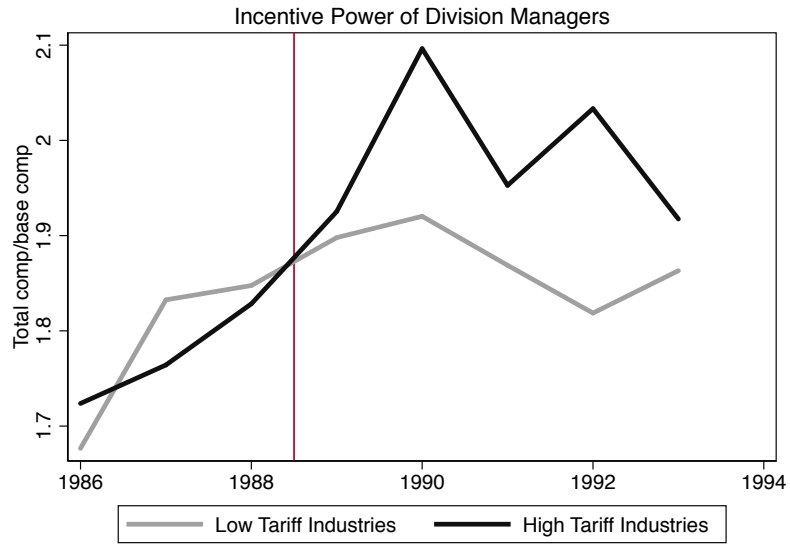


Online Appendix

Appendix Figures and Tables

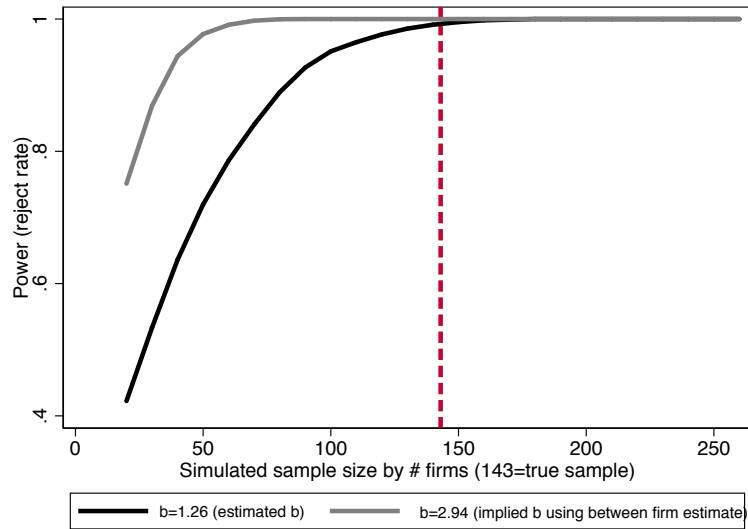
Figure A1: Incentive power before and after FTA



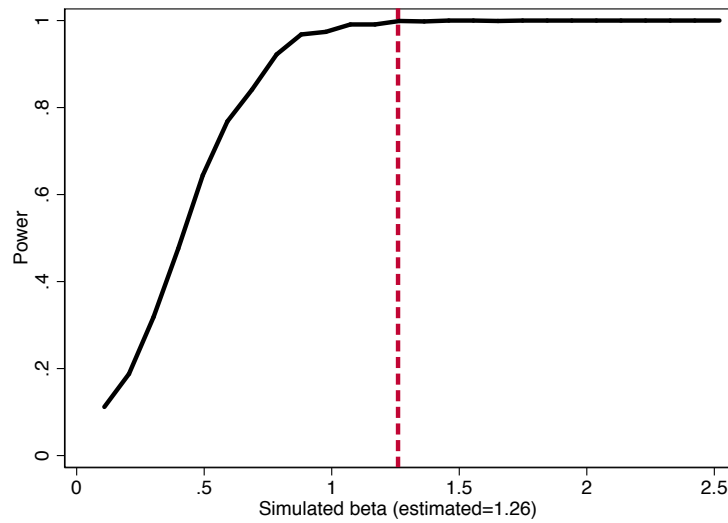
Notes: Low tariff industries defined as industries below median tariff level, high tariff industries defined as at or above median tariff level.

