V Conclusions

1.1 Results

It was shown in Section (IV2.1) that, if effort is not contractible, compensation shall be made contingent on output. It was also shown that, in this case, welfare levels can never be higher than in the case of observable effort. Barring the unrealistic cases of a deterministic production function and risk neutrality of both the principal and the agent, the optimal compensation scheme leads to a welfare loss due to imperfect risk sharing.

Section (IV2.2) derives a closed-form solution which negatively links the use of variable fee contracts to the agent’s level of risk aversion, the level of project risk and the convexity of the disutility function. The weakness of this model is its lack of generality and its unrealistic assumptions as e.g. the constant absolute risk averseness of the agent’s preferences. Especially the linear sharing rule seems to be completely arbitrary.

So far, it has been argued that variable fees may provide useful incentives in situations of hidden action, but also create imperfect risk sharing. Quite apart from any other consideration, Section (IV2.3) looks closer at the mechanics of risk sharing: In the traditional agency models, the principal is always assumed to be risk-neutral, while the agent is assumed to be risk-averse or risk-neutral. This need not be the case. The assumption that the principal is risk-neutral is often justified by the argument that he is the economically more potent party to the contract. This is largely inspired by the traditional story behind principal-agency models referring to the relationship between a company and its employees. There are several arguments why the economically more potent party should be less risk-averse: First, it is often plausible to assume that as a person becomes wealthier his absolute level of risk aversion decreases. Second, if the company is held by an entrepreneur he might be the less risk-averse type of person in the first place. In addition, employees usually only work for one company, while the owner might hold many different companies. So, he is probably better diversified. This last point is especially true for publicly held companies. However, ut in the case of a client and his consultant, things can be different. The small consultant partnership is clearly more risk-averse than its multinational client, but this changes if the big international consultancy firm provides services to a small start-up company through its incubator branch. Clearly, in the setting analysed so far - the case where, say, the principal is risk-averse and the agent risk-neutral - does not appear to be problematic as optimal incentive provision and optimal risk sharing are compatible. But what if there is a bilateral moral hazard problem? To answer this and other questions, one has to look at risk sharing in its own right. Two separate
sources of value creation by risk sharing are explored: **differences in risk attitudes** which might arise from predisposition or different levels of wealth, and **differences in diversification**. The result of this Section highlights the parties’ level of risk tolerance (both absolute and relative to each other), the specific quality of the risk involved as determined by its correlation with existing risk exposure and the level of project risk as relevant factors of optimal risk sharing. If a risk-neutral party is involved it carries all the risk. The sharing rule is independent in each state of the probability assigned to that state. Finally, the risky part of each party’s compensation is proportional to its own risk tolerance divided by the overall risk tolerance of society, if the parties’ preferences over lotteries exhibit constant risk aversion (which can be assumed locally).

In Section (IV2.4), a control theory model is set up to determine the optimal sharing rule. It is possible to reprove some of the results of earlier sections. If effort is contractible, flat fee contracts will be used. If it is not contractible it will depend on output. However, contrary to earlier results, it is also possible to shed some light on the mechanics of the optimal sharing rule. Compensation depends on output through the likelihood ratio. There are two implications: The optimal sharing rule can be understood quite intuitively as rewarding the agent if the signal makes it likely that high effort was chosen, but it also explains why little general constraints can be derived for the shape of the optimal sharing rule: It is very sensitive to specifications of utility functions and distributional assumptions. Although the optimal contract can only be derived to be linear under awkwardly improbable assumptions, there are a number of reasons explaining the practical prominence of such contracts. First, there is the transaction cost argument. Setting up complex contracts is just too expensive. Second, linear contracts are argued to be relatively robust for a large number of settings. But also non-distributional assumptions like the number of different options available to the agent affects the optimal incentive scheme. So, paradoxically, there are reasons to believe that adding complexity makes contracts simpler. It is also shown that information is only valuable if it affects posterior assessment of the effort level chosen. It must therefore be related to effort choice, but it must also be impossible to perfectly infer the information - to the extent that it is relevant to this assessment - from information of variables already included into the contract.

