Cost-Efficient Designs for Assessing Work-Related Biomechanical Exposures

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Abstract
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Work-related disorders due to biomechanical exposures have been subject to extensive research. Studies addressing these exposures have, however, paid limited attention to an efficient use of resources in exposure assessment. The present thesis investigates cost-efficient procedures for assessment of work-related biomechanical exposures, i.e. procedures aiming at a proper balance between statistical and economic performance.

Paper I is a systematic review of tools used in literature providing cost-efficient data collection designs. Two main approaches were identified in nine publications, i.e. comparing cost efficiency among alternative data collection designs, and optimizing resource allocation between different stages of data collection, e.g. subjects and samples within subjects. The studies presented, in general, simplified analyses, in particular with respect to economics.

Paper II compared the cost-efficiency of four video-based techniques for assessing upper arm postures. The comparison was based both on a comprehensive model of cost and error and additionally on two simplified models. Labour costs were a dominant factor in the cost efficiency comparison. Measurement bias and costs other than labour cost influenced the rank and economic evaluation of the assessment techniques.

Paper III compared the cost efficiency of different combinations of direct and indirect methods for exposure assessments. Although a combination of methods could significantly reduce the total cost of obtaining a desired level of precision, the total cost was, in the investigated scenario, lowest when only direct measurements were performed. However, when the total number of measurements was fixed, a combination was the most cost efficient choice.

In Paper IV, demand functions were derived for a four-stage measurement strategy with the focus of either minimizing the cost for a required precision, or maximizing the precision for a predetermined budget. The paper presents algorithms for identifying optimal values of measurement inputs at all four stages, adjusted to integers, as necessary for practical application.

In summary, the thesis shows that it is important to address all sources of costs and errors associated with alternative measurement designs in any particular study, and that an optimal determination of samples at different stages can be identified in several cases not previously addressed in the literature.

Keywords: measurement strategy; postural loads; statistical performance; optimization; economic evaluation; resource allocation; input costs; cost function; returns to scale; elasticity

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To my wife Noushin

List of publications:

Paper I. Mahmoud Rezagholi, Svend Erik Mathiassen (2010). Cost-Efficient Design of Occupational Exposure Assessment Strategies – a Review. The Annals of Occupational Hygiene; 54: 858-868.

Paper II. Mahmoud Rezagholi, Svend Erik Mathiassen, Per Liv (2012). Cost Efficiency Comparison of Four Video-based Techniques for Assessing Upper Arm Postures. Ergonomics; 55:350-360.

Paper III. Mahmoud Rezagholi, Apostolos Bantekas (2012). Optimizing the Fraction of Expensive Direct Measurements in an Exposure Assessment Study. Submitted.

Paper IV. Mahmoud Rezagholi (2012). Deriving Cost-Efficient Strategies for Observational Assessments of Postural Loads. Submitted.

Contents

Introduction

In many countries, musculoskeletal disorders (MSDs) are a major cause of increased sickness absences and disability, and thus a major cause of social costs (Lubeck, 2003; Punnett and Wegman, 2004; Piedrahita, 2006). They are also a substantial cause of reduced work quality and labour productivity (Eklund, 1995; Piedrahita, 2006). Researchers in the field have shown that biomechanical exposures at work, which are assessed by *postures*, *movements*, and *forces* (van der Beek and Frings-Dresen, 1998; Burdorf and van der Beek, 1999; Ebersole and Armstrong, 2006), are significant causes of MSDs (Tousignant et al, 2002; Punnett and Wegman, 2004). Biomechanical exposures at work have therefore been extensively investigated in order to contribute both epidemiologic and occupational ergonomic research for reducing the occurrence of work-related MSDs (van der Beek and Frings-Dresen, 1998; Spielholz et al, 2001; Punnett and Wegman, 2004).

The assessment of MSD risk in different occupational groups, however, is dependent on access to reliable quantitative data on biomechanical exposures at work. Estimates associated with large *error* do not give sufficient *information* about the exposures. Decisions must be made prior to designing assessment studies for producing sufficient information about work-related biomechanical exposure variables. The main decisions when designing measurement studies of biomechanical exposures in workplaces are centred on the following three questions:

1) **Which technical method(s) should be used in collecting and analysing data on an exposure of interest?**
2) **How many investigators (data collectors and data analysts) with which level of work intensity ought to be employed in exposure assessments?**
3) **How many subjects and recordings per subject over time should be sampled?**

Answering the questions above will result in the respective identification of "appropriate" *measurement technique*(s), *work design*, and *sampling strategy* for the study. The capital (equipment and buildings), labour (investigators), and subjects used in an exposure measurement study are the *inputs* to the *statistical production*, while the information produced about biomechanical

exposure variables by using a specific *combination* of the inputs is the *output*.

To answer the first question, different techniques have been introduced in recent decades for assessing the biomechanical exposures, while acknowledging different systematic sources of errors at different rates in using them (Winkel and Mathiassen, 1994; Burdorf and van Riel, 1996; Guangyan and Buckle, 1999; Burdorf and van der Beek, 1999; David, 2005; Stanton et al, 2005; Takala et al, 2010). The measurement techniques introduced in these studies can be classified into three main groups: *self-reports*, *observation methods*, and *direct technical methods*. Each of these techniques is characterized by a certain level of statistical and economic performance in exposure measurement. Generally, self-reports such as questionnaires and interviews carry low costs (Trask et al, 2007) but also low statistical performance (Guangyan and Buckle, 1999; Spielholz et al, 2001). Direct measurement techniques are well-known for good statistical performance, but carry high costs both for equipment and for labour in terms of calibration and data analysis (David, 2005; Koppelaar and Wells, 2005). Observation methods, on the whole, are considered a "compromise" between the other two methods in terms of cost and statistical performances (Tousignant et al, 2002; David, 2005; Bao et al, 2009). Observational exposure assessments have therefore been widely used, from simple assessments by an expert at the worksite to advanced video-based techniques (De Looze et al, 1994; Juul-Kristensen et al, 1997; Bao et al, 2007; Takala, 2010; Weir et al, 2011). Some observation methods are based on a work sampling approach, in which still images of the work are observed at certain time points (Watson and Mac Donncha, 2000; Weir et al, 2011), while others require the investigator to assess biomechanical exposures in a continuous sequence of work (Fransson-Hall et al, 1995; Burt and Punnett, 1999; Bao et al, 2007); obviously, the latter of these is more labour-intensive. These measurement techniques have also been evaluated and compared in terms of their appropriateness and agreement in assessment of different work-related biomechanical exposures, and thus their ability to produce information on the exposures (Winkel and Mathiassen, 1994; van der Beek and Frings-Dresen, 1998; Spielholz et al, 2001; Tousignant et al, 2002; David, 2005; Bao et al, 2006; Teschke et al, 2009; Dale et al, 2010; Gardner et al, 2010). Comparisons of these technical methods have also revealed that the methods can be *complementary* to each other in attempting to produce information on specific work-related biomechanical exposures (Kristensen et al, 2001; Village et al, 2009). Other methods of comparison are concerned with cost and feasibility (Trask et al, 2007; Trask et al, 2012). However, to date, techniques for producing information on work-related biochemical exposures have never been compared according to their ability to produce this information at low cost; that is, according to their *cost efficiency*. In evaluating and making decisions about a technical measurement method, neither its cost alone nor its statistical performance

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alone is an important factor. The cost of implementing a technical method of exposure measurement should thus be evaluated in relation to its ability to produce information.

In order to answer the second question, attention has been paid to appropriate work designs in terms of the quantity and intensity of work required from the investigators participating in the exposure assessment studies, including the appropriate level of competence and experience (Ketota et al, 2001; Denis et al, 2002; Sullivan et al, 2002; Ebersole and Armstrong, 2006; Kazmierczak et al, 2006; Andrews et al, 2008). The question has been particularly topical since observation methods have been developed and widely used in exposure assessment studies. Appropriate work designs are then implemented to reduce intra- and inter-observer uncertainties and thus increase the reliability of observational exposure assessments. However, a reduction of these uncertainties (i.e. an improvement of work design for exposure assessment) can be achieved by employing *skilled* labour in the form of highly competent and experienced investigators and/or by increasing the competence and experience of the currently-employed observers. The quantity, quality, and intensity of the work performed by investigators in exposure assessments should thus be evaluated economically in the relevant studies. For this, the cost of labour in the exposure assessment studies is assessed and analysed in relation to the *labour productivity*; that is, the contribution of the investigators to the amount of information produced during exposure assessments.

In addition to measurement techniques and work designs, researchers in the field have attempted to provide appropriate sampling strategies in the exposure assessment studies by answering the third research question. The aim was to determine an efficient number of subjects and exposure recordings per subject over time that would lead to reductions in important sources of random error in exposure assessments. Studies designing sampling strategies for measuring biomechanical exposures thus argue for the statistical power and precision provided by larger sample size at levels that are known with greater variability (Burdorf, 1995; Burdorf and van Riel, 1996; Hoozemans et al, 2001; Mathiassen et al, 2003; Nordander et al, 2004; Mathiassen and Paquet, 2010). Several important general guidelines for designing appropriate sampling strategies have been presented and discussed (Burdorf and van Riel, 1996). A further method of optimizing sampling strategies is provided by equations that have been developed for determining the number of subjects and the number of measurements per subject (Burdorf, 1995; Mathiassen et al, 2002), and for evaluating the precision of exposure means in terms of the variance between and within subjects (Mathiassen et al, 2003; Nordander et al, 2004; Jackson et al, 2009). The marginal effect of subject on the precision of the exposure mean estimate has also been addressed (Hoozemans et al, 2001). In addition, the ability of various sampling strate-

gies to detect an intervention's effect on the overall job exposure of an individual worker has been assessed (Mathiassen and Paquet, 2010). However, the underlying strong assumption in these studies is that sampling at each stage has the same cost when the numbers of subjects and recordings are determined only by the between- and within-subject variances. The cost of sampling at each stage should thus be considered when strategies for biomechanical exposure assessments are assessed and/or optimized. Methodologies to optimize the allocation of resources between sampling units in different stages have been available for a few decades (Cochran, 1977; Sukhatme PV et al, 1984). Further, when optimizing strategies devoted to assessing work-related biomechanical exposures by observation methods, the cost and statistical performance associated with work designs (i.e. the number of observers and their repeated assessments) should also be considered by researchers in the field.

The ability of a measurement design to produce information can be improved by using more advanced technical methods for recording and assessing exposures, recruiting qualified investigators, and increasing the amount of measurement inputs. However, this improvement may have an unwelcome price in terms of increasing cost. Thus, when choosing measurement technique(s), work design, and sampling strategy for recording and assessing biomechanical exposures, the amount of information produced (the output) should be compared with the cost of the measurement inputs. The *optimal choice* in each category should be a compromise between the quantities of cost used and the information produced during exposure measurement. A measurement design concerned purely with statistical performance is not necessarily optimal even if it produces a large amount of information (low error). Reducing bias and uncertainty may add substantially to the total cost of the statistical production, and the size of the reduction in the total error may not be worth the increase in the cost. The desired level of information or the limits of the research budget should thus be considered in designing exposure assessment studies. The optimal choice should either yield a sufficient level of information produced at minimum cost (i.e. a fixed-output cost-minimized measurement design) or produce the maximum information at a given measurement input (i.e. a fixed-cost output-maximized measurement design). There are few studies in this research area, and most were published more than ten years ago; in addition, they were aimed at providing cost-efficient designs for assessing exposures other than the biomechanical kind (Spiegelman and Gray, 1991; Spiegelman, 1994; Stram et al, 1995; Armstrong, 1995 and 1996; Lemasters et al, 1996; Duan and Mage, 1997; Shukla et al, 2005; Whitmore et al, 2005). There is also a pure theoretical study attempting to optimize a sampling strategy based on both cost and variance components (Mathiassen and Bolin, 2011).

The cost efficiency studies described above have applied methodologies both for optimizing resource allocation between different uses in exposure assessment studies (Spiegelman and Gray, 1991; Spiegelman, 1994; Stram et al, 1995; Duan and Mage, 1997; Whitmore et al, 2005; Mathiassen and Bolin, 2011) and for comparing cost efficiency in alternative non-optimal measurement designs (Armstrong, 1995 and 1996; Lemasters et al, 1996; Shukla et al, 2005). The methodologies applied for optimizing resource allocation can be used to aid decision-makers in either maximizing the quantity of information produced using the available resources or minimizing the cost of producing a certain level of information. The methodologies applied for comparing cost efficiency in alternative non-optimal measurement designs, however, are aimed at identifying the *relatively* most cost-efficient design for exposure assessment studies.

Economic decisions have been made for allocating limited resources between direct technical and indirect methods in exposure measurement (Duan and Mage, 1997), between studies devoted to epidemiological research (Spiegelman, 1994), and between different stages in exposure assessments (Stram et al, 1995; Whitmore et al, 2005; Mathiassen and Bolin, 2011). However, the problem of allocating resources between the number of investigators and the number of repeated assessments has yet to be resolved.

Economic decisions have also been made to determine the highest point at which more investment in increased accuracy is worthwhile (Armstrong, 1995 and 1996), and to identify the relatively most cost-efficient measurement designs among many alternatives in order to save resources or reduce random error (Lemasters et al, 1996; Shukla et al, 2005). However, the applied methodologies do not estimate the cost of producing additional information based on the available measurement inputs.

In economic decision-making on inputs to exposure assessments, three basic concepts are used: *opportunity cost*, *efficiency*, and *marginalism*. These are related to *scarcity* of resources, *choices* of different input combinations, and *changes* in exogenous economic parameters such as costs. Scarcity is related to the fact that the available resources are not sufficient to support all the activities we would like to use them for. Thus, we have to choose between the different ways of using the scarce (limited) resources in exposure measurement. However, in choosing one way to use the resources we forgo other opportunities to use them; this is in fact a cost, namely the opportunity cost. Thus, we should allocate the scarce resources efficiently between different uses in an exposure assessment study. The concept of marginalism, for example the cost of producing additional information on exposure or recruiting additional measurement input, is another important issue for economic decision-making. The quantity of inputs to exposure measurement such as instruments, equipment, laboratories, offices, investigators, materials, and subjects are the scarce resources that should be determined in order to im-

prove statistical and economic performances. For this, decision-makers in the field need information about changes in costs as well as information on opportunity costs, for example the benefits forgone in statistical performance when the scarce resources are used on one specific design rather than another design. The basic economic principles of opportunity cost, efficiency, and marginalism should always be followed when attempting to answer the three previously-mentioned research questions in designing exposure assessment studies. The derived solution can involve at most one of the following: 1) *minimizing* the cost of producing a predetermined quantity of information on the exposure, 2) *maximizing* the information produced for a given research budget, or 3) *reducing* cost and/or error by selecting the *relatively* most cost-efficient design. By developing the theories and methodologies applied in the available cost efficiency studies, the important problems in providing cost-efficient designs for assessing work-related biomechanical exposures can be resolved. Appropriate methodologies are needed for examining the possibilities to reduce statistical error or to save economic resources by selecting the relatively most cost-efficient design among alternative non-optimal measurement designs. Appropriate methodologies are also required for optimizing resource allocation between direct technical methods and subjective methods in exposure assessments, between subjects and recordings per subject in a sampling strategy, and between the number of investigators and the number of repeated assessments in a work design.