Figure A2: Power simulation results

Panel A: Power as a function of sample size



Panel B: Power as a function of magnitude of effect



Notes: This figure plots the results of simulated power tests for our within-firm pay gap finding corresponding to Table 3 (Col 4). Each data point represents the power based on 1000 simulated panel datasets.

Table A1: Effect of tariff reduction on incentive power

Dependent variable:	Total comp / Base Comp			
	(1)	(2)	(3)	(4)
Tariff *Post 1989	1.8619 (1.2845)	0.8957 (1.0746)	2.1105* (1.1012)	2.8721*** (0.9338)
Post 1989	0.0314 (0.0983)	0.0867 (0.0790)	0.0015 (0.0851)	-0.0711 (0.0801)
Tariff Pre-1989	1.0481 (1.3239)	1.4767 (1.1460)	.	.
Constant	1.6260*** (0.1038)	0.1227 (0.2627)	0.3202 (0.4399)	0.1891 (0.6775)
Observations	4399	4399	4399	4399
Controls	N	Y	N	Y
Fixed effects	None	None	Industry	Firm
Year fixed effects	Y	Y	Y	Y
Adjusted R-squared	0.059	0.278	0.400	0.601

Notes: See table 1 for description of variables. Errors block-bootstrapped by firm. Standard errors in (). ***, **, * refers to $p < 0.01$, 0.05 and 0.1, respectively.

Table A2: Degree of refocusing by quartile

Quartile of refocusing change	Mean
(Diversifying) 1	-0.20
2	-0.00
3	0.03
(Refocusing) 4	0.27

Table A3: Table 2 robustness**Panel A: Tables 2 and 3 replicated with year*tariff interactions**

Dependent variable:	Between-Firm Pay Gap	Within-Firm Pay Gap
	(1)	(2)
Tariff*1986	-0.9509 (1.5520)	-0.6530 (1.0090)
Tariff*1987	-0.5092 (1.2980)	-0.4477 (0.7248)
Tariff*1989	0.5521 (1.2950)	0.3352 (0.8282)
Tariff*1990	0.4885 (1.3717)	0.3613 (1.0459)
Tariff*1991	2.4912* (1.4328)	1.4356 (1.1905)
Tariff*1992	5.0136*** (1.7501)	1.4537 (1.1432)
Tariff*1993	3.5231** (1.5848)	1.4485 (1.0690)
1986	0.1821* (0.0997)	0.0195 (0.0477)
1987	0.0647 (0.0684)	0.0236 (0.0374)
1989	-0.1989** (0.0891)	-0.0400 (0.0601)
1990	-0.0519 (0.1058)	-0.0275 (0.0652)
1991	-0.1299 (0.1111)	-0.0912 (0.0667)
1992	-0.2193* (0.1150)	-0.1113 (0.0720)
1993	-0.1936* (0.1004)	-0.0879 (0.0706)
Tariff		
Constant	3.8433*** (0.4803)	0.9024* (0.4914)
Observations	784	4399
Controls	Y	Y
Fixed effects	Industry	Firm
Year fixed effects	Y	Y
Adjusted R-squared	0.544	0.279

Panel B: Table 2 replicated with tariff placebo test

Dependent variable:	Between-Firm Pay Gap		
	(1)	(2)	(3)
Tariff*Post 1989	-0.5221 (1.1082)	-0.5721 (1.2581)	0.2845 (1.0047)
Post 1989	0.0521 (0.1073)	0.0837 (0.1039)	-0.0246 (0.0848)
Tariff	-1.6424 (1.4750)	-3.0032* (1.6129)	.
Constant	1.0406*** (0.1087)	2.1458*** (0.3993)	3.7473*** (0.4824)
Observations	784	784	784
Controls	N	Y	Y
Fixed effects	None	None	Industry
Year fixed effects	Yes	Yes	Yes
Adjusted R-squared	0.197	0.272	0.537