A common assumption is that there is a **comparative cost advantage** of output monitoring compared to input monitoring. Why should this be the case? In order to implement input monitoring, the principal has to watch the agent while performing the required task. This causes **opportunity costs** to the principal. Still worse, if the principal does not know the production function of the agent he may well watch the agent while performing a task but will be **unable to interpret his actions** as to whether they are instrumental in achieving the required output.
These costs are amplified by the fact that usually the very motivation to hire an agent in the first place was that the principal either did not want or could not perform the task himself. So, either the principal has something else to do, which means that his opportunity costs are high, or the performance of the task requires specialized knowledge that the principal does not possess. The latter case does make it difficult for the principal to monitor the agent effectively. Alternatively, the principal could hire other qualified agents to do the monitoring for him, but then it may be difficult to prevent these monitoring agents from colluding with the operative agents. For all these reasons, input monitoring is likely to be very costly in many circumstances. On the other hand, output monitoring should be very easy. One only has to look to which extent the required result was achieved providing that it can be properly defined. So, if a client hires a consultant to perform a cost cutting project, it will be much easier for the client to evaluate how much cost was reduced than to interpret the wide variety of single measures the consultant takes to achieve his goal. Having clearly established the intuition for comparative cost advantage of output monitoring compared to input monitoring in a wide variety of situations, it may come as a surprise that input monitoring can theoretically infinitely approximate first best. This is because the agent, in his decision whether to cheat or not, will weigh the benefits of cheating (in the case of shirking reduced disutility of effort) against the expected value of punishment. Therefore, if harsh enough punishments are announced, the probability of detection and therefore the number and thoroughness of inspection can be infinitely reduced. In this case, there seems no rationale for output monitoring. If it is not cheaper, it will only provide an additional drawback: imperfect risk sharing. The traditional argument is that input monitoring always establishes the truth while output monitoring is prone to error. This is a crucial point, because the driving force behind imperfect risk sharing in output monitoring is the possibility of error in judgement and the subsequent punishment of the innocent. If it can be shown that output monitoring can achieve perfect accuracy or input monitoring is prone to error as well, this distinction breaks down. In fact, both can be shown to be plausible assumptions in some circumstances: Output monitoring will be perfectly accurate in the case of deterministic production functions but also if there is shifting support. Indeed the argument of Mirrlees on step-functions is in the same spirit. On the other hand, it seems implausible to assume that input monitoring will be able to prove cheating at 100%. There will always be judgement, inferences, circumstantial evidence. Whatever the process, there is a chance of error. Therefore, the above assumption needs to be relaxed to allow for error in input monitoring. These arguments appear construed, and in fact they are. In general, input monitoring will be more costly and output
monitoring more prone to error. Ignoring these arguments therefore seems to be a justified abstraction, but there still is some merit to taking them seriously. In brief, they say that regardless of whether one is looking at input monitoring or at output monitoring there are two relevant issues: Error in judgement and cost of monitoring, and that under some circumstances both schemes fare equally well or bad on these two dimensions. By acknowledging that there generally is a distinction between input monitoring and output monitoring, one is actually saying that these circumstances will rarely be present. Understanding why this is the case helps to identify other relevant situational variables which influence the problem of optimal contracting.

Chapter (IV3) discusses the role of the bankruptcy constraints and the role of error in the monitoring process. Section (IV3.1) shows that input monitoring can approximate first best even if it comes at a cost if there are no bankruptcy constraints and no possibility of error in judgement. If there is a bankruptcy constraint there will be welfare loss, because of direct monitoring cost and possibly efficiency wages or complete uncontractability (no trade). The phenomenon of efficiency wages can be understood by realizing that, for purposes of incentive, provision differentials in agent pay utility, and not absolute pay levels, are relevant. Therefore, in the absence of error in judgement, increasing agent risk averseness even helps to set up cheap incentive schemes. Yet, if error in judgement is permitted, problems of imperfect risk-sharing arise, requiring the simultaneous minimization of the cost arising from investment in the monitoring technology, the frequency of inspections and the risk-premium.

Just as input monitoring can have a problem of imperfect risk sharing due to pitfalls in the monitoring process, there are situations where output monitoring is perfectly accurate and can achieve first best beyond the obvious case of a deterministic production function. This will be the case for shifting support schemes, but Section (IV3.2) shows that such schemes are often unrealistic because of bankruptcy constraints (legal, moral, economic) and the problem of fine tuning. Another problem is moral hazard with respect to risk which arises in output monitoring schemes in the presence of bankruptcy constraints which make incentive schemes de facto asymmetrical, limiting the downside. The agent will have the incentive to choose very risky projects even if they have a lower expected value than alternative projects (this can be thought of as a call option).

