Pistorius [36] to determine the optimal switching time between two controllers, using a simple blocking experiment. It was concluded that an on-off switching time of 5 times the slowest time constant of the closed loop plant and controller would minimise the influence of external disturbances. A block experiment with a block duration of 10 times the slowest time constant of the plant is thus suggested. For a simple first order system, the time to reach a steady state is approximately equal to 5 times the slowest time constant. It is thus essential to ensure bumpless transfer of control between the two controllers to prevent the experiment from including only transient behaviour and no steady state behaviour. Randomisation was not included in the analysis [36], but it is expected that randomisation will yield better results (smaller resultant influence due to disturbances), even if the suggested switching time of 5τ is increased, as randomisation prevents biasing of the test data due to slow varying disturbances. If bumpless transfer of control is not possible, the best strategy might be a randomised block experiment with a block size larger than 5τ, but still as small as possible.

6.3. STATISTICAL TOOLS.

The test most commonly used for comparative experiments, is the t-test, of which 2 variations will be discussed. A brief discussion of analysis of variance will also be given. No statistical method is however valid, if the assumptions that are implicit to the test are ignored. The implicit assumptions for the t-test are the following [30]:

1. The data are normally distributed.
2. Each data point is a random independent sample of the population of all possible experimental outcomes, for the given system.
3. The two sample variances are estimates of the same population variance (i.e. they are not significantly different to each other).

For most processes the first assumption on normality can be accepted *a priori*, and the distribution can also be verified if necessary. The third assumption can also be checked easily using the F-test. The second assumption is a frequent cause of trouble as feed characteristics of processes vary with time and recovery or production is usually correlated with feed grade. Sequential measurements will thus not be random samples, but will depend to some extent on the preceding ones. Time trends in data raise the total variance and can also violate the assumption of sample independence [30].

t-tests are commonly used for comparing the means of two samples of data. In process control applications, the difference between the mean values of a controlled variable before and after automation can often be translated directly into increased profit. A null hypothesis is commonly stated which is assumed true until proven differently. In this case the null hypothesis would be that the two sample-means are identical. An alternative hypothesis is then stated that needs to be proven with a defined degree of statistical significance. In this example the alternative hypothesis will typically be that the two sample-means are significantly different from each other, or that one mean is larger than the other. A two-tailed test would be used for the first alternative hypothesis, and a one-tailed test for the second alternative hypothesis [31].

In most of the experimental techniques described in Section 6.2, experiments are carried out in pairs. The difference between each pair of measurements is in general of more importance than the absolute values. The t-test can be subdivided into a two-sample t-test and a paired comparison t-test.

For the two-sample t-test, the means of the two sample sets are calculated and compared using standard statistical formulas. If two controllers, A and B, were tested using a blocking technique, the mean of all the measurements for which controller A were used will be calculated, and compared to the mean of all the measurements where controller B was used. This is in general not a good comparison, since the variations between compounds will swamp any difference there may be between the two methods (Controllers A and B) [31]. A better approach would be to use the paired comparison t-test.

For the paired comparison t-test, the difference is calculated between each pair of data, provided that the data was generated in pairs. If the two controllers to be compared give similar results, the differences defined above would have a mean of zero. If one controller has a higher mean than the other does, the sample mean of the differences will be significantly different from zero. Standard statistical tables can be used to determine with a certain degree of significance if the means are really different. Napier-Munn [30] emphasises the advantages of the paired comparison t-test over the two-sample t-test, by calculating the number of paired measurements required to determine with 90 % confidence that a statistically significant increase has occurred for a given example. It was found that double the number of measurements was required for the two-sample t-test than for the paired t-test, clearly illustrating the increased sensitivity of the paired t-test.