Appendix Table A4: Vertical within-firm pay gap

Dependent variable:	Vertical DM-CEO Within-Firm Pay Gap			
	(1)	(2)	(3)	(4)
Tariff*Post 1989	5.6672 (7.0119)	8.8237 (7.7660)	4.9886 (7.1517)	5.3127 (6.2393)
Post 1989	-0.6012 (0.5778)	0.1181 (0.6315)	-0.0700 (0.5741)	-0.1389 (0.5573)
Tariff	-4.0065 (10.5291)	-7.7832 (8.4631)	.	.
Constant	6.8651*** (1.2299)	0.5028 (1.5668)	3.3992 (2.0854)	1.7895 (2.9729)
Observations	4399	4399	4399	4399
Controls	No	Yes	Yes	Yes
Fixed effects	None	None	Industry	Firm
Year fixed effects	Yes	Yes	Yes	Yes
Adjusted R-squared	0.020	0.374	0.498	0.593

Table A5: Table 3 replicated with low and high diversified firms

Dependent variable: Sample:	Within-Firm Pay Gap					
	Low Diversification			High Diversification		
	(1)	(2)	(3)	(4)	(5)	(6)
Tariff*Post 1989	2.2820* (1.2536)	2.4313 (1.5435)	2.9647* (1.7263)	-0.4472 (1.0860)	0.5463 (1.3607)	0.0268 (0.8694)
Post 1989	-0.2068** (0.0917)	-0.1571 (0.1081)	-0.1889 (0.1260)	0.1093 (0.0944)	0.0338 (0.1003)	0.0026 (0.0860)
Tariff	-1.9856 (1.4296)	.	.	0.3310 (0.9319)	.	.
Constant	1.6260*** (0.2275)	2.2009*** (0.3757)	1.8955*** (0.6439)	1.5141*** (0.2931)	1.4421** (0.5764)	-0.0742 (0.8329)
Observations	2204	2204	2204	2195	2195	2195
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	None	Industry	Firm	None	Industry	Firm
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.195	0.260	0.308	0.107	0.168	0.277

Table A6: Between-firm pay gap and Canadian export tariffs**Panel A: Export tariffs**

Dependent variable:	Between-Firm Pay Gap		
	(1)	(2)	(3)
Tariff*Post 1989	2.8892** (1.2250)	2.9693** (1.2175)	2.8385** (1.1725)
Post 1989	-0.0391 (0.1302)	-0.0064 (0.1217)	-0.1658* (0.0989)
Tariff	-3.1622* (1.7955)	-3.3027* (1.7893)	. .
Export tariff*Post 1989	-0.7321 (0.8415)	-0.7135 (0.8314)	0.0252 (0.7779)
Export tariff	1.2228 (1.3560)	0.8021 (1.3817)	
Constant	1.0539*** (0.1455)	2.0285*** (0.4045)	2.8731 (4.0285)
Observations	784	784	784
Controls	N	Y	Y
Fixed effects	None	None	Industry
Year fixed effects	Yes	Yes	Yes
Adjusted R-squared	0.194	0.257	0.541

Panel B: Residual export tariffs (portion of tariffs unexplained by import tariffs)

Dependent variable:	Between-Firm Pay Gap		
	(1)	(2)	(3)
Tariff*Post 1989	2.5249** (1.1415)	2.7040** (1.0993)	2.8192*** (1.0462)
Post 1989	-0.0664 (0.1237)	-0.0389 (0.1147)	-0.1552 (0.0946)
Tariff	-2.3027 (1.5050)	-2.6085* (1.4644)	. .
Export tariff (resid)*Post 1989	-0.7591 (1.1944)	-0.8073 (1.1869)	-0.2724 (1.0396)
Export tariff (resid)	2.4530 (1.5800)	2.1482 (1.6566)	
Constant	1.0525*** (0.1253)	2.0278*** (0.3868)	3.8962*** (0.5966)
Observations	784	784	784
Controls	N	Y	Y
Fixed effects	None	None	Industry
Year fixed effects	Yes	Yes	Yes
Adjusted R-squared	0.200	0.262	0.541

Table A7: Within-firm pay gap and Canadian export tariffs

Panel A: Export tariffs

Dependent variable:	Within-Firm Pay Gap			
	(1)	(2)	(3)	(4)
Tariff*Post 1989	1.4642 (1.0396)	1.2202 (1.0345)	1.4619 (1.1628)	1.5006 (0.9931)
Post 1989	-0.0782 (0.0762)	-0.0175 (0.0655)	-0.0481 (0.0685)	-0.0545 (0.0678)
Tariff	-1.5439 (1.2558)	-1.4139 (1.0741)	.	.
Export tariff * Post 1989	-0.6807 (0.5987)	-0.4321 (0.5750)	-0.4139 (0.5756)	-0.4980 (0.5935)
Export tariff	0.6393 (0.8472)	0.3365 (0.7300)	-1.5618 (37.3930)	
Constant	1.4805*** (0.0898)	1.5376*** (0.1627)	1.9193 (1.9285)	0.7715 (0.4727)
Observations	4399	4399	4399	4399
Controls	No	Yes	Yes	Yes
Fixed effects	None	None	Industry	Firm
Year fixed effects	No	No	No	No
Adjusted R-squared	0.005	0.113	0.177	0.279