Section (IV4.1) mentions direct transaction costs which arise for input monitoring and for output monitoring, though it will often be plausible that they are lower for output monitoring. However, there are also indirect transaction costs which arise from provisions which designed to lower direct transaction costs. The goal is to rationalize monitoring or to directly influence the agent's
disutility function. This leads to inefficiencies because production technology is prescribed from top to bottom in a way that is known to be inefficient. In addition, innovation from bottom to top is stifled. This is a distortive effect.

Section (IV.2) deals with distortion. Distortion arises if there is tension between what the principal wants and what the agent is rewarded for. Often it is not possible to eliminate this tension. What the principal wants just might not be contractible. As was shown in this subsection, distortion can be divided into two components: scaling and alignment. Scaling refers to the relative sensitivity of the two measures to changes in the drivers and alignment to the similarity of driver patterns. If a university rewards a scientist (by promotion, or resources) by the number of published articles during a certain period of time (driver), there will be a problem of alignment if the university cares about both quantity and quality. Indeed, the researcher would have the incentive to publish many of articles in low quality journals. If no ranking of journals to account for quality is available, rewards should not depend too much on the number of published articles. Now, considering different departments it could be that a typical researcher in, say, marketing has 5 times as many publications than a typical researcher in, say, mathematics. If the basis for the bonus is the number of published articles, a scaling argument suggests the bonus rate for marketing researchers to be one-fifth of the bonus rate for mathematicians. It is obvious that there is a conflict with the risk-incentive trade-off. This model suggests that the bonus rate should be high if the observed variable is relatively undisturbed. This will be the case for parameters close to the agent. These, however, will be the most distorted, suggesting that the bonus rate should be low. This is consistent with the observation that variable compensation is used more often for top managers than for middle managers. For top managers, undistorted incentives (the share price) happen to correspond with their direct responsibility for the whole company. For middle managers, however, incentives are either distorted if they depend on the individual performance of the department (failing to take into account that it is important to cooperate among departments for the benefit of the company as a whole), or too disturbed, as the share price depends on many factors beyond the reach of the middle manager.

The basic problem of contracting is to find the best contract parameters or the best mix of contract parameters. Multi-period models do not solve the contracting problem costlessly as is sometimes claimed, but rather add predictions as to which contract parameters should enter the contract given the situational setting. It could well be that parties have to decide whether they want

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to contract on an input or an output parameter. They predict that using the input variable will lead to considerable monitoring costs, distortion and enforcement problems. They will therefore consider output contracting, but as the agent is very risk-averse they will abandon this alternative and ultimately decide to contract on the input variable. In multi-period settings this might change, as is discussed in Section (IV5.2), because the law of large numbers filters out uncertainty, reducing the problem of imperfect risk sharing. Therefore, multi-period contracting could become an attractive alternative. The model also stipulates situational conditions where this will work. First of all, there has to be a possibility to conclude long-term contracts. Therefore, the business relationship must be such that similar projects will repeatedly arise. In this case, long-term contracts will still be less flexible than a sequence of short-term contracts. Therefore, the question is how important flexibility is or how predictable the future will be. Moreover, it will be more expensive to write long-term contracts compared to short-term contracts. Therefore, increased transaction cost becomes an issue. Finally, parties will value long-term contracts only if there are saving and borrowing constraints for the agent outside the primary principal-agent relationship. This might plausibly be the case for informational reasons. Therefore, if time is built into an explicit long-term contract it is possible to reduce the cost of incentives by reducing imperfect risk sharing of output-based contracts.

It has already been argued that contractibility presupposes the knowledge of the production function, observability and verifiability. An especially interesting case is discussed in Section (IV5.3) where parties can observe a performance measure that will not be verifiable by a third party such as a court. There are certain situations where a self-enforcing mechanism, also referred to as an implicit contract, exists, sustaining a contract based on such subjective performance measures. The fundamental reasoning for these mechanisms is that if one of the parties makes a promise, it must be able to commit to this promise. Otherwise, the promise is worthless and cannot create incentives. In other words, it must be clear that at the moment when the party will have to make good on its promise it must be in its interest to do so. Otherwise, it can hold up the other party. Thus, the centre of interest is the decision rule of the party. Consider, for instance, a situation where effort is observable but cannot be objectively verified. The principal cannot commit to paying a bonus contingent on effort because it will always be in his interest to renege later. So, maybe he commits on something else that constrains his future action space in such a way that it will be in his interest to make good on his promise. The tournament mechanism is a case in point. The principal facing many agents commits on the total amount of bonuses paid out. By taking away the option of saving money by reneging, he can also credibly commit to paying the bonus as promised if only an infinitesimal preference for honesty is
assumed. Also, long-term relationships are actually able to create circumstances in which parties find it easier to commit. The basic intuition is simple: If one party has experienced that the other party acted opportunistically, it will stop doing business with this party. However, if the other party values the ongoing trade relationship, it will, anticipating this decision, not let its business partner down in the first place. It was therefore argued that long-term relationships can in some circumstances support contracts that may otherwise not be feasible by reputation effects created between the parties. This is the case in a situation where one would like to contract on an input parameter but effort is not contractible as it cannot be objectively verified. Or, alternatively, one would like to contract on an output parameter but the principal cannot commit on promised bonus because it is not verifiable. In these situations a reputation effect can sustain these contracts if the trade relationship is valuable and likely to increase in value, the time horizon of the parties will not be short-term (low discount rate), the expected probability of the relationship ending is low, and the bargaining power of the agent is not too low. Moreover, the project’s visibility and the parties’ entrenchment in business circles also will play a role.