Aims

The overall aim of this doctoral thesis was to provide cost-efficient designs for assessing work-related biomechanical exposures by efficiently allocating the scarce resources to different uses in the exposure assessment studies. The specific aims for each study were:

Paper I.   To develop the theories and methodologies applied in the studies identified in a literature search through a critical review, in order to gather guidelines for providing cost-efficient designs for work-related biomechanical exposure assessments.

Paper II.   To compare the cost efficiency of four video-based techniques in assessing upper arm postures in order to identify the relatively most cost-efficient assessment technique.

Paper III. To optimize the fraction of expensive direct measurements in an exposure assessment study by resolving a precision-requiring cost minimization problem.

Paper IV.   To optimize the cost and precision of a measurement strategy consisting of a two-stage sampling strategy and a two-stage work design devoted to observational assessments of postural loads at work.

Methodologies

The basic quantities, concepts, issues, and tools used to resolve different decision problems in designing assessment studies of work-related biomechanical exposures are introduced below. They are first described generally, and then matched to the solution of each specific problem in Papers I–IV.

Conceptual and methodological framework

Error and information

Biomechanical exposure *data* is the intermediate product to provide *information* on the exposure. The exposure data is a set of objective facts about events at work that without further refinement has no value in occupational ergonomic researches. Exposure data is often incorrectly collected, and it is associated with *error*. Each statistical product, for example the exposure mean estimate, thus consists of *unwanted* error and *wanted* information. The quantity of information produced on the biomechanical exposure is considered to be high when the quantity of error in the exposure assessments is low.

The error of an exposure assessment is the distance between the true exposure and its approximate measure. The distance is caused partly by *random* sources of error, and partly by *systematic* sources of error. Random error is a result of taking only a sample of an occupational group instead of investigating the whole of the occupational group. Systematic error on the other hand, defined as constant deviation from actual value, consists of all errors that have sources other than sample estimation, such as improper application of measurement techniques, omission of variance components in assessing the precision of the mean estimate, and purposive subject selection. These two well-known types of error refer to sampling *variance* and *bias* respectively. Sampling variance is the average squared deviation of the sample mean exposure from the group mean exposure over all possible samples M that could be drawn from the population. That is, $Var(\hat{\mu}) = \dfrac{\Sigma(\hat{\mu} - \mu)^2}{M}$. Bias is the difference between the expected value of the mean exposure estimate and its true value: $B = E(\hat{\mu}) - \mu$. The maximum possible information will be pro-

duced when both bias and uncertainty are minimized. This is related to the concept of the *mean square error* (MSE) of the sample mean exposure, which is defined as the average squared distance between the true exposure and its estimate, squared so that positive and negative errors do not cancel each other out. That is, $MSE(\hat{\mu}) = E(\hat{\mu} - \mu)^2 = Var(\hat{\mu}) + B^2$. The root of the mean square error shows the quantity of error produced during an exposure assessment study. This measure is used in Paper II for assessing the statistical performance of posture assessment techniques, while only random sources of error are considered in Papers III and IV for optimizing measurement designs.

Statistical efficiency

Statistical efficiency can be assessed by quantifying the difference between the error-exposed estimate of a group mean exposure and its true value. The statistical efficiency of a measurement design, consisting of measurement technique(s) and strategy, refers to its ability to produce information on a specific exposure variable. The coefficient of statistical efficiency provided by a measurement design is defined as the minimum possible variance of an unbiased mean estimate for the given statistical resources divided by its current variance (Hogg et al, 2005). An unbiased assessment of group mean exposure is thus efficient if the minimum possible variance is equal to its current variance. If the efficiency coefficient is equal to unity, the exposure assessment is *absolutely* efficient, in which case there exists a *minimum variance unbiased estimator* (MVUE). If several measurement designs give unbiased estimates, the efficient design is the one that gives the most precise mean estimate of the exposure. A measurement design can also be *relatively* efficient if it produces less error than the alternatives under equal conditions. The concept of relative efficiency is used in the cost efficiency comparison of assessment techniques reported in Paper II.

Input-output relationship

The quantity of information produced is usually considered as the *output* of an exposure assessment study. For analysing cost efficiency, we need to know how the quantity of information is affected by measurement *inputs* such as subjects, offices and laboratories, instruments and other types of capital equipment, and particularly the investigators who collect and analyse the exposure data. The input-output relationship is a physical relationship that describes how the measurement inputs are transformed into the output information.

Technological constraints: Only certain combinations of measurement inputs are feasible for producing a given amount of information about an oc-

cupational exposure. The output is thus a function of technology and inputs according to the following *production function*:

$$Y = A \cdot f(K, L, S), \qquad\qquad\qquad [1]$$

where Y denotes the amount of information produced; A the technology; K the physical capital consisting of measurement instruments, other equipment, and buildings (offices and laboratories); L the labour (the investigators who collect and analyse the exposure data); and S the subjects. The statistical production function shows the maximum information that can be produced from a given combination of measurement inputs. This general form of statistical production function is applied in Papers I–IV in terms of different error equations.

The *isoquant plane* offers an alternative way to describe the technological constraints. An isoquant plane shows all possible combinations of measurement inputs that yield the same level of output (i.e. the same quantity of information), and thus shows the flexibility available to researchers when making the decision of an appropriate input-combination to use. The shape of the isoquant plane (or isoquant curve when only two inputs are considered) depends on the rate of substitution/complementation between the measurement inputs (cf. Figure 1 in the Appendix). Isoquants are used in Papers III and IV.

Input productivity and marginal product of input: Researchers in the field have to make decisions about the quantity of measurement inputs. For this, they need to know how the amount of information changes as the measurement inputs are incrementally increased; for example, they might want to know how the precision of a sample group mean exposure will increase if they measure one additional subject. The rate of this change is called the *marginal product* of the measurement inputs. While input *productivity*, or the average product of input i (AP_i), is the output per unit of the input, the marginal product of the input (MP_i) measures the change in output resulting from an additional unit of the input. Naturally, if all inputs to the statistical production are allowed to vary, we would expect that the marginal product of a specific measurement input will diminish as we get more and more of that input when holding other inputs constant. The precision of a mean exposure estimate will be improved by increasing the number of subjects and/or recordings over time. However, the gain in precision from increasing the units in a given sampling stage while holding units in other stages constant may diminish in size after a certain threshold (Hoozemans et al, 2001). As another example, when the labour input to collect exposure data is small and the measurement instruments are fixed, extra labour adds considerably to the amount of exposure data produced. However, when there are too many data collectors, some of them become *ineffective* in the production and the marginal product of labour falls. The concepts of input productivity and the

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marginal product of the input are used when optimizing measurement designs in Papers III and IV.

Returns to scale: If all inputs to the statistical production are increased proportionately, the amount of information produced would certainly change. This relative change gives information about *returns to scale*. When the amount of information is changed by the same proportion, that is, the *degree of homogeneity* is equal to one, the statistical production function is characterized by *constant returns to scale* (CRS). However, if the amount of this change is less than the amount of change in the measurement inputs, that is, the degree of homogeneity is less than unity, the situation is characterized by *decreasing returns to scale* (DRS). When the degree of homogeneity exceeds unity, then the statistical production technology exhibits *increasing returns to scale* (IRS). If, for instance, the precision of a sample exposure mean is the output being produced by investigators and subjects, the statistical production technology exhibits CRS when a doubling of investigators and subjects results in a doubling of precision. If this doubling results in less than doubled precision or more than doubled precision, then the production technology is characterized by DRS or IRS, respectively. Returns to scale for the statistical production technologies are discussed in Paper I and measured in Papers III and IV.

Costs

Input costs: To assess the total cost of an exposure assessment study, it is necessary to estimate input costs such as license fees and project manager salaries, the user cost of capital (equipment and buildings), the cost of maintenance and technical support (repairs, services, and calibrations), the cost of recruiting subjects, the cost of labour for recording and analysing exposure data, the cost of energy and material used, the cost of educating and training investigators, and the cost of controlling the quality of the data produced.

Accounting cost and economic cost: The total cost of an exposure measurement study includes the user cost of physical capital and the rent paid for laboratory and office buildings. However, in some cases the research group already owns a working building and hence does not have to pay rent. While a financial accountant would treat this cost as zero, an economist would note that the group could have earned rent or some other return on the building by leasing it to another company or devoting it to another activity. This forgone rent/return is the *opportunity cost* of using the building for the exposure assessment study. Similarly, an economist will take the forgone interest into account when calculating the user cost of equipment capital. We must thus distinguish between *economic* cost, which refers to the future performance of the research centre, and *accounting* cost, which focuses on the centre's fi-

nancial statement. Accounting cost consists of the actual expenses in an exposure assessment study plus depreciation charges for equipment. Economic cost is the total cost to a research centre of utilizing resources in the assessment study, including opportunity costs (i.e. costs associated with opportunities that are forgone when the resources are not put to their highest-value alternative use). The papers in this thesis consider the economic costs associated with measurement designs.

Input costs in the short run: The short run is a period of time in which the quantity of at least one measurement input remains unchanged during the statistical production (i.e. the assessment of biomechanical exposures at work). Thus, in short-run production, the total cost of exposure assessment consists of the *fixed costs* and *variable costs*. The fixed costs are those that do not vary with the level of output, and refer to the inputs that remain unchanged during exposure assessment. Fixed costs can be recovered only by ending the exposure assessment study. The variable costs are those that vary as the output varies, and refer to the variable measurement inputs. Examples of fixed costs include licence fees, rentals, and equipment costs, while examples of variable costs include the cost of recruiting subjects and investigators. In Paper II both fixed and variable costs are considered, while in Papers III and IV all costs are considered to be variable.

Input costs in the long run: The long run is the amount of time needed to make all measurement inputs variable. Thus, in the long run, all costs associated with the statistical production are variable. In this case, the user cost of capital (equipment and buildings), which usually remains unchanged in the short run, should be assessed. The user cost of capital is usually the total annual cost of owning and using a capital asset, and is equal to economic depreciation plus forgone interest as an opportunity cost. The inflation-adjusted user cost of capital (UCC) can be expressed as a rate of capital value:

$$UCC = r \cdot PP ,$$ [2]

where PP is the purchasing price, considered as the value of the capital estimated in the market, and the rate is calculated as $r =$ depreciation rate $+$ interest rate $-$ inflation rate.

Purchase prices can be amortized across the life of the asset. For example, if the price of a new measurement instrument is 40000 SEK and the working life of the instrument is five years (i.e. the depreciation rate is 20%), the amortized cost will be 8000 SEK per year. The amortized cost per year is viewed as the annual economic depreciation. If the forgone interest or financial return is 10%, while the inflation rate is 2%, the user cost of the measurement instrument is then $(0.20 + 0.10 - 0.02) \times 40000 = 11200$ SEK per

year. The estimated costs associated with different designs in Papers II–IV all include the user cost of capital.

Isocost line equations: Cost constraints in the assessment studies of biomechanical exposures at work are shown by *isocost line* equations. The equations show all possible combinations of measurement inputs that can be purchased for a given total cost. In the cost efficiency studies, the total cost of exposure assessment is mainly divided between capital (equipment and buildings) and labour (investigators), for each level of total cost. The isocost line equation is then defined as $C = w \cdot L + r \cdot K$, where w, L, r, and K stand for wages, labour, price of capital, and capital, respectively. Solving the equation for capital, an isocost line equation appears as $K = \dfrac{C}{r} - \left(\dfrac{w}{r}\right) \cdot L$.

The slope of the isocost line, $-\left(\dfrac{w}{r}\right)$, which is the opportunity cost, reveals that the research group may give up a unit of labour to purchase w/r units of capital while the total cost of the exposure assessment study remains unchanged. For example, if the investigators are paid 200 SEK per hour and the user cost of capital estimated by equation [2] is 100 SEK per hour, the research group can replace one hour of investigations with two hours of use of capital (i.e. equipment and buildings) in the exposure assessment study with no change in total cost. Thus, a capital-intensive measurement design is recommended in this case. Note that the possible input combinations in the isocost line (cf. Figure 2 in the Appendix) are all *affordable* but not necessary *optimal*. There is thus no guarantee that an affordable input combination will either minimize the total cost or maximize the quantity of information produced. A *comparative static analysis* of the isocost line equation shows that: 1) if the given total cost changes and/or both input prices change in the same proportion, the slope of the isocost line (i.e. the opportunity cost) remains unchanged, but the line shifts in parallel, and 2) an individual change in input prices or changes with different proportions in both input prices will lead to a change in the slope.

Cost curves; average and marginal costs: Analysis of cost curves associated with the statistical production technology is another important issue in an economic evaluation of a measurement design. A decision-maker should be able to predict how the cost curves will change as the corresponding output changes when *expanding* or *contracting* the exposure assessment study. Future costs may be estimated from curves relating a cost to the corresponding output that a research group can control. To predict cost correctly, we must know the underlying relationship between these variables. There are two important cost curves in producing data on biomechanical exposures: *average cost* and *marginal cost*, which are used to evaluate the measurement

design as a basis for decision-making regarding the level of output and the quantity of inputs. Average total cost, or simply average cost (*AC*), is the cost per unit output and is obtained by dividing the total cost by the output (e.g. the quantity of information produced on exposures). Marginal cost (*MC*) is the cost associated with producing one additional unit of the output (in this case, an additional unit of information produced on exposures). *MC* is obtained by differentiating the total cost function with respect to output. Least squares regression analysis is often used to fit the curve to the cost data. If the cost is proportional to its corresponding output, the average cost and the marginal cost are equal at each level of output. If, however, the cost is not strictly proportional to the output, the average and marginal costs diverge. Average and marginal costs of precision are compared in Papers III and IV.