Panel B: Residual export tariffs (portion of tariffs unexplained by import tariffs)

Dependent variable:	Within-Firm Pay Gap			
	(1)	(2)	(3)	(4)
Tariff*Post 1989	0.9623 (0.8614)	0.8965 (0.8946)	1.0933 (1.0121)	1.0314 (0.8507)
Post 1989	-0.0795 (0.0717)	-0.0148 (0.0612)	-0.0400 (0.0660)	-0.0442 (0.0639)
Tariff	-1.0303 (1.0512)	-1.1355 (0.9290)	.	.
Export tariff * Post 1989	-1.2550* (0.7427)	-0.9611 (0.6921)	-1.0095 (0.6790)	-1.2477* (0.6467)
Export tariff	1.4158 (1.0659)	0.8843 (0.9061)	-1.3268 (36.8977)	
Constant	1.4734*** (0.0822)	1.5286*** (0.1599)	1.8240*** (0.3274)	0.6782 (0.4691)
Observations	4399	4399	4399	4399
Controls	No	Yes	Yes	Yes
Fixed effects	None	None	Industry	Firm
Year fixed effects	No	No	No	No
Adjusted R-squared	0.007	0.114	0.178	0.280

Table A8: Results of first stage excess pay calculation

	(1)
Division sales (logged)	0.1887*** (0.0468)
Division sales growth	0.1345 (0.0893)
Sales per employee	0.1243** (0.0626)
Constant	10.9693*** (0.3785)
Observations	4399
Fixed effects	Industry
Adjusted R-squared	0.081

**Table A9: Differences in job-switching division manager characteristics
between high and low tariff industries**

Dependent variable:	High tariff industries			
	Pre-1989		Post-1989	
	Job switch next year (1)	No job switch next year (2)	Job switch next year (3)	No job switch next year (4)
Tenure (logged)	-0.5238** (0.2393)	-0.1667 (0.1042)	-0.0806 (0.1461)	-0.1357 (0.1001)
Division depth	-0.0861 (0.3452)	-0.1419 (0.2906)	-0.7553** (0.3429)	-0.5611** (0.2752)
Officer corp	-0.3678 (0.6610)	-0.5899 (0.5524)	-1.2646** (0.5940)	-0.9540** (0.4484)
Total compensation (logged)	-0.1005 (0.6206)	-0.0695 (0.4358)	0.2437 (0.5087)	0.1830 (0.3510)
Constant	3.1028 (7.8813)	1.6468 (5.5568)	-1.1857 (6.5912)	-1.0250 (4.5101)
Observations	172	1018	274	1532
Year Fixed Effects	Yes	Yes	Yes	Yes
Pseudo R2	0.056	0.017	0.081	0.047

Table A10: Within firm pay gap by restructuring subsamples

Dependent variable:	Within-Firm Pay Gap			
	(1)	(2)	(3)	(4)
Tariff*Post 1989	0.7520 (1.2191)	9.0416 (178.5919)	6.2835 (180.2941)	1.6263 (1.4416)
Post 1989	-0.0395 (0.0770)	-0.1169 (1.2869)	-0.0567 (14.3935)	-0.2643* (0.1371)
Tariff
Constant	0.7183 (0.8559)	3.0189 (3.7385)	0.8545 (4.0438)	0.9240 (0.8167)
Observations	1915	454	357	1673
Controls	Yes	Yes	Yes	Yes
Fixed effects	Firm	Firm	Firm	Firm
Year fixed effects	Yes	Yes	Yes	Yes
Adjusted R-squared	0.274	0.266	0.286	0.301

Column 1 includes firms that did not restructure post FTA. Column 2 include firms that added segments post FTA. Column 3 includes firms that exited segments post-FTA. Column 4 includes firms that both added and exited segments post FTA.