In Section (IV5.4) a situation is considered where the input parameter is not observable, but one can act as if it were contractible because of an implicit contract. This is because the agent’s “career” will depend on his performance track record. The market monitors past performance and only agents who achieve high performance levels will be promoted. So, they will exert effort in order to positively affect their career chances. In such a case one would not be forced to change to output contracting, which might be very expensive. If the discount rate is not zero, incentives will never be efficient in equilibrium. They will, however, be close to efficiency if the discount rate is low, job characteristics change considerably due to innovation, and the external factor in the output process is unimportant. In absolute terms, incentives will be also higher if the disutility of effort only rises slowly. A useful and intuitive concept in this context is the speed of updating. In fact, if the noise of the production function is low relative to the noise of the competence process, the speed of updating will be high, which in turn will make incentive increase, because the agent knows that higher effort will have an impact. If, however, talent will be expected to last forever, once it has been proven and potential signals to the contrary are very unreliable, one will stick to the prior belief. Therefore, if the discount rate is low and updating fast, there will be fairly high incentives to perform in equilibrium. The intuitive idea is that reputation is not worth so much, or high reputation has to be reproved very often. In this situation, career concerns solve the shirking problem. Conversely, if the discount rate is high (the agent does not care about the future) and updating is slow, there will be low incentives in the steady state. In some situations, incentives in disequilibrium will be very important. This is especially true in
cases where updating is slow and the frequency of observation is low (due to a long production process). Incentives will be initially high if the precision of the initial belief is thought to be higher in later periods. Incentives will initially be low if the precision of the initial belief is thought to be lower in later periods. The first case will be true in most cases. In the beginning, not much is known about the agent. Therefore, beliefs will be relatively imprecise. In a situation where noise of the production function is high relative to the noise of the competence process, which is equivalent to a situation where there is little change of job characteristics due to innovations, and observation of output is only a very bad signal due to an important external factor, it was said that updating will be slow and incentives will be much too low in equilibrium. If there is low initial precision it was also said that incentives will be much too high in the beginning. This situation will persist for quite some time as the speed of updating is low. Therefore, an agent working in an industry where job characteristics are not expected to change much and the external factor is very important, as might be expected for many service industries, will get very inefficient incentives from career concerns. Situations relevant for the viability of such an implicit contract will therefore be the agent’s time horizon, the role of the external factor in the production process, the amount of innovation in the relevant industry, the duration of projects, the precision of initial beliefs concerning the talent of the agent, the market’s capability to monitor the agent’s track record. The thesis that time can solve incentive problems costlessly cannot be generally upheld. It was shown that this will only be the case under very unrealistic circumstances like zero discount rate and infinite repetition. Yet, the important insight from Chapter (IV5) is that the conclusions from the one-period models are not necessarily valid in the multi-period settings. The optimal contract in the one-shot relationship will be different from the optimal contract in multi-period relationships.

1.2 Checklist

Given the complexity of contract theoretic models, it would be tempting to develop tools which allow the practitioner to apply the insights of contract theory without bothering too much about the underlying models. There are, however, two obstacles to such an approach that were already mentioned. First, no recipe or scorecard to solve for the optimal contract can be given as the final step of application requires qualitative judgement close to the specific problem. Second, even if it is possible to give a checklist of factors which are relevant to the contracting problem, it is probably difficult to apply the checklist without having understood at least the general thrust of the models they are derived from. In the following, such a checklist of all the identified relevant factors shall nevertheless be given.
1. **Production technology and monitoring technology**: The production and monitoring technologies determine the quality of the signal that can be used in output monitoring and input monitoring, respectively. It can be deterministic or stochastic (relevant for error in judgement but also for implicit contract based on learning process). If it is stochastic, it can be more or less so (project risk may be higher or lower). Furthermore, the production technology can be known or unknown to the parties. Besides output there may be other signals of effort. If there are many available signals it is important to understand how these signals depend on each other in order to decide which mix of signals should enter the optimal contract (there is no use to incur the cost to monitor signals which contain no additional information).