Price elasticity of demand: Another important tool for economic decisions around the quantity of measurement inputs is the *price elasticity of demand* (E_D^p). According to common assumptions in microeconomics, the demand of an input will usually be reduced when its price increases (negative relationship). The own-price elasticity of demand for an input measures the responsiveness of the demand function for the input in percent towards a percentage change in its price; that is, the sensitivity of an input to the statistical production towards a change in its price. This concept is applied in Paper IV.

Cost efficiency analysis

The analysis of cost efficiency when producing information on biomechanical exposures at work, regardless of specific application, is concerned with an attempt to trade off between the cost and the amount of information produced. The two main approaches to cost efficiency analysis in this research area are optimization of resource allocation between inputs to exposure measurement, and comparison of the cost efficiency in alternative non-optimal measurement designs. The objective of the first approach is either to maximize the quantity of information using defined measurement inputs, or to minimize the cost of producing a predetermined level of information. The objective of the second approach is to identify the *relatively* most cost-efficient measurement design that is able to produce more information per unit cost.

Optimizing resource allocation: The optimization of resource allocation between different measurement inputs requires both an isocost line equation and a function that describes the input-output relationship; for example, a model relating the precision of a group mean exposure estimate to the given measurement inputs. Optimization analysis results in deriving a function that describes the cost-output relationship, such as a *cost function* showing the

lowest possible cost to achieve a given precision. The optimization is based upon some general assumptions. For instance, it is assumed that decision-makers typically attempt to choose the most precise measurement design that they can afford. Researchers can also reasonably choose a cost-minimized measurement design that gives an acceptable level of precision. Thus, there are two approaches to optimizing exposure measurement designs: *precision maximization* and *cost minimization*. For producing information at optimum, the measurement inputs must be combined in one specific way. This input combination can be determined by finding the point where the isoquant curve touches the isocost line (or where the isoquant plane touches the iso-cost plane, in cases where several measurement inputs are used). When the isocost line touches the isoquant curve at a point; that is, the slope of the isocost line is equal to the slope of the isoquant curve, the *optimal choice* is made. This tangent point offers either the cheapest or the most precise meas-urement design. Thus, when using two measurement inputs, the condition holds when the ratio of the marginal products of the inputs is equal to their corresponding cost ratio, regardless of whether the aim is to minimize the cost of producing a predetermined quantity of information or to maximize the quantity of information produced for a given input. For instance, if the user cost of measurement equipment is twice the labour cost of investiga-tions, the optimal input combination is where the marginal product of the measurement equipment is twice the marginal product of an investigator. If a SEK spent for measurement equipment is more productive than a SEK spent for an investigator, the decision-maker will want to use more equipment and fewer investigators in the exposure measurement design. However, if the decision-maker reduces the number of investigators and increases the amount of equipment, the marginal product of the investigator will rise and the marginal product of the equipment will fall so that the condition holds again. The optimization condition is used in Paper IV.

The *technical efficiency* (TE) and *productive efficiency* (PE) of a currently-used design are two aspects of its economic efficiency (Farrell, 1957; Kopp, 1981; Maietta, 2000) that can be measured only when resources in exposure assessments are optimally allocated. The economic aspects of efficiency are measured by comparing non-optimal and optimal choices in terms of cost or precision. Technical efficiency, or equivalently physical efficiency, refers to the ability of a measurement design to produce information from a given quantity of measurement inputs and technology. Technical efficiency is measured as the precision actually produced divided by the maximum preci-sion technically possible with the given measurement inputs and technology. The statistical production is thus technically efficient only along an isoquant curve where the precision of mean exposure cannot be increased by any oth-er possible combination of measurement inputs. Technical inefficiency is caused by *wasting* measurement inputs and/or using the *wrong* production technology (i.e. inefficient combination of measurement inputs). The devia-

tion of observed (current) precision from the maximized precision is a measure of the technical inefficiency associated with a measurement design. Productive efficiency is defined as the ability of a measurement design to produce a well-specified precision at minimum cost. The cheapest input combination would be the point of tangency between an isocost line and the isoquant curve. In principle, a cost function will be used to estimate productive efficiency as the minimum possible cost divided by the current cost of an exposure measurement design.

The efficiency indices indicate the cost savings through the elimination of inefficiency. The quantities $1 - \text{TE}$ and $1 - \text{PE}$ indicate the reduction in total cost if the inefficiency associated with technical and productive factors, respectively, is eliminated. The concept of productive efficiency is applied in Paper III, while both technical and productive efficiency indices are applied in Paper IV.

Comparing non-optimal measurement designs: A cost efficiency comparison of alternative non-optimal measurement designs can help decision-makers allocate the available resources in such a way that error and/or cost will be reduced. To compare cost efficiency in alternative measurement designs, researchers first need to assess the total cost and total error associated with each measurement design. In the comparison approach, with a short-run economic decision, all costs and errors are important and should be taken into consideration. Another essential component is appropriate measures incorporating the expected costs and errors associated with the alternative measurement designs. These measures can then suggest which of the alternative designs is appropriate for the statistical production. There are two general research questions in this approach of cost efficiency analysis:

1) Which of the alternative measurement designs produces information at lower cost?
2) What is the cost of improving the statistical performance of a design?

The first of these can be addressed using the concept of *relative cost efficiency* (RCE), and the second using the *marginal cost-benefit ratio* (MCBR). The RCE measures the amount of information per unit cost that a measurement design can produce, while the MCBR is defined as the marginal cost of an efficient design divided by its marginal benefit in statistical performance compared to the design traditionally used. The first measure is used in Paper II, and the second in Paper III.

Methodologies applied in Papers I–IV

*The aim in **Paper I** was to develop the theories and methodologies applied in the relevant studies through a critical review in order to provide cost-efficient designs for work-related biomechanical exposure assessments.*

The review required a systematic search for gathering the relevant studies. Two search algorithms were used in the PubMed and ScienceDirect databases: [cost $\cap$ (precision $\cup$ accuracy $\cup$ power) $\cap$ assessment $\cap$ exposure] and (cost-efficient $\cap$ validation study). The relevance of the identified studies was judged using three criteria: 1) addressing assessment of exposure variables, 2) consideration of both statistical and economical performance criteria, and 3) mathematical definition and solution of the cost efficiency problem. Nine studies were assessed as being relevant for a critical review. The analysis of these studies was focused on three issues: 1) the statistical performance criterion and the model applied to assess it, 2) the isocost line equation and economic performance criterion, and 3) the cost efficiency measure.

*The aim in **Paper II** was to compare the cost efficiency of four video-based assessment techniques.*

The four techniques, which differed in cost and statistical performance, were used to assess four aspects of arm postures of hairdressers at work via a number of films recorded in Umeå. The four aspects were defined by the mean angle of the upper arm, as well as the proportions of time that the hairdressers worked with their upper arm at an angle above 60°, above 90°, and less than 15°, respectively. The four video-based assessment techniques were based either on continuous observation (CO) or work sampling (WS). In the first technique (CO15), the observer watched 15 s of a 30 min video and was then asked to assess the four postures (120 video clips). In the second technique (CO120), 2 minutes of video were shown to the observer before asking for the posture assessments (15 video clips). In the third technique (WS15), the observer was asked to estimate the upper arm angle from video stills separated by 15 s (120 stills). In the last technique (WS120), which was used as a *reference*, the posture assessments were based on video stills separated by 2 minutes (15 stills). The assessment techniques were compared under four non-optimal measurement strategies which combined labour-intensive and labour-saving work and sampling designs (Table 1).

Table 1. Specification of the four simulated measurement strategies in terms of the number of observers (n_o), repeated assessments per observer (n_a), hairdressers (n_s), and videos per hairdresser (n_m). N gives the total number of assessments.

	n_o	n_a	n_s	n_m	N
MAMS	4	2	25	4	800
MAFS	4	2	4	1	32
FAMS	1	1	25	4	100
FAFS	1	1	4	1	4

Three sources of error were considered in assessing the statistical performance of the alternative measurement designs:

1) *Random error*: This is an error of non-observation that occurs when using a sample to estimate exposure. Because not all members of the occupational group were measured, the sample mean exposure is subject to random error. The uncertainty or dispersion of the sampling distribution of the sample mean was first shown by *sampling variance*. The squared root of sampling variance (*standard error*) was then used to quantify the random error. One property of random error is that it decreases as the corresponding sample size is increased. The *precision* of the sample mean exposure was estimated as the inverse of the standard error.

2) *Misspecification error*: This type of error is caused by omitting some important sources of random error in the statistical error equation. It was important to assess both between-subject and within-subject variance, but due to the use of observational assessment techniques it was also necessary to assess between-observer and within-observer variance. Information about the amount of misspecification bias can be obtained by examining the difference between the expected "true" standard error (including all sources of random error) and the standard error excluding observation-based sources of random error.

3) *Measurement error*: Error caused by observation methods carries a high risk of systematic error, and can be the largest source of error for many exposure assessments. The behaviour of the observer, the status and calibration of the video camera, and the method by which the video films were observed could all lead to overestimation or underestimation of the posture variables. The labour input to the statistical production when processing data (entering, coding, imputing, weighting, and editing the posture data) was therefore identified as an *error-producing unit*. This factor of statistical production produces error as well as information, and hence affects the output specification. Methods of reducing and assessing the measurement bias associated with the assessment techniques include the observers' education and experience in collecting and analysing work-related arm postures, and the use of inclinometer measures.

The *absolute error*, $AE = |\hat{\mu} - \mu|$, estimated by the root of the *mean square error* of the mean posture estimate, $\sqrt{MSE(\hat{\mu})} = \sqrt{V(\hat{\mu}) + B^2}$, was used to quantify the total error produced by each assessment technique. The variance of the mean posture, $V(\hat{\mu})$, was estimated as

$$V(\hat{\mu}) = \frac{\hat{\sigma}_{bo}^2}{n_o} + \frac{\hat{\sigma}_{bs}^2}{n_s} + \frac{\hat{\sigma}_{ws}^2}{n_s \cdot n_m} + \frac{\hat{\sigma}_{wo}^2}{n_o \cdot n_a \cdot n_s \cdot n_m}, \text{ where } \hat{\sigma}_{bo}^2, \ \hat{\sigma}_{bs}^2, \ \hat{\sigma}_{ws}^2, \text{ and } \hat{\sigma}_{wo}^2$$

denote estimates of the variance between observers, between subjects, within subjects, and within observers, respectively; and n_o, n_s, n_m, and n_a represent the numbers of observers, subjects, recording days per subject, and repeated assessments per observer, respectively. The bias (B) was estimated using the difference between each assessment of a video recording and the corresponding inclinometer measure.

The total cost (TC) associated with each assessment technique was estimated as $TC = C_L + C_F + C_M + C_S + C_I + C_K + C_R + C_E + C_C$; that is, the sum of the cost of assessing postures (C_L), the cost of recording the video films (C_F), the cost of materials (C_M), the cost of software (C_S), the cost of introductory course and training for the observers (C_I), the user cost of equipment including economic depreciation and forgone interest (C_K), the rental cost of office space required for performing the assessments (C_R), the energy cost (C_E), and the cost of post-observation quality control of data (C_C). The cost of training included the costs of trainers as well as salaries paid to the supervisory trainees. The labour costs (C_L, C_F, C_S, C_I, and C_C) were estimated as $C_L = w \cdot L$, where w is the corresponding average wage per hour and L is the amount of working time. The user cost of equipment capital was estimated as $C_K = r \cdot K$, where K and r stand for the capital and the price of capital, respectively.

The *relative total cost*, $RTC_{(r,i)} = \dfrac{TC_{(r)}}{TC_{(i)}}$, and *average relative efficiency*,

$$ARE_{(i,r)} = \frac{1}{4} \cdot \sum_{j=1}^{4} \frac{AE(\hat{\mu}_{rj})}{AE(\hat{\mu}_{ij})} \text{ (where } j = 1,...,4 \text{ indexes the four postures),}$$

were used to evaluate the economic and statistical performance of each alternative assessment technique i compared to the reference r in assessing the four postures.

The *overall performance* of the alternative techniques was assessed using the *relative cost efficiency*, which measures the amount of information per unit cost that an alternative assessment technique can produce compared to the reference technique. That is, $RCE_{(i,r)} = ARE_{(i,r)} \cdot RTC_{(r,i)}$. When ignoring

economic considerations, the measurement design that produces more information is preferred. When instead ignoring statistical considerations, the measurement design that saves more resources (lower cost) is preferred. Considering both objectives in an exposure assessment study, the two basic measures (i.e. the relative total cost and the average relative efficiency) should be multiplied together to assess the overall performance of a design. If an assessment technique i produces more information per unit cost than the reference technique r, then it is relatively cost efficient $(RCE_{(i,r)} > 1)$ and will therefore be preferred to the reference technique. The relative cost efficiency measure is theoretically based on the concept of *utility* from the statistical production. The expected utility of a measurement design, $E(U_{design})$, could be assessed by the inverse of its total losses; that is, its cost-error product, $E(U_{design}) = f(TC, AE) = (TC \cdot AE)^{-1}$. The overall performance of each measurement design was thus evaluated on the basis of its expected utility.

To assess the effects of *hidden costs* (i.e. costs other than C_L) and bias on the relative cost efficiency of the alternative assessment techniques, the overall performance was also estimated (i) using only the labour cost of posture assessment and (ii) with only random errors.

*The aim in **Paper III** was to optimize the fraction of expensive direct measurements in an exposure assessment study by resolving a precision-requiring cost minimization problem.*

Two measurement techniques were assumed to be combined in an occupational exposure assessment study: one statistically efficient but expensive, and the other cheap but statistically inefficient.
The combined exposure mean, $\hat{\mu}_c$, was estimated as $\hat{\mu}_c = f_1\hat{\mu}_1 + f_2\hat{\mu}_2$, where $\hat{\mu}_1$ and $\hat{\mu}_2$ stand for the exposure means estimated by direct technical measurements and indirect subjective estimates, respectively; and f_1 and f_2 denote the fractions of the direct measurements and subjective estimates, respectively. The precision of the combined mean, $P(\hat{\mu}_c)$, was estimated as the inverse of the standard error of the combined mean,

$P(\hat{\mu}_c) = \dfrac{1}{\sqrt{Var(\hat{\mu}_c)}} = \left(\dfrac{f_1\hat{\sigma}_1^2 + f_2\hat{\sigma}_2^2}{N} \right)^{-\frac{1}{2}}$, while the total cost (TC) of the

combined data-producing technique was expressed as $TC = N \cdot \left(f_1\overline{c}_1 + f_2\overline{c}_2 \right)$, where N is the total number of measurements; $\hat{\sigma}_1^2$ and $\hat{\sigma}_2^2$ are the mean variances estimated by the direct measurement technique and the indirect

subjective method, respectively; and $\overline{c}_1$ and $\overline{c}_2$ are the average unit prices of a direct measurement and an indirect subjective estimate, respectively.