Table A11: Table 8 replicated for subset of non-restructuring firms only

Dependent variable:	Within-Firm Pay Gap			
	1 (Diversifying)	2	3	4 (Refocusing)
Quartile of refocusing change:	(1)	(2)	(3)	(4)
Tariff * Post 1989	2.5954 (11.8124)	-3.7535 (31.6499)	-5.8570 (600.7909)	-4.1318 (6.5976)
Tariff	0.1154 (1.2226)	0.7707 (4.1872)	0.0803 (47.0592)	0.1823 (0.3193)
Post 1989
Constant	2.0077 (3.1856)	1.5142 (6.3613)	3.6774 (7.6306)	0.4467 (2.6695)
Observations	507	405	361	386
Controls	Yes	Yes	Yes	Yes
Fixed effects	Firm	Firm	Firm	Firm
Year fixed effects	Yes	Yes	Yes	Yes
Adjusted R-squared	0.375	0.240	0.345	0.078

Column (1) analysis conducted at the firm-year level, Columns (2)-(4) at the division-year level.

Table A12: Change in residual pay sensitivity for below median managers

Dependent variable: Sample:	Average total compensation of above median managers		
	Below median managers		
	(1)	(2)	(3)
Excess pay*Tariff*Post 1989	4.0423** (1.5748)	4.1066** (1.7526)	1.9540 (1.8826)
Excess pay*Post 1989	-0.1878** (0.0787)	-0.1718** (0.0802)	-0.1164 (0.0776)
Tariff*Post 1989	-0.1688 (0.7362)	1.8682** (0.9247)	2.3308*** (0.8961)
Excess pay*Tariff	-1.0020 (1.5746)	-5.1620*** (1.9293)	-1.3218 (1.6370)
Excess pay	0.7926*** (0.0731)	0.7772*** (0.0815)	0.3985*** (0.0711)
Tariff	1.0159 (0.9143)	.	.
Post 1989	0.0635 (0.0568)	0.0276 (0.0612)	0.0209 (0.0668)
Constant	11.3441*** (0.2027)	11.8431*** (0.2979)	10.8892*** (0.5770)
Observations	2208	2208	2208
Controls	Yes	Yes	Yes
Fixed effects	None	Industry	Firm
Year fixed effects	Yes	Yes	Yes
Adjusted R-squared	0.760	0.812	0.909

Appendix B: Other explanations

In this appendix, we consider three additional potential explanations for the null within-firm change in pay inequality post-FTA, shown in Table 3.

First, could an increase in tournament incentives explain the consistency in pay inequality within firms? Under this explanation, firms induce effort among their employees via promise of future rewards once promoted (Main, O'Reilly and Wade, 1993; Conyon, Peck and Sadler, 2001; DeVaro, 2006). For this explanation to account for our results, firms would have to adjust their vertical rewards instead of the concurrent pay of employees. This appears not to be the case. As shown in Appendix Table A4, the vertical pay gap between the CEO and division managers within firms does not increase after the FTA.

Our second potential alternative is that pay inequality remains constant because there are high levels of cooperation and team production among division managers, which leads to similarity of pay, regardless of competitive environment. While harder to rule out definitively, we do not believe that this mechanism is driving our results. In Appendix Table A5, we split our sample by low and high levels of unrelated diversification. If team production were driving our results, we would expect to see our null result more dominant in less diversified firms, as these are the firms that are likelier to have higher levels of coordination and team production across division managers. However, we observe no statistical change in within-firm pay inequality for either subsample. Given this result, while we cannot rule this alternative out definitively, in our view it is unlikely to account for all of our results.

Our final potential alternative is that labor market frictions lead to the large changes in between-firm pay inequality and the no changes in within-firm pay inequality. For this to be true, we would have to believe one of two possibilities, either of which on its own might account for our observed patterns. For the first possibility, low labor market frictions permit easy reallocation of managers across firms so that firm-manager matching increase post-FTA. In this case, manager similarity thereby increases within firms post-FTA which results in more homogeneous pay. This greater similarity consequently offsets any increased pay differentials that may result from greater incentive power. For the second possibility, CUSFTA was a skills-biased shock, which led to certain managers becoming more valuable, but strong labor market frictions prevented their reallocation across firms. For our results to be explained by this

alternative, we would have to believe that those managers were pre-sorted to the optimal firms before the shock by luck and the shock affected the marginal value of all managers within given firms in the same way.

