XXX

Coordinator Graduate Research

Department of Management Sciences

XXX, Islamabad

Subject: **Research Report Evaluation of XXX**

I feel honored to serve in the role of an external examiner of the MS thesis entitled, “*Government Ownership Premium and Asset Pricing*” by XXX. I have examined the thesis and enjoyed it reading. Following are my concerns, which are duly categorized as major, moderate and minor.

## Major Concerns

**1.1 Introduction of the thesis**

1. The Introduction includes elaborate discussion on agency theory, but it fails to establish an argument supporting the existence of state-ownership premium (or a premium that will accrue because of agency relationship)
2. Similarly, there is discussion on ‘ Institutional Theory’ but the thesis fails to link it with the discussion on asset pricing and state-ownership premium. Readers will wonder as to why such elaborate discussions on these theories are there in the Introduction of the Thesis with no (or at least not visible from the write-up) relevance to the main topic.

**1.3 Literature review**

The main focus of the thesis is on the state-ownership premium. However, there is no literature related to this premium. Instead the thesis has assumed state ownership synonymous with institutional ownership and has started the literature section with the title ”Ownership concentration, institutional ownership literature”. The following points are a matter of concern in this regard:

1. There is no variable related to institutional ownership or concentration of ownership in the thesis, thus the inclusion of the above section is not justified.
2. Institutional ownership and government ownership cannot be considered one and the same thing due to several reasons. For example, the extant literature shows incentives and capabilities of the institutional investors to monitor the firms they have invested. No such literature can be traced in the case of government as an investor.
3. The literature review fails to develop arguments in support of state-ownership premium from the section 2.1 “Ownership concentration, institutional ownership literature”

Furthermore, other sections of the literature start discussion from the citations of empirical works, while ignoring the ground-breaking theories in the field. It is therefore suggested to:

1. include discussion on the state-ownership premium and build arguments in a manner that leads to the development of hypothesis on state-ownership premium.
2. organize the literature review such that theoretical underpinnings are discussed first and then present the empirical evidence in support or against each theory

**1.4 Incorrect conclusions/statements**

* 1. On page 49, second paragraph the thesis claims that “*big companies earn high rate of returns……these findings of the study are in line with Fama and French 1992, 1993, and 1996)…*This is a serious ignorance on the part of the writer as Fama and French consistently found that returns on the stocks of smaller firms were higher, not on the stocks of bigger firms.
  2. On page 52, the text reads “whereas, CAPM is a valid model” and a similar claim in there in the Abstract of the thesis. This is an incorrect conclusion because the thesis has not tested CAPM. The equation estimated includes GOP premium which is not part of CAPM equation
  3. In the conclusion section, page 69, the text reads “Fama and French found positive impact of size and value on stock returns” again shows the ignorance of the thesis writer as Fama and French found that size is negatively whereas B/M ratio is positively related to stock returns.
  4. Page 69, second paragraph claims that government stocks earn less returns than non-government. This claims is not supported both by the statistically insignificant difference in returns of the two groups of stocks as reported in Table 4.2 of the thesis, and by the number of years in which the government stock earned more as reported in Table 4.1 (see also comment 3.5 below)
  5. See last line of the Abstract and on page 71, last line of the first paragraph, the thesis claims “betas do not significantly forecast the future”. There is no basis for this claim as the thesis has not tested the predictive power of the included variables from lead-lag perspective. This claim would have been valid if the thesis had tested current betas to predict next period return. Instead the thesis has used both the dependent and independent variables concurrently.
  6. Recommendation in section 5.2, page 71 is not appropriate on grounds mentioned in section 1.4 (iv) and 3.5 of this evaluation report.

**1.5 Statistical Model**

The thesis in its abstract claims that it uses Fama and French (1992) methodology. Fama and French (1992) used Fama and Macbeth (1973) method of 3-steps regressions procedure. The present study has not used such 3-steps procedure. Second, the model of the present study is totally different from the fundamental equation of Fama and French which uses all the risk premiums in one regression. The present study has only written such equation on page 39, but has not actually applied it. Instead the present study has checked only two risk premiums at a time, without any justification. Hence it is suggested to incorporate strong rationale for the model used or apply the Fama and French method in its original form.