The total cost of the combined technique as a function of the precision, the mean variances, and the average unit prices was assessed for different fractions of direct measurements by using the derived cost function $TC = P^2 \cdot \left[f_1 \overline{c}_1 + \left(1 - f_1\right) \overline{c}_2 \right] \cdot \left[f_1 \hat{\sigma}_1^2 + \left(1 - f_1\right) \hat{\sigma}_2^2 \right]$. The *cost elasticity of the precision*, E_P^C, and the returns to scale, E_C^P, were estimated as $E_P^C = \dfrac{\partial \ln C}{\partial \ln P}$ and $E_C^P = \dfrac{\partial \ln P}{\partial \ln C}$, respectively, for an economic evaluation of the combined technology. We were particularly concerned with the *scale* of the exposure measurement study: did the combined production give a technological advantage that made the exposure measurement study more productive as its scale increased? This economic evaluation of the statistical production technology used the cost elasticity of precision, which is obtained by dividing the marginal cost divided by the average cost. *Economy of scale*, which relates to increasing returns to scale, is achieved when the marginal cost is less than the average cost. Conversely, *diseconomy of scale*, which relates to decreasing returns to scale, occurs when the marginal cost is greater than the average cost. In the case of constant returns to scale, the marginal cost is equal to the average cost (cf. Figure 3 in the Appendix). A rational decision-maker (in this case, a researcher in the field) should not produce more information (improving precision) after the point at which $AC = MC$, otherwise the research firm risks diseconomy of scale.

The constrained optimization problem regarding the hypothesized combined measurement technique was defined as a precision-requiring cost minimization problem, which was chosen to be resolved in order to identify the optimal fraction of direct technical measurements. Different combined techniques were compared according to their productive efficiency (PE). The productive efficiency of each alternative technique was obtained by dividing the cost of the optimal choice by the cost of the alternative with the yielded precision kept the same. The cost savings (CS) made through elimination of the productive inefficiency associated with each non-optimal design were then estimated as $1 - \text{PE}$.

The cost of improving precision by one unit when using the two measurement inputs was estimated with the *marginal cost-benefit ratio*, $MCBR_{(j)} = \dfrac{TC_{(j)} - TC_{(i)}}{P_{(j)} - P_{(i)}}$, which is the incremental cost of the introduced design j divided by the incremental benefit compared to the cheaper but less

precise design *i*. The incremental cost and the incremental benefit refer to the differences of the compared designs in cost and precision, respectively.

To allow decision-making about the fraction of direct measurements and the extent of information produced on exposures, the shape (direction and curvature) and size of changes in the total cost as a result of increasing the fraction and the level of precision were investigated. Thus, the marginal effects of the fraction and the precision on the total cost of exposure assessments were estimated by using the regression equations $TC = \alpha + \beta_1 \cdot f_1 + \beta_2 \cdot f_1^2$ and $TC = \alpha + \beta_1 \cdot P + \beta_2 \cdot P^2$, respectively. The sign of the β_2 coefficient gives information about the curvature of the relationship between the total cost and the fraction or precision, respectively. If β_2 was not significantly different from zero, the relationship could be assumed to be linear. Hence, it was possible to have both a non-linear average cost curve (AC) and a non-constant but linear marginal cost curve (MC) in the cost-precision relationship. The marginal cost increases with *P* if β_2 is positive (diseconomy of scale), and decreases with *P* if β_2 is negative (economy of scale). As formulated, if the cost-precision relationship is nonlinear, different values of AC and MC will be obtained. If the regression coefficient β_2 is not significantly different from zero, AC will be equal to MC (linear cost-precision relationship). The cost curve would be strictly concave if $C''(P) < 0$ and strictly convex if $C''(P) > 0$ for all possible values of *P*.

The models were used to optimize the fraction of direct measurements and also to evaluate different combinations of direct measurements by inclinometer (n_1) and video-based observational estimates (n_2) in assessing the proportion of time that the hairdressers in Paper II worked with their upper arm above 60°.

*The aim in **Paper IV** was to optimize the cost and the precision of a four-stage measurement strategy devoted to observational assessments of postural loads at work.*

The strategy chosen for optimization was a four-stage measurement strategy combining a two-stage assessment work design consisting of the number of observers (n_o) and the number of assessments per observer (n_a), and a two-stage data collection procedure consisting of the number of subjects (n_s) and the number of recordings per subject (n_r).

Two constrained optimization problems were defined for optimizing the measurement strategy devoted to observational assessment of a work-related biomechanical exposure: 1) minimizing the total cost of achieving an ac-

ceptable level of precision (output constraint), and 2) maximizing the precision of the group mean estimate for a predetermined cost (budget constraint); both constrained optimization problems were solved by optimally determining the measurement inputs n_s, n_o, n_r and n_a. The isocost line equation for the four-stage assessment strategy was defined as $\overline{c}_s \cdot n_s + \overline{c}_o \cdot n_o + \overline{c}_r \cdot n_s n_r + \overline{c}_a \cdot n_s n_o n_r n_a$, while the precision yielded by the measurement strategy was expressed as $\left(\dfrac{\hat{\sigma}_{bs}^2}{n_s} + \dfrac{\hat{\sigma}_{bo}^2}{n_o} + \dfrac{\hat{\sigma}_{ws}^2}{n_s n_r} + \dfrac{\hat{\sigma}_{wo}^2}{n_s n_o n_r n_a} \right)^{-\frac{1}{2}}$,

where $\hat{\sigma}_{bs}^2$, $\hat{\sigma}_{bo}^2$, $\hat{\sigma}_{ws}^2$, and $\hat{\sigma}_{wo}^2$ are the between-subject, between-observer, within-subject, and within-observer variance components, respectively and $\overline{c}_s, \overline{c}_o, \overline{c}_r$, and $\overline{c}_a$ are the average variable costs of recruiting a subject, educating and recruiting an observer, producing one video recording, and performing an assessment procedure, respectively. The error equation describes the level of precision that can be reached by every possible combination of the four measurement inputs.

For producing information at optimum, the measurement inputs must be combined in a specific way which can be determined by finding the point where the isoquant curve of precision touches the isocost line equation. Thus, for minimizing the cost of achieving a predetermined level of precision or maximizing the precision subject to a given budget, by using the four measurement inputs, the condition $\dfrac{MP_i}{MP_j} = \dfrac{c_i}{c_j}$ would hold for any two measurement inputs i and j. The optimality condition for each two measurement inputs then holds where the ratio of marginal products (MP) of inputs i and j (i.e. the proportional gain in precision from additional units of inputs i and j) is equal to its corresponding cost ratio. When the isocost line touches the curve of isoquant at a point (i.e. the slope of the isocost line is equal to the slope of the isoquant curve), the *optimal choice* for the four-stage measurement strategy is found. This tangent point offers either the cost-constant most precise or the precision-constant cost-minimized measurement strategy. The economic decision thus has a dual nature. The optimal choice of measurement inputs was analysed not only as the problem of choosing the lowest isocost line tangent to the statistical production isoquant, but also as the problem of choosing the highest isoquant curve tangent to a given isocost line. In principle, maximizing the precision subject to a budget constraint gives the same condition that was necessary for minimizing the cost of achieving a required precision. The error equation was used to derive the dual cost function, which measures the minimum costs necessary to achieve the predetermined level of precision. The derived cost function reveals: 1) how the total cost of production increases as the level of precision increases,

and 2) how the total cost changes as input prices change. The cost function was then rearranged to estimate the maximum precision that was possible with a fixed budget and the gain in precision that would be achieved by an increase in the budget.

The optimal units in each stage were derived for the two approaches, and then two cost-efficient measurement strategies were compared with the non-optimal measurement strategy to see whether they provided optimal solutions. This comparison was performed by measuring productive and technical efficiency. The productive and technical efficiencies were measured by dividing the minimized cost and the maximized precision of the optimized measurement strategy by the cost and precision of the current measurement strategy, respectively. Then, the cost saved through elimination of productive inefficiency and the gain in precision through elimination of technical inefficiency were obtained by $1 - \text{PE}$ and $1 - \text{TE}$, respectively.

The *cost elasticity of the precision* (E_P^C) and the returns to scale (E_C^P) were estimated as $E_P^C = \dfrac{\partial \ln C}{\partial \ln P}$ and $E_C^P = \dfrac{\partial \ln P}{\partial \ln C}$, respectively, for an economic evaluation of the statistical production technology and decisions about the size of the production. *Economy of scale* related to increasing returns to scale is achieved when the marginal cost is less than the average cost, while *diseconomy of scale* related to decreasing returns to scale occurs when the marginal cost of improving precision is greater than the average cost; that is, the cost elasticity of precision is greater than unity. For situations with constant returns to scale, the marginal cost is equal to the average cost (cf. Figure 3 in the Appendix). A decision to produce more information (a higher level of precision) after the point at which $AC = MC$ can result in diseconomy of scale.

The own-price elasticities of demand were estimated as tools for adjusting the demands to changes in costs and also for further economic evaluation of the statistical production technology. The own-price elasticity of demand for an input is *elastic* (sensitive to changes in cost) if it changes by more than one percent (i.e. $-\infty < E_D^p < -1$) in response to a one percent change in its price, and *inelastic* (not sensitive to changes in cost) if it changes by less than one percent (i.e. $-1 < E_D^p < 0$) in response to such a change. The applied methodologies were then developed to resolve a practical problem associated with the optimization of discrete measurement inputs.

Results

Paper I: Cost-Efficient Design of Occupational Exposure Assessment Strategies – a Review

The few relevant studies clearly showed the need for providing cost-efficient designs for exposure assessment studies by developing the applied concepts and tools in balancing cost and statistical performance. Providing cost-efficient designs requires well-behaved isocost line and error equations, in addition to data on input costs and sources of error. As the studies either compared non-optimal designs or optimized a design to exposure assessments, they should have used specific assumptions and methods in their analyses. The models applied in each study for assessing statistical efficiency, costs, and cost efficiency, were thus assessed on the basis of the objective and approach selected in the study. The concept of statistical efficiency in the reviewed studies, depending on the specific objective and approach, was expressed by indicators such as *precision*, *accuracy*, or *statistical power*. For optimization of a resource allocation, the most useful indicator of efficiency is precision, since any constant error produced in exposure assessment does not influence the optimal determination of variable samples. The concept of statistical power could only be employed in an optimization approach with some difficulty. For evaluating and comparing different measurement methods, on the other hand, it is appropriate to use indicators that include both systematic and random sources of error. Likewise, including the fixed costs associated with exposure measurement is important when evaluating or comparing alternative measurement designs. Hence, both fixed and variable costs are relevant in economic evaluation of designs where some measurement inputs are fixed. In studies optimizing resource allocation between inputs, where all inputs are allowed to vary, fixed costs should not be considered and included in the proposed isocost line equation, as they do not influence optimal solutions and economic decisions. The distinction between fixed and variable costs is thus important in cost efficiency analysis.

Generally, the reviewed studies did not discuss whether the underlying assumptions of the statistical models used for assessing precision of the mean exposure estimates were met; all studies employed a standard additive random effects model. The studies paid much less attention to the cost structure and economic analyses than to error models and statistical interpretations.

For instance, it was often not clear which cost curves (unit cost, average cost, or marginal cost) or which input costs (capital cost, labour cost, energy cost, or material cost) were considered and estimated in the isocost line equations. Regardless of whether the objective of a cost efficiency study is comparison or optimization, the input costs should be correctly identified, estimated, and modelled; otherwise, the *wrong* decision might be made. The social costs of using imperfect information on exposure (i.e. error-exposed estimates), which are the *opportunity costs* of not using the adequate measurement design, were not discussed in any of the studies, since the outputs were limited to the statistical performance of the exposure measurement design. Methods should also be developed for evaluating the social benefit of an exposure assessment study in monetary terms, so as to make it directly comparable to the total cost of the study. No studies considered the cost and productivity associated with the labour input, and none analysed the associations between minimized cost and statistical performance. The basic economic assumption in the studies was linear homogeneity between inputs and output (i.e. constant returns to scale), which leads to the *marginal cost* of producing information and/or purchasing measurement inputs being equal to the *average cost* at each quantity level of produced information and/or measurement inputs.

The various *elasticities* associated with statistical production are a fundamental issue to consider when making economic decisions on exposure assessment, but these were not addressed by any of the reviewed studies. The cost elasticity of output would show the relative change in the (minimized) cost as a result of a one percent change in the level of information produced, while the price elasticity of demand would measure the responsiveness and sensitivity of an optimized demand function to changes in an input cost. These elasticities are important tools for economic decision-making in exposure assessment studies.

Table 1. Characteristics of the cost efficiency studies reviewed in Paper I

Reference	Indicator of statistical efficiency	Purpose	Statistical assumption	Cost estimation
Lemasters et al. (1996)	Precision	Comparison of 32 alternative sampling strategies	Additive random error; three-stage sampling	Cost is equal to the number of Measurements
Shukla et al. (2005)	Precision	Comparison of alternative sampling strategies	Additive random error; three-stage sampling	Cost is equal to the number of Measurements
Armstrong (1995, 1996)	Accuracy	Comparison of two measurement methods	Existence of perfect measurement; single-stage sampling	Two different variable costs for the measurement methods; same fixed cost for both methods
Duan and Mage (1997)	Accuracy	Optimization of resource allocation between two measurement methods	Correlation between indirect and direct measurements; single-stage sampling	One cost component differing between direct and indirect measurement methods
Spiegelman and Gray (1991); Spiegelman (1994)	Discriminatory statistical power	Optimization of resource allocation between main and validation studies	Non-linear error model; single-stage sampling	Two aggregate costs of main and validation studies
Stram et al. (1995)	Precision	Optimization of resource allocation between two sampling stages	Additive random error; two-stage sampling	Two mixed cost components
Whitmore et al. (2005)	Precision	Optimization of resource allocation between three sampling stages	Additive random error; three-stage sampling	Three cost components; fixed cost addressed

Paper II: Cost Efficiency Comparison of Four Video-based Techniques for Assessing Upper Arm Postures

Due to large differences in labour costs for posture assessments, costs were a dominant factor in the cost efficiency comparison of the four assessment techniques when the comparison was based on the labour assessment costs alone (RcE), regardless of measurement strategy; and also when using the labour-intensive measurement strategy MAMS with any model. Thus, the most labour-saving assessment technique was also, in general, the most cost-efficient alternative for posture assessments. The effect of measurement strategies on the results showed that the cost savings gained by utilizing smaller work and sampling designs could allow for a more advanced assessment technique. Moreover, the gains from the improved statistical performance of larger work and sampling designs would compensate for the lower quantity of information produced by a simpler assessment technique.