In many process control applications, improved regulatory control due to a new control strategy enables the operator to move the setpoint of a controlled variable closer to a limiting value, thereby increasing process efficiency. This can typically be done in processes with constraints or in processes with non-linear performance functions [9]. Profits (determined by the performance function) are maximised by maintaining a controlled variable as close to its limits as possible. In statistical terms it can be said that a reduced variance enabled the operator to increase or decrease the mean. The advantages of

a controller capable of reducing variations from the setpoint can however not be quantified by comparing the means, since no difference would exist between the means until the setpoint is changed. An analysis of the variances of the two controllers would have to be performed prior to changing the setpoint, to determine if the mean can be shifted due to a reduced variance whilst still staying within the operational region of the plant. Variances can be compared using the F-test. The ratio between the two variances can be calculated, and the significance level (of differences in the variances) read from standard tables.

Analysis of variance (ANOVA) is typically used if several groups exist in which the same experiments were conducted. If 4 operators for example control the same plant at different times, ANOVA can be used to determine if the observed product variations can be attributed only to natural process variations and experimental error, or if the operators have a significant influence on the final product quality. ANOVA is also used in blocking experiments to determine the influence of the separate blocks.

The basic principle ANOVA relies on, is that any measurement is made up of the sum of the population mean, influences that can be accounted for, and unaccounted factors including natural variations and experimental errors. The total variance of the sample thus consists of variances due to known causes (e.g. different operators) and natural variations that cannot be accounted for. ANOVA is in principle an arithmetical process of splitting up a total variance into its component parts [37]. Once the separate contributions to the total variance have been determined, the F-test can be used to compare the identified variances to the variance describing the experimental error. If the variances are significantly different, it can be concluded that the identified factors have a significant influence on the measurements, and their expected variances can be determined.

The statistical distribution of variances is described by the χ^2-distribution. An estimate of the variances is given by ANOVA, and the expected range of the variances can be calculated using the χ^2-distribution. In experiments where the difference between setpoints and process constraints are determined by the natural variations around the setpoints, an analysis of the sample variances will give insight into the magnitude of possible setpoint changes. A more detailed discussion of ANOVA is not within the scope of this work, and

the complete statistical procedure can be found in Chatfield [31] and Davies [37], including a number of real test examples.

6.4. THE DURATION OF AN EXPERIMENT.

The duration of an experiment, and thus the number of repetitions required for each controller is determined by three factors: The degree of precision required, the amount of variability in the experimental material and the available resources to conduct the experiment, including time [38]. In general, the smaller the difference that needs to be detected, the more data is required.

The sample size of an experiment can be determined from the equations that are used to analyse the effects of the treatments. An example is shown for a one-tailed t-test, as is commonly used in testing the difference between recovery means, but a similar analysis can be done for the other tests described in Section 6.3. The equation used for a t-test is shown in Equation 6.1,

$$t = \frac{(\bar{x}_1 - \bar{x}_2) - (\mu_1 - \mu_2)_{H_0}}{\hat{\sigma}_{\bar{x}_1 - \bar{x}_2}} \tag{6.1}$$

where $\bar{x}_1 - \bar{x}_2$ is the difference between the sample means, $(\mu_1 - \mu_2)_{H_0}$ is the hypothesised difference of the population means (= 0), and $\hat{\sigma}_{\bar{x}_1 - \bar{x}_2}$ is the estimated standard error of $(\bar{x}_1 - \bar{x}_2)$ given by Equation 6.2

$$\hat{\sigma}_{\bar{x}_1 - \bar{x}_2} = \sqrt{\sigma^2(\frac{1}{n} + \frac{1}{n})}, \tag{6.2}$$

where σ^2 is the variance of the response variable which is assumed to be the same for both controllers. Assuming that both controllers are used equally often, the number of times each controller is used is thus equal to n and the total number of tests 2n. Substitution of the t-value for a defined level of significance, the difference in sample means to be tested $(\bar{x}_1 - \bar{x}_2)$ and the estimated standard error of the data $(\hat{\sigma}_{\bar{x}_1 - \bar{x}_2})$ thus yields the required value

of n. The duration of the experiment can now be calculated by multiplying the duration of each test (controller on or off) as described in Section 6.2, by the number of tests (2n).