2. **Inefficiency of prescribed production technology**: The principal might prescribe a production technology which is easy to monitor but inefficient (the prescription itself stifles innovation from bottom to top).

3. **Direct monitoring cost**: Effort may be readily contractible at low cost. In other situations, it can only be contracted if a very high-cost input monitoring scheme is put in place. Not only input monitoring but also output monitoring may cause such extra transaction costs, though they will usually be lower: Required output has to be defined and provisions have to be made to record output thus defined, sometimes resulting in extra accounting expenses. Monitoring cost will depend on legal and technological possibilities.

4. Definition of what is wanted and of what is measured and the divergence between the two: If there is a divergence this may lead to distortion.

5. **Quality of the courts or other third party enforcement facilities**: The quality of the courts can be thought of as the extent of potential contract parameters, which the court can verify, and the cost of doing so.

6. **Risk attitudes of the agent and the principal**: Both can be risk-neutral and risk-averse. If both are risk-averse, it is relevant to know the relative strength of risk averseness of one party compared to the other, but also the combined absolute level of risk averseness of both parties.

7. **Properties of the two parties’ portfolios with respect to the project**: In particular, the relative strength of diversification effects within the respective portfolios if the project is added.
8. The shape of the function specifying disutility of labour and the possibilities to influence it.

9. **The assumptions about the projects outcome:** The shape of the optimal incentive scheme depends on the distributional assumptions. For some projects a normal distribution or a lognormal distribution will be adequate. For others, there will be e.g. just two outcomes. Still others will have a bimodal distribution (if either very high or very low outcome is likely). Generally speaking, any distribution of outcome can be imagined depending on the specific circumstances.

10. **The precision of assumptions made:** Some schemes may require very accurate information with respect to parties’ risk preferences and the distribution of the project’s outcome (e.g. step function schemes).

11. **The action space of the parties:** The question is whether all relevant actions available to the parties are captured in the model. If e.g. parties can observe their performance in the process of performance, the issue of path-dependency of incentives arises.

12. **The bankruptcy constraint:** The amount of loss that a party can absorb, which may be limited by legal, economic and moral constraints.

13. **The scope of cheating:** The absolute extent of the benefit that the agent can appropriate by using his informational advantage.

14. **Time horizon:** How long will the relationship last? Is there a definite ending? What is the probability of the relationship after each trade ending?

15. **The presence of borrowing constraints:** If there is a sequence of separable projects, imperfect risk-sharing can be solved by borrowing or by dissaving if outcome is low, and saving if outcome is high.

16. **Potential gains of trade:** The utility that can be created if the trade can take place.

17. **The patience of the parties:** Extent to which parties value present payments over future payments.

18. **Expected growth rate of the value of the relationship over time:** Is the relationship of the two parties expected to bring ever more utility to the parties each time it is repeated?
19. **The relative bargaining power of the parties**: How are gains of trade divided between the two parties?

20. **Innovation**: The extent to which job characteristics change and therefore the extent to which it is possible to infer from past to present and future talent. Innovation will arguably be lower for a lawyer than for a software developer.

21. **Precision of prior beliefs about capability**: The extent to which a party considers its prior assessment to be precise as opposed to ambiguous.

### 1.3 Outlook

Traditionally, economists specializing in contract theory advise government regulation authorities or appear as expert witnesses in competition lawsuits if they have a practical interest at all. This is surely a very important field of application. The objective to help individuals, companies and lawyers to design better contracts and organizations in the course of their business is relatively less prominent.

There is a reason for that. It was already discussed that it is questionable whether contract theory can actually provide any added value in this field. It could be argued that most of what it has to say is already known by lawyers and practitioners of other fields. The underlying argument is that contracts and institutions are the product of evolution and that they survived because they proved to be successful in the past. Especially in very stable settings, it is difficult to imagine that something which was used and tested in a myriad of situations for decades or even centuries (like basic contract types of civil law) should be fundamentally flawed. Claiming such a thing would indeed be an example of the most naïve form of rationalist hubris.