The labour-intensive assessment techniques were generally superior to the labour-saving techniques in terms of statistical performance. However, the additional labour used in the labour-intensive techniques (i.e. the *incremental labour cost* of these techniques over the labour-saving techniques) exceeded their *incremental benefit* in statistical performance. Thus, labour-saving techniques generally appeared to be cost-efficient alternatives for posture assessments, relative to the labour-intensive techniques.

For the most resource-intensive measurement strategy, the ranking of the four assessment techniques was not affected when costs other than labour costs for posture assessments were excluded, nor when bias in the assessment of statistical errors was neglected. However, with a more resource-saving measurement strategy, improvements of the cost efficiency model did influence which technique appeared to be preferable.

Table 2: Assessment techniques ranked according to their cost efficiencies when using total costs and total errors (RCE), only assessment costs but total errors (RcE), and total costs but only random errors (RCe). The ranking is given under the four measurement strategies MAMS, MAFS, FAMS, and FAFS.

MAMS			MAFS			FAMS			FAFS		
RCE	RcE	RCe	RCE	RcE	RCe	RCE	RcE	RCe	RCE	RcE	RCe
WS120 (1)	WS120 (1)	WS120 (1)	WS120 (1)	WS120 (1)	WS15 (1.049)	WS120 (1)	WS120 (1)	WS15 (1.344)	CO120 (1.234)	WS120 (1)	WS15 (1.447)
WS15 (0.566)	WS15 (0.116)	WS15 (0.761)	CO120 (0.899)	WS15 (0.107)	WS120 (1)	CO120 (0.954)	WS15 (0.118)	WS120 (1)	CO15 (1.226)	WS15 (0.134)	CO15 (1.341)
CO120 (0.519)	CO120 (0.076)	CO120 (0.401)	WS15 (0.774)	CO120 (0.067)	CO120 (0.888)	CO15 (0.931)	CO120 (0.071)	CO15 (0.941)	WS15 (1.059)	CO120 (0.076)	CO120 (1.308)
CO15 (0.328)	CO15 (0.041)	CO15 (0.301)	CO15 (0.743)	CO15 (0.035)	CO15 (0.760)	WS15 (0.850)	CO15 (0.044)	CO120 (0.824)	WS120 (1)	CO15 (0.042)	WS120 (1)

Paper III: Optimizing the Fraction of Expensive Direct Measurements in an Exposure Assessment Study

Indefinite number of total measurements

The total cost of measurements decreased significantly as the fraction of direct measurements approached unity. By using direct measurements in a combined technique, the researcher could thus improve the cost efficiency of the observation method. When the total number of measurements was indefinite, the optimal fraction of direct measurements that minimized the cost of achieving a predetermined level of precision was unity; that is, although direct measurements were more expensive, the cost could only be minimized by using direct measurements alone. Hence, a combined technique was not the optimal choice in this case. A combined technique was only appropriate to employ when the total cost was also predetermined.

The regression analysis gave the result of $TC = 264967 + 68811 \cdot f_1 - 224198 \cdot f_1^2$. Here, the autonomous cost of the statistical production (264967 SEK) is the cost of achieving the required precision by using observational assessments alone (cf. Table 3). The high value of β_2 (224198 SEK) shows the significant curvature of the cost function, and the negative sign of β_2 shows that the cost will decrease at an increasing rate for every unit by which the fraction is increased.

In economics, it is hypothesized that cost curves will fall as the output increases up to a certain level, and then begin to rise again as capacity is reached or diseconomies set in. However, in the present study, with the precision of the combined mean considered as output and the fraction of the direct measurements set as constant, the total cost of the combined measurement technique increased with the output for all values of output, according to the cost-precision association $C = 32175 \cdot P^2$. The measurement technology was characterised by decreasing returns to scale, which led to the marginal cost exceeding the average cost; that is, diseconomies of scale would prevail if the marginal benefit of producing additional information would be less than the marginal cost.

Table 3. Calculation of total cost (TC), total number of measurements (N), numbers of direct measurements (n_1) and indirect estimates (n_2), productive efficiency (PE), and cost saving (CS) for different values of f_1. Costs are in SEK, and all values are rounded to the nearest integer. The predetermined precision is 2.75.

f_1	$1 - f_1$	TC	N	n_1	n_2	PE (%)	CS (%)
0.00	1.00	264967	346	0	346	41	59
0.10	0.90	269607	319	32	287	41	59
0.20	0.80	269762	291	58	233	41	59
0.30	0.70	265433	263	79	184	41	59
0.40	0.60	256620	236	94	141	43	57
0.50	0.50	243323	208	104	104	45	55
0.60	0.40	225543	180	108	72	49	51
0.70	0.30	203278	153	107	46	54	46
0.80	0.20	176530	125	100	25	62	38
0.90	0.10	145297	97	88	10	75	25
1.00	0.00	109581	70	70	0	100	0

Definite number of total measurements

When the total number of measurements was predetermined, a combined technique could be optimal in terms of a constraint on either cost or precision. As an example, the precision for a research budget that could not exceed 100000 SEK was maximized by $f_1 = 0.2$, while the cost of achieving a precision of at least 2 was minimized by $f_1 = 0.6$. If the research budget increased from 92700 SEK to 117000 SEK, the increased budget allowed for 30 more direct measurements instead of observational assessments, to improve the precision by 0.295. MCBR values showed whether the necessary investment for improving precision of an optimal design by one unit could be funded. The higher the demanded precision, the lower the necessary investment.

Table 4. Calculation of total cost (TC) and precision of combined mean (P) for different values of f_1. The cost of improving precision by one unit, MCBR, is calculated for the previous alternative. The total number of measurements (N), that is, the sum of direct measurements (n_1) and indirect estimates (n_2), is 100. Costs are in SEK.

N	f_1	$1-f_1$	n_1	n_2	TC	P	MCBR
100	0	1	0	100	76500	1.478	0
100	0.1	0.9	10	90	84600	1.540	125011
100	0.2	0.8	20	80	92700	1.612	109408
100	0.3	0.7	30	70	100800	1.695	94514
100	0.4	0.6	40	60	108900	1.791	80364
100	0.5	0.5	50	50	117000	1.907	67000
100	0.6	0.4	60	40	125100	2.048	54470
100	0.7	0.3	70	30	133200	2.226	42834
100	0.8	0.2	80	20	141300	2.460	32168
100	0.9	0.1	90	10	149400	2.789	22567
100	1	0	100	0	157500	3.297	14166

Paper IV: Deriving Cost-Efficient Strategies for Observational Assessments of Postural Loads

Demand functions for the four measurement inputs differed depending on the objective and constraint in the defined optimization problems. Cost-minimized demand functions for the four measurement inputs were derived in relation to the required precision (P):

$$n_s^* = \frac{k \cdot \hat{\sigma}_{bs} \cdot P^2}{\overline{c}_s^{1/2}}; \quad n_o^* = \frac{k \cdot \hat{\sigma}_{bo} \cdot P^2}{\overline{c}_o^{1/2}}; \quad n_r^* = \frac{\hat{\sigma}_{ws}}{\hat{\sigma}_{bs}} \cdot \frac{\overline{c}_s^{1/2}}{\overline{c}_r^{1/2}}; \quad n_a^* = \frac{k^{-1} \cdot \hat{\sigma}_{wo} \cdot \overline{c}_o^{1/2} \cdot \overline{c}_r^{1/2}}{P^2 \cdot \hat{\sigma}_{bo} \cdot \hat{\sigma}_{ws} \cdot \overline{c}_a^{1/2}}$$
$$[1]$$

and the precision-maximized demand functions for the four measurement inputs were derived in relation to the budget constraint (B):

$$n_s^* = \frac{\hat{\sigma}_{bs}}{\overline{c}_s^{1/2}} \cdot \frac{B}{k}; \quad n_o^* = \frac{\hat{\sigma}_{bo}}{\overline{c}_o^{1/2}} \cdot \frac{B}{k}; \quad n_r^* = \frac{\hat{\sigma}_{ws}}{\hat{\sigma}_{bs}} \cdot \frac{\overline{c}_s^{1/2}}{\overline{c}_r^{1/2}}; \quad n_a^* = \frac{k \cdot \hat{\sigma}_{wo} \cdot \overline{c}_o^{1/2} \cdot \overline{c}_r^{1/2}}{B \cdot \hat{\sigma}_{bo} \cdot \hat{\sigma}_{ws} \cdot \overline{c}_a^{1/2}},$$
$$[2]$$

where $k = \hat{\sigma}_{bs} \cdot \overline{c}_s^{1/2} + \hat{\sigma}_{ws} \cdot \overline{c}_r^{1/2} + \hat{\sigma}_{bo} \cdot \overline{c}_o^{1/2} + \hat{\sigma}_{wo} \cdot \overline{c}_a^{1/2}$.

Regardless of the optimization problem, the demand of each input in each stage of the statistical production had a positive relation with its variability but a negative relation with its cost. An increase in an input cost principally decreased its input demand, but at the same time could increase the input demand in another stage due to the substitution effects in the statistical pro-

duction. Further, an increase in the research budget when maximizing precision, or an increase in the required precision when minimizing cost, led to increasing the demands for subjects and observers but decreasing the demand for assessments, because of lower productivity and cost savings. The demand function for recordings per subject was not, however, affected by any changes in the research budget or in the required precision.

The measurement inputs in both approaches had inelastic demands, since the own-price elasticities of demand were less than one. This means that the input demands were not sensitive to changes in costs, and the optimized solutions were robust over slight cost-changing.

The cost function of the statistical production, which shows the minimum amount of cost necessary to achieve a predetermined level of precision of mean estimate, was derived as

$$C^{\min} = P^2 \cdot \left(\hat{\sigma}_{bs} \cdot \overline{c}_s^{1/2} + \hat{\sigma}_{bo} \cdot \overline{c}_o^{1/2} + \hat{\sigma}_{ws} \cdot \overline{c}_r^{1/2} + \hat{\sigma}_{wo} \cdot \overline{c}_a^{1/2} \right)^2 ,$$ while the maximized precision as a function of budget was

$$P^{\max} = \frac{B^{1/2}}{\hat{\sigma}_{bs} \cdot \overline{c}_s^{1/2} + \hat{\sigma}_{bo} \cdot \overline{c}_o^{1/2} + \hat{\sigma}_{ws} \cdot \overline{c}_r^{1/2} + \hat{\sigma}_{wo} \cdot \overline{c}_a^{1/2}} .$$ As expected for optimality, the (minimized) cost and (maximized) precision functions derived from either optimization problem showed a duality. The derived cost function was increasing in the precision and in all sources of cost and variability. Because the statistical production was characterized by decreasing returns to scale, the marginal cost of improving precision was larger than its average cost, which could result in diseconomies of scale.

The non-optimized measurement strategy applied in Paper II ($n_s = 4$; $n_o = 4$; $n_r = 1$; $n_a = 2$) for assessing working arm postures of hairdressers could be replaced either by the cost-minimized measurement strategy ($n_s = 6$; $n_o = 1$; $n_r = 1$; $n_a = 5$) in order to save 12% of resources, or by the precision-maximized measurement strategy ($n_s = 6$; $n_o = 2$; $n_r = 1$; $n_a = 2$) in order to improve the precision of the posture mean estimate by 7%. The precision yielded and the cost used by the optimized measurement strategies are given in Table 5.

Table 5: Precision and total cost of current non-optimized, cost-minimized, and precision maximized measurement strategies.

	Measurement strategy	$P(\hat{\mu})$	TC
Non-optimized	$n_s = 4$; $n_o = 4$; $n_r = 1$; $n_a = 2$	0.241	4392
Cost-minimized	$n_s = 6$; $n_o = 1$; $n_r = 1$; $n_a = 5$	0.245	3867
Precision-maximized	$n_s = 6$; $n_o = 2$; $n_r = 1$; $n_a = 2$	0.259	4200

Discussion

Assumptions regarding data distributions and *methods* for estimations

In the studies reviewed in Paper I and the data collection in Papers II–IV, cost and/or exposure data are assumed to be distributed normally. According to the *central limit theorem*, the costs and errors of sample mean assessments would approach a normal distribution with increasing sample size. However, sample sizes could not always be made sufficiently large. Exposure values may be assigned on the assumption that the data is log-normally distributed, and data analysis is then conducted on the natural logarithms of the original values. Empirical evidence indicates that some occupational exposures are quasi-log-normally distributed (Rappaport, 1991; Osvoll and Woldbæk, 1999). If the distribution of occupational exposure data tends to be log-normal, the ANOVA is usually fitted to log-transformed data (Loomis and Kromhout, 2004). Thus, $\bar{X} = \exp\ (\bar{X}_L + 0.5S_L^2)$ is the *maximum likelihood* (ML) estimate of μ. While both direct and ML estimates can be used at intermediate sample sizes $(20 \leq n \leq 50)$, the direct estimate of mean is more precise than the maximum likelihood estimate if the sample is small $(n < 20)$ (Rappaport, 1991). When the sample sizes are large $(n > 50)$, the maximum likelihood estimates of the mean and variance of a log-normal distribution, although slightly biased, are more precise. Hence, when assuming a log-normal model for occupational exposures, the mean and variance are not independent as in the case with the normal distribution. The assumption about exposure data distributions should also be investigated for each particular exposure, for example working arm postures.

Regarding assumptions about distribution of cost data, one should note that non-zero cost observations are typically truncated and skewed to the right (i.e. toward higher costs), and their distribution might be approximated by a log-normal distribution. Thus, the log-transformed data could approximate a normal distribution. The standard estimation of sample mean, which was used for cost assessments in the cost efficiency studies, then had the largest MSE because of the distribution of cost data. The ML estimator is usually applied when skewness is not high; otherwise, a conditionally minimal MSE estimator is recommended (Zhou). Mathematical methods such as non-parametric estimation methods (Cooper et al, 2003), Bayesian statistics

(Lambert et al, 2008), and regression analysis (Lin, 2003) have been developed to estimate costs in the presence of uncertainty and incompleteness of data. However, these techniques cannot be applied in situations with very small sample sizes, as in Paper II (particularly around salaries).