While either of these alternatives are possible, we do not believe they are driving our results. As we show in Table 7 in the main manuscript, we do not see large re-sorting post FTA, which would be a necessary condition in the first alternative. For the second alternative, we think the required assumptions of optimal pre-sorting of managers, combined with the homogenous effect of the shock on the marginal value of all managers, are both unrealistically strong assumptions to make in this context. This is particularly the case when we consider that this alternative would have also to explain the pay patterns reported in Table 4-6 across low and high social comparison subsamples (i.e., the pre-sorting would have to have taken place predominantly in the high social comparison subsamples, for reasons apart from social comparison, which we think is implausible). Given this discussion, we think it is unlikely that labor market frictions – either high or low – are the driving factor for our results.

Appendix C: Power Tests

For each simulation, we measure power as the rate of (correct) rejections of the null hypothesis that the treatment effect is zero in the simulated sample at a 5% confidence threshold. If the rejection rate from a given simulation is high, then we can conclude—for that (N, b) combination—that a null finding in an actual sample is likely a real effect, rather than a consequence of lack of power. For example, for a simulation using N=143 firms and b=1.26 (using Table 3, Column 2, the cross-sectional specification with the full set of controls, and the furthest of our three fully-controlled models to statistical significance, hence our most conservative test), a high rejection rate would indicate sufficient statistical power so that an actual effect of 1.26 would be detectable with our sample size. In that case, we can have confidence that a null finding reflects no actual change in horizontal pay inequality.

We run three separate simulations. In the first two simulations, we measure power as a function of N, our sample size, keeping b constant. In the first series, we set b=1.26, our estimated coefficient in our study. The goal of this first series is to measure our power assuming that the underlying actual treatment effect were the same as what we measure in our analysis. A finding of high power in this case would allow us to reject the alternative interpretation that our estimated treatment effect of 1.26 is real and we lack the power to establish statistical significance.

Our second series of simulations calculates the power for the less restrictive interpretation of our results that the within-firm treatment may be greater than zero, but is less than the across-firm treatment. For this series, we set b=2.94, the implied treatment effect if the magnitude of the within-firm treatment matched the estimated magnitude at the industry level, as estimated in Table 2, Column 2.¹ The goal of this series is to test whether we have the power to detect an estimated treatment effect of the magnitude that we detected in our industry-level analysis. A finding of high power in this case enables us to make the less restrictive claim that, at minimum, the change in within-firm inequality is lower in magnitude than the between-firm effect.

¹ We obtain this value of 2.94 as follows: the estimated coefficient in Table 2 Column 3 is 2.85, which amounts to a 7.7% increase in the between-firm pay gap of 1.48 at mean tariff level of 3.9 percent. An equivalent percent increase in the within-firm pay gap of 1.40 using the same mean tariff level would require a coefficient of 2.94.

In our third set of simulations, we measure power as a function of b , keeping N constant at 143 firms, our actual sample size. The goal of this analysis is to estimate the level of the treatment effect, b , above which we can say, with confidence, it is unlikely that lack of power can plausibly drive our results.

Our results are shown in Figure 3. Panel A shows the results of the first two series of simulations. The black data series represents the estimated power if the true underlying treatment effect is 1.26. The results of this test show our sample provides sufficient power to correctly reject null findings with 97% probability, well above the standard power threshold of 0.80.

The results of the second series of power tests are even stronger. The industry-level treatment effect of $b=2.94$ yields an estimated power of 100%; that is, in none of our 1000 simulations was a level of $b=2.94$ falsely rejected. Given this result, we can say with confidence that any within-firm effect is statistically likely to be lower than the between-firm effect.

The results of the third series of tests provide further evidence of this interpretation. Given our sample size, any treatment effect larger than 0.65 yields a power of 80% or greater. This result can be interpreted as providing us confidence that, at minimum, our estimated effect is above the effect size at the detectable power thresholds.

In sum, these tests provide confidence that our sample provides us the power to detect a true within-firm effect, should one exist in the data, and that, at minimum, any such effect is substantially smaller than the between-firm effect.

* Insert Figure A2 here *