6.5. THREATS TO VALIDITY.

In the process of designing an appropriate experimental procedure, it is important to be aware of a number of threats to the validity of the results. If a threat is identified that might invalidate the results, the experimental procedure should be revised to eliminate the potential threat. Tien [39] classified the threats in 5 categories identifying 20 threats. Although many of these threats are applicable to the social sciences, many can be extended to the evaluation of engineering processes. A short discussion of the most important threats follows:

1. Internal validity.

    Extraneous events not representing typical operating conditions may occur during the test period. These incidents should be removed from the test-data if possible. Gradual deterioration of processes may occur, including cyclical deterioration that may be observed as a long time trend in product quality. Natural variations in the test-units, including variations in measurements need to be considered. Instrumentation changes or calibration may take place during the evaluation and should be accounted for if it cannot be avoided.

2. External validity.

    The sensitivity or responsiveness of a test subject may change since the test subject is aware of the test. An operator could for example control a plant better than normal during the test, since he is aware that his actions are being monitored.

3. Construct validity.

    Tests conducted under non-typical conditions can prevent the results from being extended to the general operational case.

4. Statistical conclusion validity.

    Manual intervention or the lack thereof, e.g. selecting which data should be omitted, is a serious threat if not conducted appropriately. This may lead to the rejection of a true hypothesis (type 1 error) or the non-rejection of a false hypothesis (type 2 error).

5. Conduct conclusion validity. Design complexity may preclude the complete and successful conduct of the evaluation. Economic infeasibility, including hidden and unanticipated costs, may lead to incomplete or inappropriate evaluation methods.

6.6. AN EVALUATION FRAMEWORK.

"The reason most evaluations or purposeful analyses fail – or are not valid – is because research or evaluation designs are lacking" [39]. According to Napier-Munn [30] plant improvement trials are often inappropriately designed, take too long and lead either to no conclusion when a useful conclusion could have been reached, or the wrong conclusion. "Unless a sensible design is employed, it may be very difficult or even impossible to obtain valid conclusions from the resulting data" [31]. It is thus clear that a clearly defined framework should be used to set up and conduct experiments, in order to draw any sensible conclusions.

The following evaluation framework is suggested by Chatfield [31]:

1. Process understanding.
2. Define the problem.
3. Determine which quantities should be measured.
4. Determine the accuracy of measurements and calibrate instrumentation.
5. If some variable is derived from other measurements, determine the distribution of the derived variable by examining the distribution of error through the system.
6. Make a list of factors influencing the value of the response variable.
7. Calculate the number of observations that should be made, based on statistical significance.
8. Decide which values should be used for each factor in each individual test run.
9. Set up a mathematical model to describe the testing procedure (e.g. the difference between two means).
10. Test the hypothesis and draw conclusions.

The framework suggested by Craig and Henning [35] starts where Chatfield's [31] framework ended, although some overlapping occurs. It elaborates much more on the experimental procedure, whilst Chatfield [31] focuses on the preliminary steps ensuring good experimental design. The framework proposed by Craig and Henning [35] follows:

1. State the hypothesis that needs to be tested, e.g. "The new controller is better than the old one".

2. Establish a base case to aid in experimental design.

3. Design an experiment to generate unbiased production data, which captures the economic performance of the control systems.

4. Monitor the experiment and make sure it is carried out as planned.

5. Analyse the generated data and determine the sample statistics for each.

6. Test and accept or reject the hypothesis.

7. Estimate the monetary benefits.

8. Do an economic project evaluation.

Marlin [34] proposed a benefit analysis method comprising three steps (points 1 to 3 in the following discussion). These steps however address a wide range of actions, mostly overlapping with the steps mentioned by Chatfield [31]. After the initial benefit analysis, two more steps (points 4 and 5) are mentioned, in some ways overlapping with the procedure proposed by Craig and Henning [35]. The method proposed by Marlin [34] is however not intended for experimental procedures, but mainly for the estimation of benefits from historical data. The procedure and a short discussion are given for completeness:

1. Interviews.
   The aim of this step is to become familiar with the plant operation, review control equipment, inspect field instrumentation, etc.