Collecting biomechanical exposure data at work is a much easier task than transforming it into useful information on the exposure variable. A large quantity of exposure data in the empirical exposure assessment studies does not necessarily translate to sufficient information on the exposure observed. Average measures and variance components were used in the cost efficiency studies to transform exposure data into useful information. There are three classical *Pythagorean* means: the arithmetic mean, $A = \dfrac{1}{n}\sum\limits_{i=1}^{n} x_i$, the geometric mean, $G = \left(\prod\limits_{i=1}^{n} x_i\right)^{\frac{1}{n}}$, and the harmonic mean, $H = \dfrac{n}{\sum\limits_{i=1}^{n}\dfrac{1}{x_i}}$. The arithmetic mean was widely used in the studies reviewed in Paper I, and was also used in Papers II–IV. Generally, the arithmetic mean, which is usually simply called the mean, is the most useful measure of central tendency in statistical analysis, even when the data tends to be skewed, as here. Aside from its ease of use and comprehension, another reason for its usefulness is that it has the lowest variance and therefore a higher estimated precision. However, in order to obtain an unbiased arithmetic mean and variance estimate, the sample estimates should be randomly selected and approximately normally distributed.

The three means are ordered as $A > G > H$, and the special case $A = G = H$ will occur if and only if all the observations of an exposure or an input cost have an equal value; that is, $x_1 = x_2 = \cdots = x_n$. Geometric and harmonic means could also be applied, since the variables of interest (exposures and input costs) had positive values. The harmonic mean is appropriate for situations when the average of *rates* is wanted, such as the cost of shares purchased each month. In comparison to the arithmetic mean, the harmonic mean tends more toward the lowest elements of the dataset. The *log-average* measure, which is simply the arithmetic mean of the log-transformed values of the measurement data, could also be used as a measure of average costs.

In addition to the commonly used indicators of *average* or central tendency, the concept of the *median* could also be used; this identifies the value which lies at the centre of the measurements when they are ordered by size. This average measure is the most appropriate to use for observations (such as cost and exposure data) that are skewed. However, although it is recommended to use the median instead of the mean as a measure of central location in

skewed data, means were used for all applications in the studies reviewed in Paper I as well as Papers II–IV. This was often reasonable for cost assessments, because the median could not be used to recover the total cost in each stage of the statistical production; analysis and prediction of costs require explicit functions, which cannot be provided by the median. However, there is no reason that the median could not be applied for exposure assessments.

While the estimation methods for variance components in the studies reviewed in Paper I were unclear, all variance components used in Papers II–IV were estimated by the *restricted maximum likelihood* method. Other estimators for assessing variance components do exist, such as *fitting constants*, but there is no widely-accepted "best" estimator for variance (Samuels et al, 1985).

A computer-based *bootstrapping technique* could be applied to achieve unbiased estimates of both the means and the standard errors of the means. The non-parametric bootstrap method is carried out in three steps: 1) re-sampling (with replacement) from the original sample and calculation of the bootstrap mean and variance, 2) repeating step 1 at least 1000 times in order to construct an empirical distribution of estimated parameters, and 3) calculating the average of all bootstrap means and variances (Efron and Tibshirani, 1986). The advantage of this empirical method is that it does not require any parametric assumptions concerning the underlying distribution of exposure and cost data.

To summarize, the result of the cost efficiency analyses could be affected by the assumptions about data distribution on exposures and input costs, and also by the methods applied in the relevant studies for assessing means and variances. However, not all of the cost efficiency studies had paid enough attention to this possibility. Each specific cost efficiency study should consider which types of estimators would be most appropriate for the data, and should particularly examine the assumptions they rely on.

Additional systematic sources of error

In Paper II, the measurement bias produced by the compared assessment techniques was assessed using inclinometer data, and attempts were made to eliminate misspecification bias by considering inter- and intra-observer variance. However, there are two other important sources of systematic error that were not considered either in Paper II or in the cost efficiency studies reviewed in Paper I. The two sources of error, which are important to consider in comparison of non-optimal measurement designs, are:

1) *Frame error*: Frame error or *selection bias* occurs when subject-sampling is performed *purposively* (rather than randomly with equal selection probability), as with the "convenience sample" used in Paper II. A randomized sample is *representative* of the underlying occupational group, and provides the possibility to improve the precision of the mean exposure estimate with no worry about increasing selection bias. Without randomization, the sample mean exposure is not an unbiased estimator of the group mean exposure. When sampling subjects purposively, the sample mean is in fact an estimator of the *frame* population mean. In cases where frame error exists, the occupational group mean is really a weighted mean of the covered (on the frame) and non-covered (outside of the frame) population. The amount of non-coverage bias (frame error) is a product of the non-coverage rate and the difference between covered and non-covered means in the occupational group.

2) *Non-response error*: Non-response error can appear when some important events on the exposure variable fail to be measured. The absence of subjects, refusals, instrument failure, incomplete response due to incorrect monitoring, too short a measurement time, and missing the most important periods of working time are some examples that could cause missed information about work-related biomechanical exposures. This type of systematic error is an indicator not only of statistical non-performance but also of economic non-performance, when some statistical resources are wasted or ineffectively used. The estimated mean in cases where non-response bias exists is a weighted average of the estimated response and non-response means. The amount of non-response bias is a product of the non-response rate and the difference between response and non-response sample means.

If these sources of systematic error had in fact been present in the posture assessment study in Paper II, our estimates of the total error produced by each technique would have been underestimates. Hence, the larger the contribution of these error sources, the greater our underestimation of the total error. This underestimation, in turn, could lead to overestimation of the ability of the assessment techniques to produce information at low cost, with the amount of overestimation potentially depending on the specific technique. The reliability of the results of cost efficiency comparison study in Paper II could thus be reduced if the frame and non-response errors were large.

The effect of errors on exposure value

Most of the studies reviewed in Paper I, like the studies in Papers II–IV, used a linear additive random effects model to describe exposure variation. The model for a three-stage sampling design to assess work-related biome-

chanical exposures is usually defined as $y_{sdq} = \mu + \varepsilon_s + \varepsilon_{sd} + \varepsilon_{sdq}$, where μ is the group mean exposure, and $\varepsilon_s, \varepsilon_{sd}$, and ε_{sdq} are the errors caused by subjects, days, and quantum, respectively. All errors have zero mean and variances of σ_s^2, σ_d^2, and σ_q^2, respectively (Mathiassen et al, 2002). The group mean variance is thus the sum of the variance components at the three stages. However, the variance of sample mean estimate formulated as

$$Var(\hat{\mu}) = \frac{\hat{\sigma}_s^2}{n_s} + \frac{\hat{\sigma}_d^2}{n_s n_d} + \frac{\hat{\sigma}_q^2}{n_s n_d n_q}$$ is linear homogenous in the numbers of sam-

pling units n_s, n_d, n_q, which in economic terms represents constant returns to scale. As an alternative, multiplicative effects models defined as $y_{sdq} = E(y_{sdq}) \cdot \varepsilon_s \cdot \varepsilon_{sd} \cdot \varepsilon_{sdq}$ (Firth and Harris, 1991) for the same sampling design could be applied. Allowing the effects to have different rates, the model could be developed as $y_{sdq} = E(y_{sdq}) \cdot \varepsilon_s^\alpha \cdot \varepsilon_{sd}^\beta \cdot \varepsilon_{sdq}^\gamma$, which in logarithm would be expressed as

$\ln y_{sdq} = \ln E(y_{sdq}) + \alpha \cdot \ln \varepsilon_s + \beta \cdot \ln \varepsilon_{sd} + \gamma \cdot \ln \varepsilon_{sdq}$. While there is evidence that the effects are better represented as multiplicative than as additive in some other variables (ibid.), there is no evidence from working life examining whether the effects are additive and/or multiplicative. To summarize, cost efficiency studies should investigate more thoroughly whether assumptions concerning the effects of errors at different stages of sampling are met, since any deviation may jeopardize the results of cost efficiency evaluations of measurement designs.

Precision versus accuracy

The cost efficiency studies attempted to produce *estimates* of occupational group mean exposures at low cost. However, these estimates were often characterized by uncertainty and bias, and thus probably deviated from the *true* group mean exposures. Accuracy (unbiasedness) and precision (reproducibility) were thus two dimensions of statistical performance associated with the measurement designs compared in the cost efficiency studies. While a precise estimate of mean exposure, which is robust over replications, requires large sample sizes, an accurate exposure estimate, with a value close to the true exposure, calls for randomization of sampling, advanced technical methods, and highly competent investigators to record and analyse the exposure data. Thus, increased economic resources should allow for increasing both the precision and the accuracy of exposure assessments. However, because random error is cheaply and routinely measureable and reducible by increasing sample sizes, and is defined in an explicit model, cost efficiency

studies have often focused on this type of error alone. Exposure assessment studies have generally preferred precision over accuracy of estimates; not always because of the difficulty and expense of recruiting skilled labour and acquiring advanced technical equipment, but often because of difficulties and expenses inherent in the randomization of sampling and the assessment of accuracy. Thus, there has been a tendency among researchers in the field to assume "non-biased" estimates and then to attempt to improve precision. Theoretically, one can assume that no bias is involved in an exposure assessment study. In practice, however, even if we use advanced techniques and skilled labour, it is not realistic to believe that the bias can be eliminated completely. In addition, bias can appear not only in point estimates (mean exposure), but also in estimates of variance components, which affect the evaluation of precision and cost efficiency.

A confidence interval for a group mean exposure may have a much lower effective confidence level because of the effect of bias. The confidence interval constructed with a 95% confidence level is $\mu = \hat{\mu} \pm 1.96\sigma$, meaning that there is a 5% probability that the true mean exposure will diverge from its estimate by more than 1.96σ. Where bias exists, this probability increases, and the confidence interval changes to $\mu = \hat{\mu} \pm 1.96\sqrt{MSE(\hat{\mu})}$. Hence, the larger the bias, the greater the probability of divergence.

In Paper II, both the precision and the accuracy of the compared assessment techniques were considered in the economic decision, which is reasonable in the short-run case. Conversely, in Papers III and IV, no sources of systematic error were considered, as in the long-run case all the measurement inputs to be optimized were variable, and bias was assumed to be independent of these variable measurement inputs. However, when maximizing the precision of the mean estimate, while an increase in sample sizes can lead to reducing random error, it can unfortunately also lead to increasing some systematic errors (Biemer and Lyberg, 2003). Considering this, the large non-reducible systematic error (bias) in exposure assessments does not allow the researcher to optimally determine sample sizes in order to maximize the precision of a measurement design for a given research budget. More empirical research is needed to discover whether any type of bias is dependent on variable measurement inputs.

The choice between advanced measurement technique and large samples

One challenge of decision-making in Paper II was the choice between a simple assessment technique with a more comprehensive measurement strategy

and a more advanced technique with a resource-saving measurement strategy. The difficulty in decision-making could be eased by assessing the marginal effect of each measurement input on the precision (i.e. its marginal products) and the cost of reducing each source of error associated with the assessment techniques and measurement strategies. Generally, when inexpensive simple assessments of occupational exposures are selected over expensive advanced technical measurements, the resources saved by choosing the cheaper measurement method can then be used to recruit a more comprehensive measurement strategy. Investment in the measurement strategy, on the other hand, reduces the uncertainty (though at a declining rate) but not the bias, which is usually assumed to be independent of all sample sizes and may be quite large. Thus, once the sample sizes have reached a certain level, further increases can hardly reduce the total error any more. In addition, according to the results of Papers III and IV, the cost of improving precision is very high. Considering these facts, an increase in the research budget should now be used for employing more advanced measurement techniques in order to reduce bias. Compared to an expensive advanced technique, a cheaper measurement technique with a higher bias can be relatively cost-efficient, since the variance of the currently used measurement strategy is much higher than the bias of the cheap method. On the other hand, the expensive advanced technique, which leads to increased accuracy, may be the cost-efficient choice since the improvement in precision by increasing sample sizes, partially or entirely, is negligible and costly. The results of Paper II could be more applicable in resolving short-run economic decision problems if the marginal products of measurement inputs and the costs of increasing precision and accuracy were assessed. When choosing between an advanced technical method with small sample sizes and a simple subjective technique with large sample sizes, the bias ratio B/σ is thus an important tool in the decision-making. If the current budget increases, the bias ratio determines the direction in which the increased resources should be used; for recruitment of the advanced technical method, or increasing sample sizes. While the bias ratio is not an appropriate measure for statistical performance of the compared designs, because it does not consider absolute values of bias and uncertainty, it could be used as a decisive rule when allocating economic resources between measurement techniques and samplings in a measurement design. Usually, when using simple measurement techniques, a bias greater than half of the standard error caused by measurement strategy (sampling units) is considered to be large, and vice versa. If, however, the sampling units are sufficiently large, the bias ratio is required not to exceed 0.1 (Biemer and Lyberg, 2003).

Evaluation of measures for statistical performance

In the cost efficiency studies reviewed in Paper I, as in Papers II–IV, the statistical performance of an exposure measurement design was assessed with different measures such as sampling variance, standard error, validity coefficient, reliability, statistical power, and absolute error. When bias is expected in cost efficiency comparisons, as in Paper II, one should formulate the statistical performance including both variance and bias. If the objective of an exposure assessment is to estimate a parameter with some specified precision, as in Papers III and IV, then analysis of variance (ANOVA) is appropriate. The usual measures of the uncertainty of an estimate are sampling variance and standard error of sample mean. These concepts are used to evaluate the precision of an exposure assessment. Standard error is preferred to sampling variance, because it has the same unit of measurement as the exposure variable itself. In addition, standard error is the measure that is used in constructing the confidence interval. However, *coefficient of variation*, which gives the standard error as a proportion of the mean, could give more information about the precision of a measurement design. The value of the standard error of the mean yielded by an exposure assessment design often does not give sufficient information for evaluating the precision of an exposure measurement design, unless it is compared with the value of the estimated mean exposure. If the size of the standard error is small relative to the size of the mean exposure estimate, the precision could be acceptable. Conversely, if the standard error is large relative to the size of the mean exposure estimate, the precision of the measurement design may be unacceptable. As the coefficient of variation lacks any unit of measurement, it could be used to estimate the efficient cost of an exposure assessment design.