Still, it was argued that there is some scope for rational construction. The analytical approach provides the notions necessary to analyse complex phenomena. It helps to understand that what drives the success of established institutions, to classify and describe institutional phenomena and to teach and communicate about what happens in institutions; but beyond that it would not have much practical relevance in improving existing contracts. It may, however, be speculated that this argument does not apply in situations which are relatively new. Explicit knowledge is unimportant for doing routine business, but as soon as a new situation arises there is an advantage of turning implicit knowledge into
explicit knowledge. This facilitates thinking about these new challenges and making use of past experience by means of recombination⁴⁰¹.

Moreover, even if some features of the underlying problem of contracting like information asymmetry and uncertainty will always be the same, the pace of institutional change has arguably increased at a time of rapid technological innovation and an increasingly deregulated and globalized economy. Deregulation allows for new kinds of labour contracts, financial innovation and increased global competition increasing the action space of individual agents. The revolution in communication technology led to innovations in transactions (peer-to-peer online auctions, e-commerce). Global competition leads to an increased need for flexible and unorthodox transnational company alliances, but also cultural change affects contracting: The decline of moral institutions⁴⁰², but also the break-up of cosy national clubs where a renegade agent could easily be punished by social sanctioning changes the way contracts can be enforced.

On a fundamental level, it could be questioned if adaptation to change should not be left to decentralized spontaneous order. In a provocative way one could ask if the rationalist who was ousted as the great central planner should have a comeback as an advisor of individual agents. In other words, even if evolution did not have a chance, it may still be better to proceed by trial-and-error than by trying to save time by taking a short-cut using analytical models. This is not the view of the author. Still, it has to be acknowledged that economic contract theory still has to undergo much more of a market test. It is the ability of such theories to explain existing institutions and the extent to which it is possible to convince practitioners of the value and the additional insight provided by this approach which validates these modes much more than any econometric test. It is in this respect that contract theory still has a long way to go.

Of course, some examples were mentioned in the text. The predictions of contract theory seem to fit quite well with certain institutional phenomena. Yet, it is far from playing a role as a framework for setting up real world contracts. Economists routinely complain about the fact that lawyers apparently do not care about what economists have to say, but the experience of the author suggests otherwise. Lawyers and managers are very interested in the promise of contract

⁴⁰¹ This is actually the method of analysis-synthesis.
⁴⁰² If everybody believed in divine justice this would be a very elegant way to solve the problem of moral hazard. But it is hard to believe that this mechanism ever worked effectively. Historically, it seems that it worked to some extent to suppress the lower classes, but even at the time failed to convince the estate manager to be honest towards the absentee noble landowner.
theory but soon get frustrated when they see what they think is considerable analytical complication for relatively meagre results. This is partly to blame on the mathematical illiteracy of most lawyers, or conversely the unwillingness of economists to “translate” their findings into common language, but there are still other problems: As was already mentioned in the introduction, there is the obsession of many economists with modeling single effects rather than dealing with concrete problems. Some effects are certainly more important than others, but in order to decide which effect is important and what can safely be ignored, one must see the whole picture. While there may be some importance in modeling small effects, there is certainly an imbalance between modeling such effects and summarizing, describing and applying them. It was also argued in the methodological part that analytical models can very fruitfully be used to derive qualitative results but that casuistic work should be used for the applications, leaving out the intractible middle ground; but the wrong conclusion for an economist would be to stop when analysis ends and informed judgement begins. This could be justified by citing separation of labour, saying that the development of applications can be done by others. The truth is, however, that nobody is waiting on the other side of this unilaterally defined interface. The fact that applications require judgement and are no longer analytically tractable or can be accessed by econometrics does not mean that they are any less “scientific”. Indeed, they are a very important part of the argument.

The following research programme is therefore proposed: First the analytical contract theoretical models should be summarized and systematically arranged. This should be done in a way that makes it possible for readers to treat these models largely as black box models. Then, much of casuistic work should be done in order to apply the models, mindful of the act that this application is nothing mechanical but rather an integral part of problem solving. Such casuistic work would not only display best practice of problem solving and be a good way of communication and teaching, but also enrich the model by helping to understand phenomena which evolved over time. In addition, translation into specific contexts allows submitting contract theory to a market test which is arguably the best empirical corroboration available for contract theory. A particularly interesting avenue for future research would be not just to analyse codified civil law but also model-contract collections of law firms and organizational solutions within companies.
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