2. Basis and base case operation.
   This step identifies typical plant operation using the existing control system, identifies time periods where the plant operated in unusual modes, etc.

3. Opportunity identification.

    A brainstorming session usually initiates this step, after which feasible opportunities are identified. Potential benefits are calculated for the identified opportunities.

4. Conclusions based on benefits.

    Since the aim is to identify benefits and not test them, the conclusions include estimated control benefits, the control concept, control and process equipment needed, and the engineering effort required.

5. Control benefits calculation.

    The benefit is calculated taking the improvement, the value of the improvement, the unit throughput, the operational time and a service factor into account. These factors will be discussed in more detail in Section 6.6.

The evaluation framework suggested by Tien [39] is subdivided into 3 main steps:

1. A projected look at the range of program characteristics (from its rationale, through its operation and anticipated findings). This step also includes the identification of the problem that needs to be solved.

2. A prospective consideration of the threats to the validity of the final evaluation. The threats were discussed in detail in Section 6.4.

3. A more immediate identification of the evaluation design elements. This step is subdivided into the following categories:

3.1. The test hypothesis to be used.

3.2. The selection scheme of test groups.

3.3. The measuring framework, including the variables to be measured as well as a model describing their linkages.

3.4. The measurement method, e.g. the number of samples, test period, etc.

3.5. Selection of the analytical techniques to be used for data analysis.

Many of the steps proposed by Tien [39] overlap with those suggested by Chatfield [31], Craig and Henning [35] or combine several identified steps. The steps proposed by Craig and Henning [35] are in the final stage of the general control problem (GCP) whilst the preliminary steps (e.g. process understanding) would be covered, by solving the complete

GCP. It will thus be attempted to create a more comprehensive evaluation framework by combining the three suggested strategies. Although many of the steps may seem obvious (e.g. process understanding), their importance are often underestimated, and these steps will thus form part of the combined evaluation strategy.

The following combined evaluation strategy is thus proposed:

1. Process understanding.
2. Define the problem to be solved.
3. Determine the variables that should be measured.
4. Determine the accuracy of the measurements and calibrate instrumentation.
5. If some variable is derived from other measurements, determine the distribution of the derived variable by examining the distribution of error through the system.
6. Make a list of factors influencing the value of the response variable, which could invalidate the result.
7. State the hypothesis that needs to be tested.
8. Design an experiment to generate unbiased production data, which captures the economic performance of the control system. This step includes mathematical modelling of the test procedure, the number of observations required, the test period, a selection of analytical techniques to be used for analysis, etc.
9. Monitor the experiment and make sure it is carried out as planned.
10. Analyse the generated data and determine sample statistics for each.
11. Test and accept or reject the hypothesis.
12. Estimate the monetary benefits.
13. Do an economic project evaluation.

6.7. THE ECONOMIC EVALUATION OF CONTROLLERS.

The functional evaluation of a controller is often a simple process, since a display of trends is likely to provide sufficient proof of a controller's regulatory improvement. The economic performance evaluation is however a much more complicated process

necessitating a carefully planned experiment. Such a scenario is described by Craig and Henning [35], for a flotation circuit. Although the regulatory improvement of the new controller is clearly discernible from time trends, a carefully planned experiment was required to measure an improved recovery (proportional to profit) in the presence of noise with a magnitude of approximately 10 times the measured improvement.

Marlin *et al.* [34] defined the following formula describing benefits.

$$\text{BENEFIT} = (\text{IMPROVEMENT}) \times (\text{INCREMENTAL VALUE}) \times$$
$$(\text{UNIT THROUGHPUT}) \times (\text{TIME}) \times (\text{SERVICE FACTOR}) \qquad (6.3)$$

Knowledge of the improvement brought about by the new controller, the increase in profit due to an incremental change, the unit throughput per year, and the proportion of the time the plant and controller is operational, allows the calculation of the benefits of the new controller. The improvement can be estimated using any of the experimental techniques described above. For a multivariable process the determination of the incremental values is however a complicated process due to interactions between variables and possible non-linear characteristics. The format of the cost function (a combination of all the incremental values and cost of operation) can be rather complex and will depend on the plant configuration.