If the precision of the mean estimate is measured by the inverse of the sampling variance, the statistical production shows constant returns to scale, as in the studies by Stram et al (1995), Whitmore et al (2005), and Mathiassen and Bolin (2011). In this case, the marginal cost of improving precision is equal to its average cost. If, however, the precision is measured by the inverse of the standard error of mean, as in Papers II–IV, the statistical production shows decreasing returns to scale. In this case, the marginal cost of improving precision exceeds its average cost. The precision of an exposure measurement design has also been evaluated by the stability of intraclass correlations (Donner and Eliasziw, 1987; Shoukri et al, 2003). In a two-stage sampling strategy, for instance, the intraclass correlation within subjects is defined as the proportion of the total exposure variation, $\rho_{ws} = \dfrac{\sigma_{bs}^2}{\sigma_{bs}^2 + \sigma_{ws}^2}$,

where σ_{bs}^2 and σ_{ws}^2 are between- and within-subject variances, respectively. The variance of intraclass correlation can then be estimated as

$$Var(\rho) = \frac{MSB - MSW}{MSB + (n_s - 1)MSW}$$, where MSB and MSW are between-subject
and within-subject mean square, respectively. Like the coefficient of varia-
tion, this measure lacks any measurement unit; it could also be used for cal-
culating the efficient cost of the alternative designs in Paper II.

When comparing the cost efficiency of alternative measurement designs, the
appropriate measure of statistical performance is the absolute error estimated
by the root of mean square error, as employed in Paper II. Assuming no bi-
ases for the alternatives in exposure assessments, the design that produces
less random error at the same cost is the appropriate design to implement.
However, where bias exists, ignorance of all sources of systematic errors
(biases) can lead to the identification of the *wrong* measurement design. The
results of Paper II reinforced this statement; when the measurement bias was
ignored in the model, the bias-exposed assessment technique WS15 was
identified as cost-efficient for the most cases. Precision represents only one
dimension of statistical performance in comparing non-optimal measurement
designs in a short-run economic decision (i.e. where some measurement
inputs are fixed). The root of $MSE(\hat{\mu})$ includes all types of error produced
during exposure assessment, and is therefore the most appropriate measure
for evaluating the informative value of statistical products in this case. By
selecting the design that produces less mean square error in relation to the
alternatives, researchers aim to improve both precision and accuracy (i.e. the
amount of information produced).

Bias was assumed to be independent of the variable measurement inputs in
the optimization-based studies in Papers III and IV. Thus, bias is not usually
considered when determining the quantities of variable measurement inputs
(i.e. in long-run economic decisions). In this approach, not only precision but
also (with some difficulties) reliability and statistical power could be em-
ployed for assessing the performance of statistical production. However, the
possibility of using reliability and power was not investigated and examined
in Papers III and IV. Discussion about the choice between precision and
power/reliability in sampling optimization is still ongoing. While minimizing
the random error is an important objective in biomechanical exposure as-
sessment studies, most data analysts are interested in maximizing the power
of their hypothesis tests (Cohen, 2005). The statistical performance of an
exposure measurement design has been measured by its statistical power
(Spiegelman and Gray, 1991; Spiegelman, 1994). For a power calculation,
we need to know parameters α and β, which denote respectively the proba-
bilities of type *I* error (null hypothesis, H_0, is rejected when it is, in fact, true)
and type *II* error (H_0 is accepted when alternative hypothesis, H_a, is, in fact,
true). The power of a statistical test, or the probability that the test would
lead to rejection of the null hypothesis that was false, is then $1 - \beta$.

In principle, the smaller the variance, the greater the power, so the problems are equivalent. However, the relationship between variance and power is nonlinear, so is could be difficult to determine sample sizes that would yield a particular power for a fixed significance level (Cohen, 2005). On the other hand, sample size determination based on the expected confidence interval width could be misleading if the expected intervals are not "centred" with respect to the specified values (Greenland, 1988).

Output of an exposure assessment study

The output of each exposure assessment in the cost efficiency studies reviewed in Paper I and the studies reported in Papers II–IV was the quantity of information on the exposure. The absolute error of the mean, $AE(\hat{\mu}) = |\hat{\mu} - \mu| = \sqrt{MSE(\hat{\mu})}$, was used in Paper II to assess the quantity of information produced by alternative assessment techniques. However, there is no method to perfectly calculate the absolute error, since the *true exposure* is not known. The estimation of the quantity of information produced can thus itself be exposed to a large error, while the quantity cannot be compared with the cost of producing it as they are measured by different measurement units. The standard error, $SE(\hat{\mu}) = \sqrt{Var(\hat{\mu})}$, was employed in Papers III and IV to assess the quantity of information produced by the applied measurement designs. However, the statistical production could not be economically evaluated since the marginal benefit of producing information was unknown. The quantity of information produced by a measurement design, expressed in terms of precision and/or accuracy, is not the only output of an exposure assessment study. In addition to the quantitative criteria, there are qualitative criteria such as *relevancy*, *timeliness*, *consistency*, *accessibility*, *comparability*, and *completeness* (Biemer and Lyberg, 2003) for evaluating the *usefulness* and social *benefits* of the information produced on different exposure variables. Although both the quantity and quality of information produced on biomechanical exposure are important for assessing the *value of information* (VOI) produced in exposure assessment studies, the cost efficiency studies reviewed in Paper I and described in Papers II–IV were concerned only with the *quantity* of information produced. The VOI can be applied to assess the output of assessment studies on work-related biomechanical exposures in terms of money; that is, in the same terms in which the cost of producing information on the exposures is assessed. In health economic analysis, the benefits of a health care intervention in terms of both quantity and quality of life are assessed by "willingness to pay" (Goossens et al, 1999; McIntosh et al., 2010). The quantity and quality of information produced about an exposure of interest can also be assessed in monetary terms, based on the same approach. The information produced on an exposure to

musculoskeletal disorders can be used in providing an appropriate prevention program. Assuming the total social cost caused by the exposure will be clearly reduced by applying the prevention program, the information produced by the exposure assessment study can easily be valued in terms of money. All the positive results of the prevention program, such as reduction of sickness absences across the occupational group, and increasing of work quality and productivity, are benefits to society that are usually expressed in monetary terms. The VOI is the amount of money a decision-maker would be willing to pay for information about an occupational exposure of interest, prior to making an economic decision about producing the information. Statistical efficiency (precision and/or accuracy) is only one factor determining the amount of money a decision-maker would be willing to pay for the information; the value of the information produced is also determined by the *usefulness* of the information in further research and the expected social *benefits* of using the information.

Input costs in short-run and long-run production

The reliability of the results of the studies described in Papers I–IV could be improved if more attention was paid to model construction and parameter estimation regarding input costs. Only by correctly formulating the total input costs that have been used to produce the information can the resources be optimally allocated and the cost-efficient measurement design identified.

The curvature of cost developments: The cost efficiency studies did not investigate how the input costs developed as a function of the measurement inputs. This development depends on the type of exposure assessment study. If the exposure assessment study is carried out in the short run, as in Paper II, the *curvature* of the variable input costs development should be investigated in order to estimate and predict costs correctly. Assessment of labour costs should be based on this investigation, since labour productivity growth is usually fast in the beginning of the statistical production. The average variable costs in the short run then develop non-linearly with the quantity of measurement inputs. Thus, the cost efficiency comparison in Paper II, as a short-run economic decision, should not ignore the development of costs. The cost model used in Paper II should allow the variable costs to vary non-linearly with the number of measurement inputs.

However, if the exposure assessment study is carried out in the long run, *stable* average variable costs with linear characteristics as a result of stable labour productivity should be considered, as in Papers III and IV.

The user cost of capital: In Papers II–IV, the opportunity cost associated with physical capital was calculated using the forgone interest or financial

return. However, a more accurate method would reflect an investment with similar risk, and include a risk premium according to the *Capital Asset Pricing Model* $r = r_f + \beta(r_m - r_f)$, where r_f is the risk-free rate of interest or expected return of the risk-free rate, r_m is the expected return on the stock market, and β measures the asset's non-diversifiable risk. It is also worth mentioning that $r_m - r_f$ is the risk premium on the market.

Which costs matter? In Paper II, all fixed and variable costs were assessed, while in Papers III and IV only the costs associated with variable measurement inputs were considered. In principle, when identifying measurement inputs and selecting a model for calculating their costs, the type of statistical production (i.e. whether the exposure assessment study is performed in the short or the long run), the methodology of the cost efficiency analysis (i.e. how an economic decision is made in exposure assessments), and the output of the statistical production (the amount of exposure data recorded, precision, accuracy, etc.) and its specific definition (i.e. error equation) are considered. In short-run economic decisions on non-optimal measurement designs, as in Paper II, fixed costs should be assessed because at least one measurement input will remain unchanged in the exposure assessment study. In optimizing the variable measurement inputs, however, only costs related to the variable measurement inputs to be optimized will be considered in the isocost line equations, because no input remains unchanged during the study. Assessing exposures in the long run is thus the usual underlying assumption in optimization-based studies. In these studies, as in Papers III and IV, the isocost equation should be referred to the same variables that are shared by the error equation. The input costs considered in the isocost equation must thus follow the measurement inputs defined in the statistical model.

Important issues regarding *elasticities* in economic decision-making

In Paper IV, own-price elasticities of demand for four measurement inputs were estimated as an important tool in making a decision on the quantity of the measurement inputs. The demand for a measurement input may also change as a result of a change in the price of another measurement input. This occurrence is called *cross-price elasticity* of demand, which measures the percentage change in demand for an input as the price of another input changes by one percent. The two measurement inputs are substitutes if the cross-price elasticity is positive, and are complementary to each other if it is negative. The estimation of cross-price elasticities of demand for the measurement inputs optimized in Paper IV could thus be used for further economic analysis.

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Another important issue of elasticity, which could be assessed in Paper IV for advanced economic analysis, is the *elasticity of substitution*. Elasticity of substitution between any two measurement inputs, σ_{ij}, measures the percentage change in the ratio of inputs resulting from one percentage change in the corresponding price ratio. It is usually introduced as a measure of the relationship between the *technical rate of substitution* (TRS) and the input ratio. A positive value of σ_{ij} indicates that inputs *i* and *j* are substitutes, while a negative value of σ_{ij} indicates complementary inputs. The greater the elasticity of substitution, the greater will be the input substitution effects of changes in input prices; $\sigma_{ij} > 0.5$ indicates strict substitutability between the two inputs. There are thus both substitution and complementation properties in the *composited* measurement inputs to produce information on any biomechanical exposure at work. The degree of complementarity and substitutability are different between different measurement inputs and in different situations. For instance, measurement instruments and data collectors are strictly complementary inputs in producing information on an exposure when the measurement instrument and the skill of data collectors cannot be developed. However, they can also be substitute inputs to the statistical production when they are characterized by development opportunity. As a contrary example, in Paper III, direct technical measurements and indirect subjective estimates were assumed to be strict substitutes in exposure assessment. However, they could be complementary inputs when the information produced by each technique was not complete for further exposure analysis. The shape of isoquant curves is therefore used for mapping the TRS (i.e. the slope of isoquant curve) and then making decision about the quantity of any two measurement inputs. The TRS gives information about the amount of a measurement input that a researcher is willing to give up in order to employ one additional unit of another input for producing information on an exposure. Hence, the TRS decreases as we move down the isoquant curve until it reaches and even falls below unity (cf. Figure 1 in the Appendix).

Finally, the *elasticity of cost* and the *elasticity of marginal cost* with respect to one input price could also be used to evaluate the responsiveness of the minimized or marginal cost towards a change in an input price.

The *simple* and *obscure* cost-output relationship

Simplicity: The simple additive effects model, which describes the input-output relationship and is used in Papers II–IV and the cost efficiency studies reviewed in Paper I, is the cause of *simplicity* in the cost-output relationship. The specifications of the cost-output relationship derived from the op-

timization analysis are determined by the specifications of the input-output relationship considered. A major reason to employ the traditional model in assessing the precision of the data produced in the optimization-based cost efficiency studies is the practical need for closed solutions for optimizing resource allocation. However, as the isocost line equation had to be constructed according to the traditional statistical model, the (minimized) cost function was also affected by the property of the model. If the precision is defined as the inverse of the mean variance, as in the optimization-based studies reviewed in Paper I, the statistical production exhibits constant returns to scale; the marginal cost is equal to the average cost in each level of production. The cost-output association in this case is modelled as $C = \alpha + \beta \cdot P$. If, however, the precision is defined as the inverse of the standard error of mean, as in Papers II–IV, the statistical production shows decreasing returns to scale; the marginal cost is twice the average cost. The cost-output relation in this case is best modelled as $C = \alpha + \beta_1 \cdot P + \beta_2 \cdot P^2$ (cf. Paper III). If neither the average cost nor the marginal cost curve are allowed to be linear, a cubic function such as $C = \alpha + \beta \cdot P + \beta_2 \cdot P^2 + \beta_3 \cdot P^3$ could be used for illustrating the cost-output relationship. The cubic function was not used in Paper III because the marginal cost of precision was not assessed to vary non-linearly. However, in principle, estimation of the cubic function is a statistical test of linearity in marginal cost. The simplicity in the cost-output relationship will still remain in economic evaluations of measurement designs as long as the linear input-output relationship is assumed in the traditional statistical model.

Obscurity: As discussed in Papers III and IV, since the information produced on work-related biomechanical exposures is not yet economically valued as a function of precision, the derived cost function cannot be used to draw any firm conclusions about economies/diseconomies of scale. The invaluable precision of the mean estimate, considered as output, was the reason to have an *obscure* cost-output relationship in the optimization studies. The obscurity led to difficulties in decision-making for any improvement of precision, where the economic value of additional precision was not known and could not therefore be compared with its marginal cost.