## 2. Moderate Concerns

**2.1 Statement of objectives**

**Objective 2.** The text reads, “*To prepare a model for asset pricing on the basis of ownership premium*”. Which seems too broad and out of the scope of the thesis. The thesis has considered only state ownership premium which does not include other ownership patterns, and hence a general ownership premium claim is invalid.

**Objective 3.** Objective no.3 is not clear as to what is meant by future (se all comment 1.4 (iv) above).

**2.2 Sample**

The study has used data of 48 firms whereas assets pricing studies traditionally have used very large data sets for making the results generalizable. There is no justification as to why the study uses only data of 48 firms whereas data for other companies are easily available. This might be one of the reason that results are insignificant for a number of variables in the thesis.

**3.3 The APT equation**

The study claims that it uses Fama and French (1992) version of model which is an extension of the CAPM. However, the thesis starts its methodology section with the equation of Arbitrage Pricing Theory (APT). The APT equation is then neither linked with the discussion nor used elsewhere in the analysis. One wonders why the APT equation is then included in the text.

**3.4 Two Pass regressions**

The thesis does not discuss the relevance of and justification for the use of two pass regression. The use of two pass regression should be supported from theory or econometric considerations.

**3.5 Descriptive Statistics**

The thesis claims that government stocks are less risky and hence earn lower returns as per the figures reported in Table 4.1. However, the table shows that 4 out of 8 years, returns on government stocks were higher. Similarly, standard deviation was higher in 5 out of 8 years for government stocks. Moreover, the statistical significance of the differences in the two stocks is null. This also shows that the state-ownership premium is non-existent. Thus claiming that ‘risk-return relationship holds in Pakistan’, (page 45, last paragraph’) is not appropriate.

Furthermore, comparing values in Table 4.2 and Table 4.4 show inconsistency in the value of GON returns. In Table 4.2, GON has average return of -0007 for the entire period whereas its value in Table 4.4 is 0.003, there seems to be some inconsistency in the calculations.

## 3. Minor concerns

**3.1 First person pronouns**

1. The thesis has used ‘we’ (e.g, second last line on page 35, and on page 69) which is not appropriate in research reports. Instead the thesis can use the term ‘this study’.

**3.2 Equations**

There is a large number of equations ranging over 5 pages. This is overkill. All such specific equations can be replaced with few generic forms.

**3.3 Format of the tables**

Tables do not have proper descriptions with regard to data used and symbol explanation. Also, the column headings reads “intercept” and below that are t-values, p-values, coefficients of the dependent variables. All this creates confusion. Furthermore, there are frequent appearance of ‘E-raised to power’ cases in the tables which are difficult to read. And the general format of the table is not standard as one can see in good reports or journal articles.

**3.4 References**

There are many instance in the report where only the author names are given, while ignoring the year ( see for example, Fama and French page 19; Alchian, Harold, page 13.) In other cases, the thesis cites both the first and last name of the authors for in-text citations.

**3.5 Missing references**

The following references exist in the text, but not in the bibliography.

* Ross (1973) p. 11 of the thesis
* Jensen and Meckling (1976) p.11
* Sharp 1964 p.11
* Lintner 1965 p.11
* Black 1972 p.11
* Alchian p.13
* Harold Demsetz p.13
* Bernouli (1738) p.15
* Berle and Means 1932 p.15
* Denis, Denis, and Sairy (1997) p.15
* Sharpe (1965) p.16
* Black p.17
* Scholes p.17
* Ross 1976 p.18

And many other referencing are missing.

In my opinion, if the above corrections are incorporated in the research report, they would add significant value to report which then would merit public defense and award of MS degree.

Regards

------------------------------------------

**DR. ATTAULLAH SHAH**

Assistant Professor

Institute of Management Sciences  
1-A, Sector E / 5, Phase – VII, Hayatabad, Peshawar  
**Personal Website:** [www.opendoors.pk](http://www.opendoors.pk)