The fact that a controller can increase profit is in general not sufficient motivation for a control upgrade, or sufficient proof that the project is economically viable. An analysis of the expected cash cost and cash revenues associated with the project throughout the expected project life needs to be done. A diagram showing the typical characteristics of the cumulative cash flow for the duration of a project is shown in Figure 6.1 [40]. The aim of any project should be to ensure that the final positive cash flow at H is as large as possible, especially compared to the initial negative cash flow at D (see Figure 6.1). A detailed discussion of capital budgeting tools can be found in Allen [40], but a short discussion of the most commonly used capital budgeting tools follows.
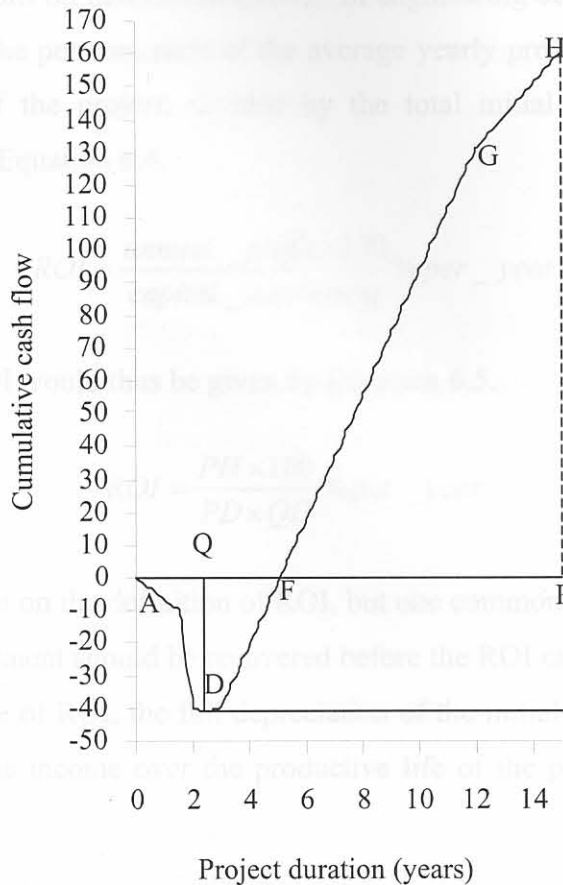
Figure 6.1. Typical cumulative cash flow diagram [40].

The simplest capital budgeting tool is the payback time. This is the time that elapses from the start of the project (A) to the breakeven point (F) (see Figure 6.1). The payback time thus indicates the time required in recovering early project expenditure and the cumulative investment, from the cumulative net project income [40]. The shorter the payback time, the more attractive the project appears.

One variation on the payback time is the time required to recover the initial investment. In this case the payback time would start at the time where the capital investment has all been spent, and not at the beginning of the project (A). Payback time provides no indication of the expected return on the investment and ignores everything in time beyond the breakeven point. It is thus very limited in its interpretation [40] and other more descriptive capital budgeting tools are often preferred.

The second tool is return on investment (ROI). In engineering economic evaluation, ROI is usually defined as the per cent ratio of the average yearly profit (net cash inflow) over the productive life of the project, divided by the total initial investment [40]. This definition is shown in Equation 6.4.

$$ROI = \frac{annual\_profit \times 100}{capital\_investment} \% per\_year \qquad (6.4)$$

For Figure 6.1, the ROI would thus be given by Equation 6.5.

$$ROI = \frac{PH \times 100}{PD \times QD} \% per\_year \qquad (6.5)$$

Several variations exist on the definition of ROI, but one common argument put forward is that the original investment should be recovered before the ROI can be calculated. For the calculation of this type of ROI, the full depreciation of the initial investment is calculated as a charge against the income over the productive life of the project [40], as shown in Equation 6.6.

$$ROI\_(including\_depreciation) = \frac{(PH - QD) \times 100}{PD \times QD} \% per\_year \qquad (6.6)$$

Both payback time and ROI are very selective in the project cash flow information that they use, and both techniques ignore important relevant information regarding the changing pattern of cash flow with time and the time value of money [40]. Two other measures incorporating these factors will be discussed in turn.