To solve both problems, simplicity and obscurity, the cost-output relationship should be analysed by first evaluating the output economically, as a "benefit" in the health economic literature (McIntosh et al., 2010), and then using appropriate cost functions developed in production economics (Chambers, 1994). The standard (logarithmic) cost function usually takes the following functional form:

$$\ln C = \alpha + \gamma \ln Y + \sum_{i=1}^{n} \beta_i \ln \overline{c}_i + \varepsilon, \qquad\qquad [1]$$

where Y denotes the value of information produced; α, β_i, and γ are regression coefficients; and ε is a disturbance term. For minimizing the total cost of an exposure assessment study using the standard cost function, the output of the statistical production should be valued in the same terms as the total cost (i.e. in terms of money), for a complete economic analysis. The simple logarithmic cost function [1] for statistical production, however, constrains the elasticity of substitution between any two measurement inputs to be equal to unity. To avoid the restriction, econometricians suggest using the *transcendental logarithmic cost function* (TLCF) which is based on a second-order Taylor's series approximation theorem in logarithms (Berndt, 1996; Bantekas, 2008). The transcendental logarithmic cost function, which is simply called the *translog* cost function, has also been applied in health economic studies to explore associations between cost inefficiency and hospital health outcomes (Mckay and Deily, 2008). The non technological progress of a general specification of translog cost function is expressed as:

$$\ln C = \alpha + \sum_{i=1}^{n} \beta_i \cdot \ln \overline{c_i} + \frac{1}{2} \sum_{i=1}^{n} \sum_{j=1}^{n} \beta_{ij} \cdot \ln \overline{c_i} \cdot \ln \overline{c_j}$$
$$+ \gamma_Y \cdot \ln Y + \frac{1}{2} \gamma_{YY} \cdot \left(\ln Y \right)^2 + \sum_{i=1}^{n} \delta_{iY} \cdot \ln \overline{c_i} \cdot \ln Y + \varepsilon \quad ,$$

[2]

where i and j are measurement inputs.

The standard economic analysis, however, requires a time-series, cross-sectional, or pooled empirical dataset on costs and the value of information. The required datasets can thus be obtained in exposure assessment studies across *time* and *space*. If the working arm postures of hairdressers were assessed regularly at certain intervals of time and/or in a wide range of workplaces at the country/province level, the standard cost functions could be applied in Papers II–IV.

Uncertainties in the cost efficiency analyses

Costs and benefits of remedying the uncertainties associated with data collection: The methodologies applied in Paper II and other studies comparing the cost efficiency of alternative designs required that all relevant quantities (error and cost components) were accessible. Thus, for reducing uncertainty in the results of the cost efficiency studies, the required quantities should be efficiently estimated. More information might thus be acquired for making the *right* decision about implementation of a suggested measurement design in Paper II. The *expected value of perfect information* (EVPI) approach could be used to estimate the value of obtaining further information to reduce this uncertainty. The expected cost of the uncertainty would be determined by the probability that a decision was *wrong* and the size of the

opportunity cost if the wrong decision was made, since perfect information would eliminate the possibility of making the wrong decision. When deciding whether to produce additional information, the marginal benefit and the marginal cost of exposure assessments should be estimated. We had to estimate the *benefits* to society of further information, or equivalently, the *opportunity cost* of not using the appropriate measurement design. The EVPI is the difference between the expected benefits to society provided by using perfect information on quantities of costs and errors, and the expected social costs when using error-exposed estimates. In other words, the total *EVPI* is the difference between the expected benefits of the optimal decision the society would make under *certainty* and the expected costs of wrong decisions the society would make under *uncertainty*.

Limitation in optimizing measurement designs: The measurement design to be optimized in Papers III and IV is assumed to be performed in a situation where all input costs and errors vary with the quantity of measurement inputs. This is the usual underlying assumption in optimization-based studies; that is, it is usually assumed that the exposures are assessed in the long run. In practice, only the known types of cost and error that are related to the measurement inputs to be optimized will be considered in the isocost line and error equations. Further, the isocost equation will be referred to the same measurement inputs that are shared by the (random) error equation. The structure of the isocost equation will thus follow the structure of the statistical model instead of the economic principles. Finally, the optimized solutions are conditional on the functional forms of the isocost and error equations being correct.

Limitations of the methodologies in comparing non-optimal designs: In Paper II, the comparison of alternative measurement designs differing in economic and statistical performance revealed that labour-intensive measurement designs often produced more information on working arm postures than the labour-saving designs, at an incremental cost. A research question in similar situations is whether the incremental information justifies the incremental cost. Economic decision theory suggests that the cost of producing additional information would be compared with the opportunity cost (expected social cost) of using the *inferior* but labour-saving measurement design that produces insufficient information on the exposure, or equivalently, with the expected social benefit of employing the *superior* but labour-intensive measurement design. However, the value of information on the working arm postures was unknown, and so there was no reliable criterion to use when making economic decisions around production of additional information. In the absence of a reliable criterion, there was an obscurity in the definition of the cost efficiency comparison of non-optimal measurement designs. The definition was careful to include the criterion that the appropriate measurement design to employ was the one that produces more infor-

mation on the exposure relative to the cost of achieving it. However, the analytical tools applied in the comparison analysis (i.e. RCE in Paper II and MCBR in Paper III) do not provide the same kind of evidence on technical and productive efficiencies that would be available if using optimization analysis. Only under assumption of the same returns to scale for alternative measurement designs will the analytical tools be able to identify the relatively most cost-efficient design.

Uncertainties in cost efficiency measures: The analytical tools introduced in this thesis for comparing the cost efficiency of non-optimal measurement designs do not provide sufficient information for decision-making. Ambiguity arose with the relative cost efficiency measure (RCE), as applied in Paper II, as the value it produced was only an *overall* performance measure of the alternative measurement design compared to the reference. Although the information from the RCE value was decomposed into relative statistical and economic performances in Paper II, it did not include the cost of improving statistical performance. The additional cost of using a superior measurement design in statistical performance could not therefore be justified by the error reduction opportunity. The MCBR measures applied in Paper III would give the decision-maker a foundation to resolve this problem. However, ambiguity also arose with this measure, since it could not give decision-makers sufficient information without a specified maximal justified investment for improving statistical performance. According to the application of the MCBR measure in Paper III, the design including more direct measurements produced more information for the resources it used (and therefore was appropriate to implement) if the value was below the maximum justified investment for reducing the amount of error by one unit. However, when the benefit of additional information is unknown, the amount of the investment can only be determined *subjectively*. Thus, the measure does not provide a final decision rule, since the social benefit of an increase in the amount of information produced on a work-related biomechanical exposure is not known. The researchers in the field can, instead, formulate a decision rule that indicates whether an increase in the amount of the information produced may be funded; that is, they can decide how much they are able or willing to invest for an improvement in statistical efficiency. The decision over which design is appropriate to implement will then be made when the estimated value of MCBR compares with the maximal ability/willingness to invest for an increase in the quantity of the information produced by one unit. Generally, the resource-intensive measurement design with a higher ability to produce information is the cost-efficient design compared to the other design for a MCBR value less than the maximal investment.

To sum up, for a more comprehensive economic evaluation of alternative non-optimal measurement designs, both approaches in cost efficiency comparison, RCE and MCBR, should be applied. The cost efficiency measures

alone give only partial assistance for decision-making about non-optimal measurement designs.

Current *position* and *recommendations* for future research

Current position: There is still a need for further criticism of the models used in making economic decisions about measurement designs, since all the above-discussed *limitations* and *insufficiencies* associated with the exposure assessment studies still remain. Extensive data on input costs over time and workplaces is not available, since assessment studies on biomechanical exposures are often carried out over a limited time at a specific and predetermined workplace. Because of the practical limitations, researchers are forced to simplify input-output relationships and introduce strong assumptions in order to derive the required closed-form solutions. However, the derived cost functions, which convey cost-output relationships in the statistical production, will be simplified if the error equations, which show input-output relationships in the statistical production, are simplified. Moreover, the social cost of an exposure of interest is not yet economically modelled, and thus the marginal benefit of producing additional information on the exposure cannot be determined in order to be compared with the corresponding marginal cost. There are other limitations that usually appear in practicing the derived solutions for allocating resources between different measurement inputs. Firstly, in addition to statistical and economic considerations, there are often practical constraints and feasible alternative designs considered in selection of a measurement design. These considerations are not usually displayed in the applied methodologies. The cost-efficient measurement design provided by the methodologies may thus be infeasible due to *exogenous factors* such as practical and technological constraints and/or required facilities that are not reflected in the model and are thus determined out of the model. Secondly, it may be possible to produce more percentile information on a biomechanical exposure at work by increasing a particular percentile in the measurement inputs, which exhibits increasing returns to scale for the exposure assessment study. However, this possibility, which is an economic incentive for expansion of the statistical production, is not included in the available error equations applied in the relevant studies. When the precision is formulated as the inverse of the mean variance, the statistical production shows constant returns to scale (cf. Stram et al, 1995; Whitmore et al, 2005; Mathiassen and Bolin, 2011); however, when the precision is formulated as the inverse of the standard error, the statistical production shows decreasing returns to scale (cf. Papers II–IV). Finally, the variables to be optimized in an exposure measurement design (measurement inputs in the statistical production) may be discrete rather than continuous, hence not, in principle, differentiable.

However, in order to derive the optimized solutions, they are assumed to be continuous, and then the Lagrangian function is differentiated. At the end of optimization, when the derived optimal values are adjusted to the nearest integers, the constraint is violated (cf. Papers III and IV). The problem can be resolved by considering the constraint in the adjustments, as in Paper IV. However, the new optimized solution may not differ significantly from the current design in terms of cost efficiency.

Final recommendations:
- Parametric assumptions concerning the underlying distribution of the exposure to be measured, and related cost data, should be investigated in order to apply appropriate estimation methods.
- Assumptions concerning the effects of errors at different stages of sampling should also be investigated. This will allow the application of appropriate effects models for estimating the total error produced in each exposure assessment study.
- In short-run economic decisions, when alternative measurement designs are compared, all sources of costs and errors should be considered and estimated, as the curvature of cost developments.
- The marginal cost and marginal product of measurement inputs should be estimated in order to provide a sound basis on which to make economic decisions.
- The social benefit of an exposure assessment study should be evaluated, to allow an assessment of the value of information produced by employing a specific measurement design.
- Instead of sampling units in each stage, the quantity of capital input (buildings, measurement instruments, and other equipment), labour inputs (data collectors and data analysts), and other inputs to the statistical production should be optimized in long-run economic decisions.

Conclusions

Efficient allocation of resources in assessing biomechanical exposures at work requires appropriate statistical and economical models in both comparison and optimization of measurement designs.

- When comparing the cost efficiency of alternative designs, variations in input costs and all sources of costs and statistical errors associated with the compared designs should be considered, and the cost of improving statistical performance should be estimated. In optimizing measurement designs, the optimized solutions should be further refined with comparative static analyses, cost functions, and economic interpretations.

- Labour-intensity in exposure assessments is an important determinant in cost efficiency comparisons. The labour cost of improving the statistical performance in exposure assessments can thus be very high.

- An increase in the fraction of expensive direct measurements can reduce the total cost of achieving a required precision. Hence, the cost of a measurement design is not the determining factor in cost efficiency analysis; the decisive factor is the cost of achieving a required precision and the cost of improving precision by using the design.

- Although the overall performance of the optimized measurement strategy can be reduced after any necessary adjustment, either the scarce resources can be saved or the precision of the group mean exposure estimate can be improved through an optimization.

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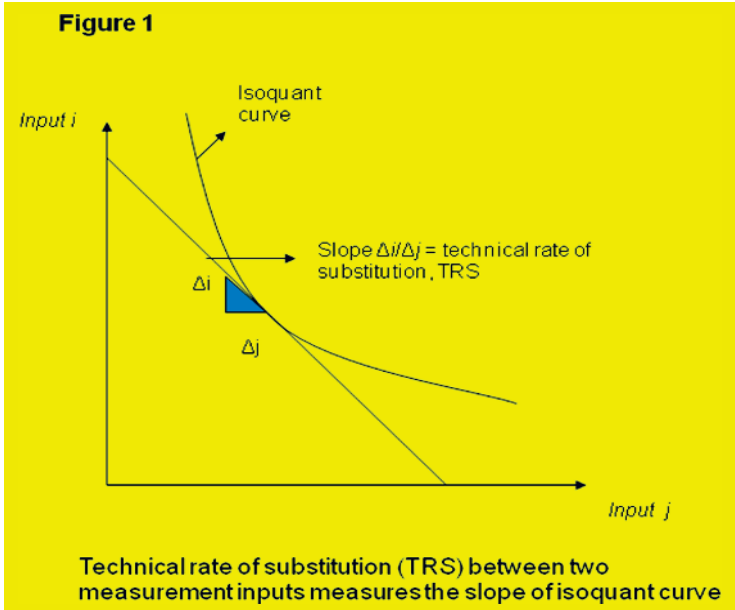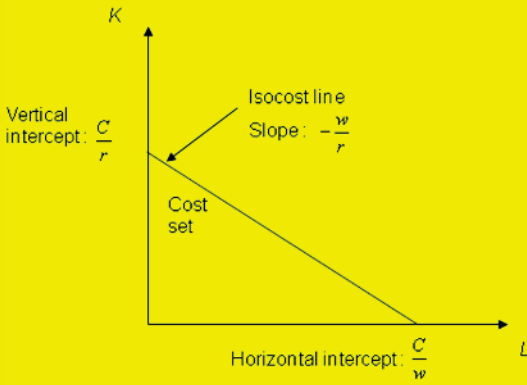
Appendix

Figure 1

Input i

Isoquant curve

Slope Δi/Δj = technical rate of substitution, TRS

Δi

Δj

Input j

Technical rate of substitution (TRS) between two measurement inputs measures the slope of isoquant curve

Figure 2

K

Vertical intercept: $\frac{C}{r}$

Isocost line

Slope: $-\frac{w}{r}$

Cost set

Horizontal intercept: $\frac{C}{w}$

L

Cost constraint of a measurement design: the cost set consist of all combinations of capital and labour that are affordable at the given total cost

Figure 3

C

MC

AC = MC
=> CRS

AC

IRS

DRS

Y
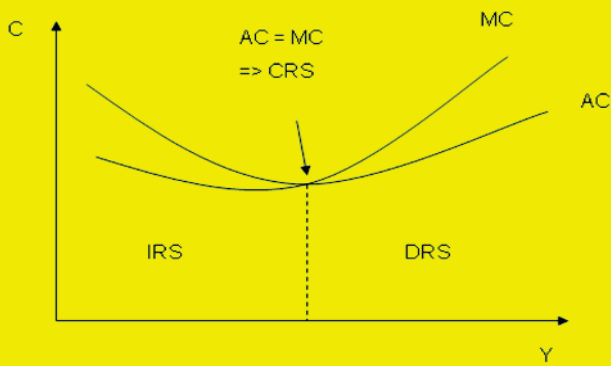
Long-run average and marginal cost curves: differences between average cost and marginal cost determine the kinds of returns to scale

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