In economic terms a project can be regarded as a series of cash flows throughout the project's lifetime. To take the time value of money into account, the annual cash flows have to be discounted to the time of evaluation before they can be compared and used as an evaluation measure [40]. The present value (PV) of a cash flow (C_t) at the end of project year t, is given by Equation 6.7, for i the applied discount rate.

$$PV = \frac{C_t}{(1 + i)^t} \qquad (6.7)$$

The net present value (NPV) of a complete project is the sum of the present values of the project's individual cash flows. The NPV is given by Equation 6.8, for n the complete project life in years.

$$NPV = \sum PV = \sum_{t=0}^{t=n} \frac{C_t}{(1+i)^t} \qquad (6.8)$$

An effect of the discounting of the project cash flows, is that cash flows later in the life of the project make progressively smaller contributions to the project NPV [40]. Inaccurate estimates of the exact project lifetime or cash flows late in the project life would thus not influence the NPV significantly.

For a project to be viable in terms of generating a profit, the NPV should be positive. The discount rate would be determined by the effective cost of capital and the risk of the project [40].

Another economic measure used to determine project viability is the discounted cash flow rate of return (DCFR). It is also commonly referred to as interest rate of return and internal rate of return (IRR). DCFR is closely related to NPV, and is defined as the yearly discount rate that would yield a project NPV equal to zero [40]. The DCFR is thus determined by solving Equation 6.9 recursively, where $100I$ = project DCFR, % per year [40].

$$NPV = \sum_{t=0}^{t=n} \frac{C_t}{(1+I)^t} = 0 \qquad (6.9)$$

A higher discount rate than the DCFR would thus yield a negative NPV and a lower discount rate a positive NPV. DCFR provides a measure of the rate of return expected over the whole project life, and thus the larger the value of the DCFR, the more economically attractive is the project [40].

The focus of this dissertation is the identification of suitable control strategies to increase profit, and the estimation of the profits attributable to advanced control. Specific cash flow calculations used to motivate control upgrades would depend on the level of automation of a specific steel plant, and can not be extended to a general case. Using the capital budgeting tools discussed above and the results of the simulation study presented in Chapter 7, an estimate of the economic viability of the suggested control strategy can be made to represent the scenario at a specific steel plant.

The advantages of a new control system are often not fully described by a benefit calculation. Some advantages that are difficult to quantify might include increased field instrument service factors, better operator displays, automated history data base, and better regulatory control which results in smoother plant operation [39]. Reduction in pseudo downtime, i.e. by preventing unscheduled plant shutdowns, by enabling fast recovery after plant disruptions, and by enabling fast plant start-ups and shutdowns are further unquantified advantages [35]. Tien [39] suggested listing these factors as additional advantages to the project, as it may be a determining factor in management decisions.

6.8. CONCLUSION.

A wide range of methods is available to ensure accurate evaluation of control systems. Unfortunately inappropriate techniques are often used to evaluate control systems or to justify implementation of new control systems, leading to invalid results. A number of experimental procedures were discussed (blocking, replication and randomisation), applicable to processes operating in noisy environments and which are subject to large natural variations. The most common statistical tools used for comparative experiments were discussed and a framework for experimental design presented that would ensure the generation of unbiased data. The process of translating a functional improvement into an economic benefit was also discussed briefly and some capital budgeting tools mentioned. The evaluation framework will form the basis of the economic evaluation presented in Chapter 7, and the tools described will be utilised where applicable.